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THE

PHILOSOPHICAL TRANSACTIONS

(From the Year 1743, *to the Year* 1750*)*

ABRIDGED,

AND

Diſpoſed under GENERAL HEADS.

The *Latin* PAPERS being tranſlated into *Engliſh*.

By *JOHN MARTYN*, F. R. S.

Profeſſor of BOTANY in the Univerſity of *Cambridge*.

VOLUME THE TENTH.

CONTAINING,

PART I. The MATHEMATICAL PAPERS.

PART II. The PHYSIOLOGICAL PAPERS.

PART III. The ANATOMICAL and MEDICAL PAPERS.

PART IV. The HISTORICAL and MISCELLANEOUS PAPERS.

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LONDON, 1756.

LONDON

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For the improving of

NATURAL KNOWLEDGE,

This Abridgment of the PHILOSOPHICAL TRANSACTIONS
is moſt humbly dedicated by

Chelſey, Jan. 1.
1756.

John Martyn.

A TABLE, shewing for what Months each Tranſaction was publiſhed.

VOL.	No.		
XLIII.	472.	*January*, *February*, *March*, and *April*,	1744.
	473.	*May*, and Part of *June*,	
	474.	*June*, *July*, *Auguſt*, *September*, *October*, *November*, *December*,	
	475.	*January*, *February*, *March*,	1745.
	476.	*April*, *May*, *June*, *July*,	
	477.	*Auguſt*, *September*, *October*, *November*, *December*,	
		Supplement,	
XLIV.	478.	*January*, *February*,	1746.
	479.	*March*, *April*,	
	480.	*May*, *June*,	
	481.	*October*, *November*, *December*,	
		Supplement,	
	482.	*January*, *February*,	1747.
	483.	*March*, *April*, *May*,	
	484.	*October*, *November*, *December*,	
		Appendix,	
		Supplement,	
XLV.	485.	*January*,	1748.
	486.	*February*, *March*,	
	487.	*April*, *May*, and Part of *June*,	
	488.	*June*,	
	489.	*October*, *November*,	
	490.	*December*,	
XLVI.	491.	*January*, *February*, *March*,	1749.
	492.	*April*, *May*, *June*,	
	493.	*October*, *November*, *December*,	
	494.	*January*, *February*, *March*, *April*,	1750.
	495.	*April*, *May*, *June*, *July*,	
	496.	*November*, *December*,	
	497.	Appendix,	

At the beginning of Vol. XLIII. *the following Advertiſement is inſerted.*

Where-ever it is ſaid, at the head of any paper, *here printed with Additions*, or with *Alterations*; It is to be underſtood, that the author of ſuch paper made ſuch *additions* or *alterations* himſelf; for none of them have been made by the editor. And where it is ſaid, *preſented* on ſuch a day; it implies that the paper was not read; the contents of it being of ſuch a nature as not to be underſtood at a bare reading; and that therefore the ſubject in general was only mentioned, or the title read.

This Advertiſement is alſo publiſhed at the beginning of Vol. XLVI.

This XLVI. Volume of the *Philoſophical Tranſactions* concludes thoſe publiſhed by the late *Cromwell Mortimer*, M. D. *Secretary* of the *Royal-Society*; the laſt number being printed off juſt before his death on the 7th of *January* 1752.

The following ERRATA *were corrected in the* Tranſactions at large, *too late to be rectified in the Abridgment.*

Vol. VIII. *Page* 199. *in the margin*, *for* Oct. 31. 1738. *read Oct.* 31. 1736.

This article therefore inſtead of being numbered XXVII. *ſhould be* XXV. 4.

Vol. IX. *Page* 461, *line* 29. *read*, mentions not only a *Pen of Iron*, but alſo *the Point of a Diamond.*

The READER *is deſired alſo to correct the following* ERRORS *in the preſent Volume.*

Page 301, *in the margin*, *for* Fig. 18. *read* Fig. 12.

Page 473, *in the table*, May 13. *for* 33, 23. (the height of the Barometer) *read* 30, 23. *and* July 24, *for* 22, 66. *read* 29, 66.

Page 481, *in the margin*, *for* July 15, *read July* 18.

Page 1034, *in the margin*, *for* Warren, *read* Warwick.

THE

THE

CONTENTS.

VOL. X. PART I.

The MATHEMATICAL PAPERS.

CHAP. I.

ALGEBRA, ARITHMETICK, FLUXIONS, GEOMETRY.

I. *OF the Fluents of* Multinomials, *and Series affected by* radical Signs, *which do not begin to converge till after the second Term; by Mr* Simpson, Page 1

II. *Concerning the easiest method for calculating the value of annuities upon lives, from tables of observations, by Mr* de Moivre, 6

III. *An account of a new invented arithmetical instrument called a* Shwan-pan, *or* Chinese Accompting Table; *by Mr* Smethurst, 13

IV. *A commodious disposition of equations for exhibiting the relations of* Goniometrical Lines, *by Mr* Jones, 14

V. *An Essay on* Quantity, *by Dr* Miles, 22

CHAP. II.

OPTICKS.

I. *OF the application of a* Micrometer *to a* Microscope, *by Prof.* Hollman 29

II. *Of* fallacious Vision through compound Microscopes, *by Mr* Gmelin, Page 30

CHAP. III.

ASTRONOMY.

I. *AN* apparent motion *in some of the* fixed Stars, *by Dr* Bradley, 32

II. *Declinations of some* Southern Stars *of the 1st and 2d magnitude, in* June 1738, *with the method of finding the time at sea in the night, by the aspect of the* Southern Cross, *by* M. de la Condamine, 53

III. *A new method of calculating* Eclipses *of the* Sun; *or any* Occultations *of the* Stars *by the* Moon; *by Prof.* Gerstten, 55

IV. 1. *The* Sun's Eclipse of July 14, 1748. *observed at* Marlborough-house, *with the 12 foot refracting Telescope fixed as a finder to the tube of the great 12 foot reflector: by Dr* Bevis, 68

2. ——— *by Mr* Day, *ibid.*

3. ——— *at the Observatory Royal at* Berlin, *by Mr* Greschow, *ibid.*

4. ——— *by the* Earl *of* Morton, *Mr* le Monnier, *and Mr* Short, Page 69
5. ——— *at* Madrid, *by* Don Ant. de Ulloa, 77
V. 1. Solar Eclipſe, *Jan.* 8, 1730, *N. S. at* Rome, *by Mr* Maire, 78
2. ——— *at the Obſervatory at* Berlin, *by* M. Griſchow, *jun. and* M. Kies, *ibid.*
VI. *Concerning the* Moon's *motion, by Mr* Dunthorne, *ibid.*
VII. *Of the* Acceleration *of the* Moon *by the ſame*, 84
VIII. 1. *The* Moon's Eclipſe *of* July 28, 1748, *obſerved at* Marlborough-houſe *by Dr* Bevis, 89
2. ——— *at* Madrid, *by* D. Ant. de Ulloa, *ibid.*
IX. 1. *An* Eclipſe *of the* Moon, *Dec.* 12, 1749, *obſerved at Mr* Graham's *in* Fleet-ſtreet, *by Dr* Bevis, *and Mr* Short, 90
2. ——— *at* Earith, *near* St Ives, *in* Huntingdonſhire, *by Mr* Elſtobb, 91
3. ——— *at* Rome, *by Mr* Maire, *ibid.*
X. 1. Eclipſe *of the* Moon, *June* 8, 1750, *obſerved in* Surry-ſtreet *in the* Strand, *by Mr* Catlin, *and Mr* Short, 92
2. ——— *at* Wittemberg, *by Prof.* Boſe, *ibid.*
XI. *Total* Eclipſe *of the* Moon, *obſerved* Dec. 2, 1750, *in the* Strand, *by Dr* Bevis, *and Mr* Short, 94
XII. *Occultation of* Jupiter *by the* Moon, *obſerved at* London, *by Dr* Bevis, 95
XIII. *The* Phaenomena *of* Venus, *repreſented in an* Orrery *by Mr* Ferguſon, *agreeable to the obſervations of* S. Bianchini, *ibid.*
XIV. *Obſervations concerning* Mercury, *by Dr* Bevis, 104
XV. *Tranſit of* Mercury *over the* Sun, Nov. 5, 1743, *at the Obſervatory at* Gieſen, *by Prof.* Gerſten, *ibid.*
XVI. *Occultation of* Cor Leonis *by the* Moon, Mar. 12, 1747, *in* Surry-ſtreet *in the* Strand, London, *by Dr* Bevis, Page 110
XVII. *Obſervations on the late* Comet, *made at* Sherborn *and* Oxford, *with the Elements for computing it's motions, by the Rev. Mr* Joſ. Betts, 111
XVIII. *The Path of the* Comet, *which appeared from the beginning of* March 1742, *to the beginning of* April, *from the obſervations made at the Obſervatory and College of the* Jeſuits *at* Pekin *in* China, 116
XIX. *The Paths of* Comets, *according to the Hypotheſis, which makes them deſcribe a Parabola about the* Sun, *by* Nicholas Struyck, 117
XX. *Various aſtronomical obſervations made in* Paragua, *in* S. America, 118
XXI. *Remarks upon the* Solar *and* Lunar *years, the* Golden Number, *the* Epact, *and a method of finding the time of* Eaſter, *as it is now obſerved in moſt parts of* Europe, *by the* E. *of* Macclesfield, 131
XXII. *Gradual Approach of the* Earth *to the* Sun, *by Prof.* Euler, 141
XXIII. *Contraction of the* Orbits *of the* Planets, *by the ſame*, *ibid.*
XXIV. *A new method of making a* Mural Quadrant, *which ſhall be free from many of the inconveniencies to which thoſe now in uſe are ſubject*, by Mr Gerſten, 143
XXV. Deſcription *and* Uſes *of an* Equatorial Teleſcope, *by Mr* Short, 154
XXVI. *Improvement of the* Celeſtial Globe, *by Mr* Ferguſon, 156
XXVII. *A Letter from the Widow of the late Mr* Senex *to the* Pref. *concerning the large Globes prepared by her huſband*, 158

CHAP.

CHAP. IV.

MECHANICKS, ACOUSTICKS.

I. THE Action *of* Springs, *by Dr* Jurin, 160
II. *An inquiry into the* Meaſure *of the Force of Bodies in* Motion : *with a propoſal of an* Experimentum Crucis *to decide the Controverſy about it* ; *by* the ſame, 174
III. Dynamical Principles, *or* Metaphyſical Principles *of* Mechanicks, *by* the ſame, 182
IV. 1. *A new* Mirror, *which burns at* 66 *feet diſtance, invented by* M. de Buffon, in a letter *from Mr* Turberville Needham, 194
2. *Concerning the ſame* Mirror *burning at* 150 *feet diſtance, in a letter from the* Marquis Nicolini, 195
3. *Abſtract of a letter from* M. Buffon, *concerning his Re-invention of* Archimedes's *burning* Specula, *ibid.*
V. *The motion of* Projectiles *near the Earth's Surface conſidered, independent of the properties of the Conic Sections, by Mr* Simpſon, 196
VI. 1. *Obſervations on the height to which* Rockets *aſcend, by Mr* Robins, 201
2. *An account of ſome experiments made by Mr* Robins, *Mr* Da Coſta, *and ſeveral other Gentlemen, in order to diſcover the height to which* Rockets *may be made to aſcend, and to what diſtance their* Light *may be ſeen* ; *by Mr* Ellicott, 202
VII. *Deſcription of a* Machine *to blow* Fire *by the fall of* Water, *by Mr* Stirling, 205
VIII. *Tables of* Specific Gravities, *extracted from various Authors, with ſome Obſervations upon the ſame by Dr* Davis, 206
IX. *A Letter from* Robert Southwell, *Eſq; to Mr* Henry Oldenburgh, *concerning ſome extraordinary* Ecchoes, Page 246

CHAP. V.

HYDRAULICKS.

I. A *Deſcription of a* Water-Wheel *for* Mills, *invented by Mr* Philip Williams, 247
II. *A Deſcription of a* Clepſydra, *or* Water-Clock ; *by the Hon.* Charles Hamilton, *Eſq;* 248

CHAP. VI.

GEOGRAPHY and NAVIGATION.

I. OBſervations *determining the* Longitude *of* Kingſton *in* Jamaica, *by Mr* Short, 250
II. *Extract of a Letter from Prof.* Euler *to the Rev. Mr* Wetſtein, *concerning the* Diſcoveries *of the* Ruſſians *on the* N. E Coaſt *of* Aſia, 251
III. *A Letter from* Arthur Dobbs, *Eſq; to the ſame, concerning the Diſtances between* Aſia *and* America, 252
IV. *A Letter from* F. Gaubil, *to Dr* Mortimer, *containing ſome account of the knowledge of* Geography *amongſt the* Chineſe, 255
V. *A Letter from Mr* John Robertſon, *to the* Preſ. *containing an Explanation of the late Dr* Halley's *Demonſtration of the Analogy of* Logarithmic Tangents *to the* Meridian Line, *or Sum of* Secants, 255
VI. *A* Machine *for* ſounding *the* Sea *at any depth, or in any part, by Major* Cook, 261

CHAP. VII.

MUSICK.

I. OF *the various* Genera *and* Species *of* Muſick *among the* Ancients, *with ſome* Obſervations *concerning their* Scale, *by Dr* Pepuſch, Page 261
II. *A* Machine *to write down* extempore Voluntaries, *or other pieces of* Muſick, *by the Rev. Mr* Creed. 266

VOL. X. PART II.

The PHYSIOLOGICAL PAPERS.

CHAP. I.

PHYSIOLOGY, METEOROLOGY, PNEUMATICKS.

I. 1. *ABſtract of what is contained in a book concerning* Electricity, *juſt publiſhed at* Leipſick, 1744, *by Prof.* Winkler, 269
2. *Of* Electricity, *by Prof.* Hollman, 271
3. *Of firing* Phoſphorus *by* Electricity, *by Dr* Miles, 272
4. *New Obſervations on* Electricity, *by Prof.* Winkler, 273
5. *Abſtract of a letter from M.* De Bozes, *to M.* De Maizeaux, 277
6. *A Letter from Dr* Miles *to the* Preſ. *containing Obſervations of* luminous Emanations *from* human Bodies, *and from* Brutes; *with ſome remarks on* Electricity, *ibid.*
7. *Experiments and Obſervations, tending to illuſtrate the nature and properties of* Electricity, *by Mr* Watſon, 279
8. *Further Experiments and Obſervations by the ſame,* 290
9. *A* Sequel *to the Experiments and Obſervations, in a letter to the* Royal-Society, *by the ſame,* 294
10. *Extracts of two letters from Dr* Miles *to Mr* Baker, *concerning the effects of a cane of black* Sealing-Wax, *and a Cane of* Brimſtone, *in* Electrical Experiments, 317
11. *Extracts of two letters from the ſame, containing ſeveral* Electrical Experiments, 319
12. *Part of a letter from Mr* Trembley *to the* Preſ. *concerning the Light cauſed by* Quickſilver *ſhaken in a Glaſs Tube, proceeding from* Electricity, 321
13. *Part of a letter from Dr* Miles *to Mr* Baker, *concerning* Electrical Fire, 322
14. *A letter from the ſame, concerning the Electricity of* Water, 323
15. *A letter from* ——— *to Mr* Ellicot, *of weighing the ſtrength of* Electrical Effluvia, 324
16. *Part of two letters from Dr* Miles *to Mr* Baker, *containing ſome* Electrical Obſervations, 325
17. *Extract of a letter from Prof.* Winkler, *concerning the effects of* Electricity *on himſelf and his wife,* 327
18. *A letter to Mr* Robins, *ſhewing that the* Electricity *of Glaſs diſturbs the* Mariner's Compaſs, *and alſo nice* Balances, 328
19. *Extract of a letter from Mr* Boſe *to Mr* Watſon, *on the* Electricity *of* Glaſs *that has been expoſed to ſtrong Fires,* 329
20. *Extract*

20. *Extract of a letter from Mr* Needham *to the* Pref. *concerning ſome new* Electrical Experiments *lately made at* Paris, *ibid.*
21. *Extract of a Memoir concerning the Communication of* Electricity; *by M.* le Monnier *the younger*, 336
22. *Obſervations upon ſo much of the preceding Article, as relates to the communicating the* Electric Virtue *to* Non-electrics, *by Mr* Watſon, 339
23. *Part of a letter from Mr* Browning *of* Briſtol, *to Mr* Baker, *concerning the Effect of* Electricity *on* Vegetables, 342
24. *Extract of a letter from Mr* Cooke *to Mr* Collinſon, *concerning the Property of* New Flannel *ſparkling in the dark*, 343
25. *Part of two letters from the ſame, concerning the ſparkling of* Flannel, *and the* Hair *of* Animals *in the dark, ibid.*
26. *Deſcription and Figures of an* Electrical Machine, *by Prof.* Winkler, 345
27. *A collection of the* Electrical Experiments *communicated to the* R. Soc. *by Mr* Watſon, 347
28. *Some farther Inquiries into the Nature and Properties of* Electricity, *by the ſame*, 368
29. *Part of a letter from the Abbé* Nollet *to Mr* Folkes, *concerning* Electricity, 382
30, 31. *An* Eſſay *towards diſcovering the* Laws *of* Electricity, *by Mr* Ellicott, 386
32. *A Continuation of the foregoing* Eſſay, 394
33. *A new diſcovery of the uſefulneſs of* Electricity *in* Medicine, *by Prof.* Winkler, 399
34. *A letter from Mr* Baker, *to the* Pref. *concerning ſeveral* Medical Experiments *of* Electricity, 404
35. *A letter from Mr* Roche *to the* Pref. *of a* Fuſtian Frock *being ſet on fire by* Electricity, 406
36. *Extract of a letter from Dr* Hales *to Mr* Hall, *concerning ſome* Electrical *Experiments*, *ibid.*
37. *An account of the Experiments made by ſome Gentlemen of the* R. S. *in order to meaſure the abſolute Velocity of* Electricity, 407
38. *A letter from Mr* Watſon, *to the* R. S. *declaring that he as well as many others have not been able to make* Odours *paſs through Glaſs, by means of* Electricity; *and giving a particular account of Prof.* Boſe *at* Wittembergh's *Experiment of* Beatification, *or cauſing a* Glory *to appear round a Man's Head by* Electricity, 410
39. *Extract of a letter from the Abbé* Nollet, *to the Duke of* Richmond, *accompanying an Examination of certain* Phaenomena *in* Electricity *publiſhed in* Italy, *by the ſame*, 414.

II. 1. *An Obſervation on the* Barometer, *by Prof.* Hollman, 428
2. *The Agreement of* Barometers, *with the changes of the Weather, by the ſame, ibid.*

III. 1. *A letter from Dr* Miles *to Mr* Baker, *concerning the difference of the degrees of* Cold *marked by a* Thermometer *kept within doors, or without in the open Air*, 433
2. *A diſcourſe concerning the Uſefulneſs of* Thermometers *in Chemical Experiments*; *and concerning the Principles on which the* Thermometers *now in uſe have been conſtructed*; *together with the deſcription and uſes of a* Metalline Thermometer, *newly invented by Dr* Mortimer, 435
3. *A letter from* Maurice Johnſon, *Eſq*; Pref. *of the* Gentleman's Society *at* Spalding, *to Dr* Jurin, *concerning a* Metalline Thermometer *in the* Muſeum *of that* Society, 446

4. *A*

4. *A Letter from Dr* Miles *to Mr* Folkes, *concerning* Thermometers, *and ſome obſervations of the* Weather, Page 447
IV. *The* Heat *of* boiling Water, *varies according to the* Weight *of the* Air; *by M.* de Monteſquieu, *in a letter to Mr* Folkes, 449
V. *Of ſudden* Freezing, *by Prof.* Hollman, 450
VI. *Deſcription of an improved* Hygroſcope, *from Mr* Arderon *to Mr* Baker, 453
VII. *Improvement of the* Weather-Cord, *in a letter from Mr* Arderon *to Mr* Baker, *ibid.*
VIII. *An* Hygrometer *made of a* Deal-Rod, *by the ſame*, 454
IX. Scheme *of a* Diary *of the* Weather; *together with Draughts and Deſcriptions of* Machines *ſubſervient thereunto, by Mr* Pickering, 456
X. *A letter from Dr* Lining *to Dr* Mortimer, *concerning the* Weather *in* South-Carolina; *with abſtracts of the tables of his* Meteorogical Obſervations *in* Charles Town, 465
XI. *A letter from Dr* Miles *to Mr* Baker, *concerning a very* cold Day, *and another a very* hot Day, *in* June *and* July 1749, *and of the near* Agreement *of* Thermometers *in* London *and at* Tooting, 471
XII. *Two letters from Dr* Miles *to Mr* Baker, *concerning the* Heat *of the* Weather *in* July *and* September 1750, 474
XIII. *Extract of a letter from Mr* Arderon *to Mr* Baker, *concerning the* hot Weather *in* July 1750, *ibid.*
XIV. *A letter from Dr* Miles *to the* Preſ. *concerning the* Storm *of* Thunder, *which happened* June 12, 1748, *at* Streatham *in* Surry, 475
XV. *Of the burning of the Steeple of* Danbury *in* Eſſex, *by* Lightning; *by* Smart Lethieullier, *Eſq*; Page 478
XVI. *The appearance of a fiery* Meteor, *as ſeen by Mr* Cradock, *ibid.*
XVII. *Concerning a fiery* Meteor, *ſeen in the Air by the Rev. Mr* Coſtard, 479
XVIII. *A remarkable* Meteor *ſeen in* Rutland, *which reſembled a* Water-Spout, *by* Tho. Barker, *Eſq*; *ibid.*
XIX. *An extraordinary* Fire-Ball, *burſting at Sea, by Mr* Chalmers, 480
XX. *An obſervation of an uncommon* Gleam *of* Light *proceeding from the* Sun, *by Mr* Collinſon, 481
XXI. *A* Halo, *or* Mock-Sun, *obſerved by Mr* Arderon, *ibid.*
XXII. *A Deſcription of an extraordinary* Rainbow, *obſerved July* 15, 1748, *by* Peter Daval, *Eſq*; *ibid.*
XXIII. *A luminous Arch, by Dr* Cowper, *Dean of* Durham, 482
XXIV. *An obſervation of an extraordinary* Lunar Circle, *and of two* Paraſelenes, *made at* Paris, *Oct.* 20. 1747, *by Mr* Greſchow, 483
XXV. 1. *An* Aurora Borealis *ſeen at* Chelſey, *by* J. Martyn, *ibid.*
2. ——— *at* Tooting, *by Dr* Miles, 484
3. ——— *at* Plymouth, *by Dr* Huxham, 485
4. *Another by the ſame*, *ibid.*
5. ——— *at* London, *by Mr* Baker, *ibid.*
6. ——— *at* Norwich, *by Mr* Arderon, *ibid.*
XXVI. Aurora Auſtralis *ſeen at* Chelſey *by* J. Martyn, 488
XXVII. *An* Earthquake *at* Taunton, *July* 1, 1747, *by Mr* Forſter, *ibid.*
XXVIII. 1. ——— *at* London, *Feb.* 8, 1749-50, *by Mr* Baker, 489
2. —— *by Dr* Knight, 490
3. —— *by* Jo. Freeman, *Eſq*; 491
4. ——

4. —— *at* Eltham, *by* Will. Fauquier, *Esq*; Page 491
5. —— *at* Tooting, *by Dr* Miles, 492
6. —— *at* Chelsey, *by* J. Martyn, 493
7. —— *at* London, *by Mr* Trembley, *ibid.*
8. —— *East from* London, *by* Smart Lethieullier, *Esq*; 494
9. —— *at* Plymouth, *by Mr* Barlow, *ibid.*
XXX. 1. —— *Mar.* 8, 1749-50, *at* London, *by the* President, 495
2. —— *by Dr* Birch, 496
3. —— *by Mr* Baker, *ibid.*
4. —— *at* Kensington, *by* M. Clare, *Esq*; 497
5. —— *at* London, *by Dr* Layard, *ibid.*
6. —— *by Mr* Pickering, 498
7. —— *by* Ja. Burrow, *Esq*; 499
8. —— *at* Tooting, *by Dr* Miles, 500
9. —— *at* Chelsey, *by* J. Martyn, 501
10. —— *at* London, *by* Michael Russel, *Esq*; *ibid.*
11. —— *by Dr* Parsons, 502
12. —— *at* Southwark, *by* Ja. Burrow, *Esq*; 503
13. —— *at* London, *by Dr* Mortimer, 504
14. —— *at* Tooting, *by Dr* Miles, *ibid.*
15. —— *by the Rev.* Wm. Cooper, *Dean of* Durham, 505
16. —— *The* President's *report of the account given him by* Tho. Burrat *of* Kensington, 506
17. —— *an account of part of a* Roof *of a* Pot-house *at* Lambeth *being flung down*; *communicated by Mr* Jackson *to Dr* Mortimer, *ibid.*
XXXI. 1. —— *Mar.* 18. 1749-50, *at* Portsmouth, *in a letter to Mr* Ellicot, Page 507
2. —— *in a letter to* Dan. Wray, *Esq*; *ibid.*
3. —— *in a letter from the Rev. Mr* Taylor *to Mr* Roderick, *ibid.*
4. —— *in a letter from Mr* Cooke *to Mr* Collinson, 508
5. —— *in a letter to Mr* Colebrooke, *ibid.*
6. —— *at* Hackney, *in a letter from Mr* Newcome, *to the* President, *ibid.*
7. —— *at* East-Sheen, *near* Richmond-Park *in* Surry, *by* James Burrow, *Esq*; 509
XXXII. —— *Mar.* 14. 1749-50, *by the Rev.* Stephen Hales, *D. D. ibid.*
XXXIII. —— *Mar.* 18, 1749-50, *at* Bridport, *by Mr* Downe, 510
XXXIV. 1. —— *Apr.* 2, 1750, *in a letter from* Chester, *communicated by* Rob. Paul, *Esq*; *F. R. S. ibid.*
2. —— *in* Flintshire, *in a letter from Mr* Pennant *to* Rich. Holford, *Esq*; 511
3. —— *in* Cheshire, *in a letter from Mr* Warburton, *to the* Pres. *serving to incloſe an extract of a letter from the Rev. Mr* Seddon, *of* Warrington *in* Lancashire, *to Mr* Philpot *of* Chester, *ibid.*
XXXV. —— *at* Winbourn *in* Dorsetshire, *May* 4, 1749, *and at* Taunton *in* Somersetshire, *July* 1, 1747, *by Mr* Baker, 512
XXXVI. —— *at* Norwich, *June* 7, 1750, *by Mr* Arderon, 513
XXXVII. 1. —— Aug. 23, 1750, *by* Maurice Johnson, *Esq*; *ibid.*
2. —— *by Dr* John Green, 514
XXXVIII. 1. —— *at* Newton *in* Northamptonshire, *Sept.* 30, 1750, *by* W. Folkes, *Esq*; *ibid.*
2. —— *near* Bury St. Edmund's *in* Suffolk,

Suffolk, *and at* Narborough *in* Leiceſterſhire, *by* James Burrow, *Eſq*; Page 514
3. ——— *in a letter from the Rev. Mr* Nixon, *to Prof.* Ward, 515
4. ——— *farther particulars in a letter from the Rev. Mr* Nixon, *to the* Preſ. 516
5. ——— *in a letter from Dr* Doddridge, *to Mr* Baker, 517
6. ——— *by the Steward, to the* Earl *of* Cardigan, 522
7. ——— *in a letter from Mr* Green *to Mr* Ayſcough, 523
8. ——— *in a letter from Dr* Miles *to Mr* Baker, *ibid.*
9. ——— *a letter from the Rev. Mr* Nixon *to the* Preſ. *ſerving to accompany a letter from Mr* Smith *to Mr* Nixon, 524
XXXIX. ——— *in* France, *by* M. de Reaumur, 526
XL. ——— *at* Smyrna, *by Dr* Mackenzie, *ibid.*
XLI. 1. *On the Cauſes of* Earthquakes, *by Dr* Stukely, *ibid.*
2. ——— *by the ſame*, 529
3. ——— *by Dr* Hales, 535
XLII. *The* Philoſophy *of* Earthquakes, *by Dr* Stukely, 541
XLIII. *Two letters from Mr* Wheeler *to the* Preſ. *concerning* a Rotatory Motion *of* Glaſs Tubes *about their* Axes, *when placed in a certain manner before the fire*, 551
XLIV. *Account of a Book intituled*, De quamplurimis Phoſphoris nunc primum detectis Commentarius Auctore Jac. Barthol. Beccario, *by Mr* Watſon, 555
XLV. *The* Lacrymæ Batavicæ, *or Glaſs Drops, the tempering of* Steel *and* Effervеſence *accounted for by the ſame Principle, by* M. le Cat, 560

CHAP. II.

HYDROLOGY.

I. *OF the* Fontaine du Salût *near* Bagneres *in* Gaſcony, *with other Obſervations, by* M. Secondat de Monteſquieu, 567
II. *Of the hot Springs at* Carlſbad, *by Dr* Mounſey, 569
III. *An Examination of the Strength of ſeveral of the* Principal purging Waters, *eſpecially of that of* Jeſſop's Well, *by Dr* Hales, 574
IV. *The ſtate of the* Tides *in* Orkney, *by Mr* Mackenzie, 577
V. *An* irregular Tide *in the River of* Forth; *by Mr* Wright, 583
VI. *A ſurprizing* Inundation *in the valley of* St John's *near* Keſwick *in* Cumberland, *by* John Lock, *Eſq*; 584
VII. *A* Burning Well, *by the Rev. Mr.* Maſon, 586

CHAP. III.

MINERALOGY.

I. *SOME account of the* ſinking *down of a piece of* Ground, *at* Horſeford *in* Norfolk, *by Mr* Arderon, 587
II. *A letter from Mr* Durant, *to the Hon.* Rob. Boyle, *Eſq*; *concerning a* Coal-Mine *taking* Fire *near* Newcaſtle; *of the* Blue Well; *and of a* ſubterraneous Cavern *in* Weredale, 588
III. 1. *Obſervations on the* Precipices *or* cliffs *on the N. E.* Sea-Coaſt *of* Norfolk, *by Mr* Arderon, 589
2. *An account of the* Strata *of* Shells, *and other* Foſſils, *found at* Cantly Whitehouſe *in* Norfolk, *by the ſame*, 590
3. *An account of other* Foſſils *found near* Hartford-

Hartford-bridge *in* Norfolk, *by the same*, Page 592
IV. *An account of large* ſubterraneous Caverns *in the chalk hills near* Norwich, *by Mr* Arderon, 593
V. *An account of the* Giant's Cauſeway *in* Ireland, *by Dr* Pococke, 594
VI. *A* moving moſs *in the neighbourhood of* Church-Town *in* Lancaſhire, *by the Rev. Mr* Richmond, 596
VII. *A* Foſſil Skeleton *of a Man, by* Roger Gale, *Eſq*; 597
VIII. *An account of ſome* human bones *incruſted with ſtone, now in the* Villa Ludoviſia *at* Rome, *by the Preſident*, 598
IX. *A letter from Mr* Baker *to the* Preſ. *concerning an extraordinary large* Foſſil Tooth *of an* Elephant, 599
X. *An Accouut of* 2 *extraordinary* Deer's horns, *found under ground in different parts of* Yorkſhire; *in a letter from Mr* Knowlton *to Mr* Cateſby, 601
XI. *An inquiry into the* Lapis Oſteocolla, *by Mr* Beurer *of* Nuremberg, 603
XII. *A letter from Mr* Hill *to the* Preſ. *concerning* Windſor Loam, 605
XIII. *The formation of* Pebbles, *by Mr* Arderon, 608
XIV. *Some obſervations upon* Gems *or* Precious Stones, *more particularly ſuch as the Ancients uſed to engrave upon, by* Robert Dingley, *Eſq*; 609
XV. *An account of certain perfect minute* Cryſtal Stones, *by Dr* Parſons, 612
XVI. *The ſpecific gravity of* Diamonds, *in a letter from Mr* Ellicot *to the* Preſ. *ibid.*
XVII. *An Extract, by* P. H. Zolman, *Eſq*; *of a* Philoſ. *Account of a new opinion concerning the origin of* Petrifactions *found in the Earth, which has hitherto been aſcribed to the univerſal Deluge*; *as contained in an* Italian *book, intitled* De Cruſtacei ed altri marini Corpi che ſe trovano ſu' Monti di *Anton. Lazaro Moro, Venice* 1740, *communicated together with ſeveral remarks*; *by Mr* Ehrhart, *in* High-Dutch, *at* Memmingen, 1745, 4*to*. Page 615
XVIII. *A letter from Mr* Simon, *of* Dublin, *to the* Preſ. *concerning the* Petrifactions *of* Lough-Neagh *in* Ireland, *to which is annexed a letter from the Biſhop of* Cloyne *to* Tho. Prior, *Eſq*; 616
XIX. *An account of a beautiful* Stalactites, *now in the* Muſeum *of the* R. S. *by Dr* Huxham, *in a letter to Dr* Mortimer, 627
XX. *A Diſſertation on the* Belemnites, *by Mr* Da Coſta, 627
XXI. *Remarks on the* Turquoiſe, *by Dr* Mortimer, 632
XXII. 1. *A deſcription of a curious* Echinites, *by Mr* Baker, 634
2. *Concerning two beautiful* Echinites, *by Mr* Da Coſta, 635
3. *Concerning a flat* ſpheroidal Stone, *having lines regularly croſſing it, by Mr* Plat, 638
4. *The deſcription and figures of a ſmall flat* ſpheroidal Stone, *having lines formed upon it, by Dr* Mortimer, *ibid.*
XXIII. *A beautiful* Nautilites, *ſhewn to the* R. S. *by the Rev. Dr* Lyttleton, 639
XXIV. *Conſiderations on two extraordinary* Belemnitæ; *by Mr* D. E. Baker, *ibid.*
XXV. *Some* Vertebræ *of* Ammonitæ *or* Cornua Ammonis, *by Mr* Baker, 641
XXVI. *An inquiry into the original ſtate and properties of* Spar, *and* Sparry Productions; *particularly the* Spars, *or* Cryſtals *found in the* Corniſh Mines, *called* Corniſh Diamonds, *by the Rev. Mr* William Borlace, 642
XXVII.

XXVII. 1. *Concerning a non-deſcript* petrified Inſect, *by the Rev. Dean* Lyttleton, Page 655
2. *A farther account by the ſame*, 656
XXVIII. *An account of a treatiſe, by* Wm. Brownrigg, *M. D. F. R. S. intituled*, The Art of making common Salt, as now practiſed in moſt parts of the World; with ſeveral Improvements propoſed in that Art, for the uſe of the *Britiſh* Dominions, *abſtracted by Mr* Watſon, 657
XXIX. *Of the* Salt-mines *near* Cracau, *by Dr* Mounſey, 668
XXX. *Of the* Foſſils *of* Bohemia, *by the ſame*, 670
XXXI. *Of ſome* Foſſils *found in* Ireland, *by Mr* Simon, *ibid.*
XXXII. *Concerning* Spelter, *and melting* Iron *with Pit-Coal, by Mr* Maſon, 671
XXXIII. 1. *Concerning a new* Semimetal *called* Platina, *by Mr* Watſon, *ibid.*
2. *Of the ſame*, *ibid.*
3. *Of the ſame*, 674
4. ——— *by Mr* Eman. Mendez da Coſta, 675
5. ——— *by Dr* Brownrigg, *ibid.*
XXXIV. *Concerning the* Everlaſting Fire *in* Perſia, *by Dr* Mounſey, 677

CHAP. IV.
MAGNETICKS.

I. MAgnetical Experiments *ſhewn before the* R. S. *by Dr* Knight, 678
II. 1. *A Collection of the* magnet. Exp. *communicated to the* R. S. *by Dr* Knight, 681
2. *An account of ſome* new Exp. *lately made with* artificial Magnets, *by the ſame*, 684
3. *Some further* Exp. *relating to the general* Phænomena *of* Magnetiſm, *by the ſame*, Page 685
III. *A letter from the ſame to the* Preſ. *concerning the* Poles *of* Magnets *being variouſly placed*, 688
IV. *A deſcription of a* Mariner's Compaſs *contrived by the ſame*, 689
V. *An account of ſome improvements of the* Mariner's Compaſs, *in order to render the* Card *and* Needle *propoſed by Dr* Knight, *of general uſe, by Mr* Smeaton, 693
VI. *A letter from Capt.* Waddell, *to Mr* Franks, *concerning the effects of* Lightning, *in deſtroying the* Polarity *of a* Mariner's Compaſs; *to which are ſubjoined ſome Remarks thereon, by Dr* Knight, 695
VII. *Obſervations made during the laſt* 3 *years, of the quantity of the* variation *of the* magnetic horizontal Needle *to the* W. *by Mr* Geo. Graham, 698

CHAP. V.
BOTANY, AGRICULTURE.

I. THE *eſtabliſhment of a new* Genus *of* Plants *called* Salvadora, *with it's deſcription, by Dr* Garcin, 699
II. *An* aquatic Plant *found at* Bagneres, *by* M. de Monteſquieu, 702
III. *A deſcription of the* Cyanus, foliis radicalibus, *&c. by Prof.* Haller, *ibid.*
IV. *Concerning a Plant but little known, and hitherto undeſcribed, by Mr* Watſon, 703
V. *An account of a new ſpecies of* Fungus, *by* John Martyn, 705
VI. *A deſcription of a curious* Sea Plant, *by Sir* Hans Sloane, 706
VII. *A*

VII. *A Catalogue of Plants presented to the* R. S. *by the Comp. of Apoth. of* London, *pursuant to the Direction of Sir* Hans Sloane, *Bart. by Mr* Joseph Miller, Page 707
VIII. *Some Account of the remains of* John Tradescant's *Garden at* Lambeth, *by Mr* Watson, 740
IX. *Concerning the* Cyprus *of the Ancients, by Dr* Garcin, 741
X. *A letter from Dr* Miles *to Mr* Baker, *concerning the green* Mould *on Fire-Wood: With some Observations on the minuteness of the* Seeds *of some Plants, by Mr* Baker, 748
XI. *Observations relating to* vegetable Seeds, *by Dr* Parsons, 750
XII. 1. *The Effect of the* Farina *of different Sorts of* Apple-Trees, *on the Fruit of a neighbouring Tree,* 751
2. *and* 3. ——— *by the same,* 752
XIII. 1. *A letter from Mr* Badcock *to Mr* Baker, *containing some microscopical Observations on the* Farina Fœcundans *of the* Holy-Oak, *and the* Passion-Flower, 753
2. *Farther Observations and Experiments on the* Passion-Flower, *and it's* Farina; *by the same,* 756
3. *Concerning the* Farina Fœcundans *of the* Yew-Tree; *by the same,* 757
XIV. *A letter from Mr* Hill *to the* President, *concerning the Manner of the seeding of* Mosses; *and in particular of the* Hypnum terrestre, trichoides, luteo-virens, vulgare, majus, capitulis erectis, 758
XV. *Concerning the Vegetation of* Melon-Seeds *33 Years old*; *by* Roger Gale, *Esq*; 761
XVI. *Microscopical Observations on the Root of* Ipecacuanha; *by Mr* Gmelin, *ibid.*
XVII. *The* Bark *prevents catching cold, by the Rev Dr* Salter, 762
XVIII. *Concerning some Persons being* poisoned *by eating boiled* Hemlock; *by Mr* Watson, Page 763
XIX. *Critical Observations concerning the* Oenanthe aquat. succo viroso crocante Lob. *by the same*; *occasioned by a letter from Mr* Howell, *giving an Account of the poisonous Effects of this Plant to some* French Prisoners *at* Pembroke, 765
XX. *An Account of the* poisonous Root *lately found mixed among the* Gentian, *by Dr* Brocklesby, 772
XXI. *Extract of an Essay upon the origin of* Amber, *by Dr* Fothergill, 774
XXII. *The Method of gathering* Manna *near* Naples, *in a letter from* Rob. Moore, *Esq*; *to Mr* Watson, 776
XXIII. *An Account of the Preparation and Uses of the various Kinds of* Pot-Ash, *by Dr* Mitchell, *ibid.*
XXIV. 1. *Concerning the* Propagation *and* Culture *of* Mushrooms, *by Mr* Pickering, 788
2. *Remarks on the preceding Paper, with* Observations *upon the* poisonous faculty *of some Sorts of* Fungi, *by Mr* Watson, 790
XXV. *Of covering Trees with* Ivy, *by* R. Gale, *Esq*; F. R. S. 793
XXVI. *Extract of a letter from Dr* Miles *to the* Pres. *relating to some Improvements which may be made in* Cyder *and* Perry, *ibid.*
XXVII. *The Substance of some Experiments of planting* Seeds *in* Moss, *by Mr* Bonnet, 795
XXVIII. *A letter from Mr* Pickering *to the* Pres. *concerning the manuring of Land with* Fossil Shells, 796

VOL. X. PART III.

CONTAINING THE

ANATOMICAL and MEDICAL PAPERS.

CHAP. I.

ZOOLOGY, and the ANATOMY of ANIMALS.

I. *Concerning the minute* Eels *in* Paſte *being viviparous, by Mr* James Sherwood, Page 799
II. *A letter from Mr* Baker *to the* Preſ. *concerning a new diſcovered* Sea-Inſect, *which he calls the* Eye-Sucker, 800
III. 1. *Obſervations upon ſeveral newly diſcovered* Species *of* freſh-water Polypi, *by Mr* Trembley, 801
2. ——— *continued by the ſame*, 807
IV. *A Suppoſition how the* white Matter *is produced, which floats about the Air in Autumn, by Mr* Arderon, 820
V. *A letter from Mr* Baker *to the* Preſ. *concerning the* Grubs *deſtroying the Graſs in* Norfolk, *ibid.*
VI. *A letter from the Earl of* Orrery *to the* Preſ. *incloſing an Account of the* Cornel Caterpillar, *in a letter from the Rev. Mr* Skelton, *to his Lordſhip*, 824
VII. *An Abſtract of Mr* Bonnet's *Memoir concerning* Caterpillars, *by Mr* Trembley, 831
VIII. *An Abſtract of Mr* Gould's *Account of* Engliſh Ants, *by the Rev. Dr* Miles, 833
IX. *An Account of the* Locuſts, *which did vaſt damage in* Walachia, *&c. by a Gentleman who lives in* Tranſilvania, Page 840
X. 1. *Some Obſervations on a ſort of* Libella *or* Ephemeron, *by Mr* Collinſon, 843
2. *Obſervations on the* Dragon-Fly *of* Penſilvania, *collected from Mr* Bartram's *letters, by Mr* Collinſon, 845
3. *A further Account by the ſame*, 846
XI. 1. *An Account of ſome very curious* Waſps Neſts *made of Clay in* Penſilvania, *by the ſame*, 847
2. *A Deſcription of the great* black Waſp *from* Penſilvania, *by the ſame*, 848
XII. *A letter from* Arthur Dobbs, *Eſq*; *to* Cha. Stanhope, *Eſq*; *concerning* Bees, *and their method of gathering* Wax *and* Honey, 849
XIII. *Extract of a letter from* Bombay, *communicated by* Francis Wollaſton, *Eſq*; *of a* Porcupine *ſwallowed by a* Snake, 855
XIV. *A letter from Mr* Breintal *to Mr* Collinſon, *containing an Account of what he felt after being bit by a* Rattle-Snake, 856
XV. *A letter from Mr* D. E. Baker *to the* Preſ. *concerning the Property of* Water-Efts *in ſlipping off their* Skins *as* Serpents *do*, 857
XVI. *Extract of a letter from Dr* Bartram *to Mr* Collinſon, *containing ſome Obſervations concerning the* Salt-Marſh-Muſcle, *the* Oyſter-Banks, *and the* Freſh-

Freſh-Water-Muſcle *of* Penſilvania, Page 860
XVII. *Some Obſervations on the hardneſs of* Shells, *and on the Food of the* Soal-Fiſh, *by Mr* Collinſon, 861
XVIII. *Obſervations upon certain* Shell-Fiſh *lodged in a large Stone brought from* Mahon *Harbour by Mr* More, *by Dr* Parſons, 862
XIX. *Obſervations on the* Cancer-Major, *by Mr* Collinſon, 864
XX. *Some Account of the* Rana Piſcatrix, *by Dr* Parſons, 866
XXI. 1. *A letter from Mr* Arderon *to Mr* Baker, *on keeping* ſmall Fiſh *in* Glaſs Jars; *and of an eaſy Method of* catching Fiſh, 869
2. ——— *by the ſame*, 871
3. ——— *by the ſame*, 872
XXII. *Concerning the* perpendicular Aſcent *of* Eels, *by the ſame*, 874
XXIII. *The Figure of the* Moſtela Foſſilis, *communicated from Dr* Gronovius *to Mr* Collinſon, *ibid.*
XXIV. *Some Obſervations on the* Belluga Stone, *by Mr* Collinſon, *ibid.*
XXV. *A letter from Mr* Baker *to the* Pref. *concerning an extraordinary* Fiſh, *called in* Ruſſia, Quab; *and concerning the Stones called* Crab's Eyes, 876
XXVI. *The Deſcription of a* Fiſh *ſhewn to the* R. S. *by Mr* Ralph Bigland, *drawn up by Dr* Mortimer, 879
XXVII. 1. *Extract of a letter from Mr* Arderon *to Mr* Baker, *concerning the* Hearing *of* Fiſh, 880
2. *An Account of Mr* Klein's *Treatiſe upon the* Sounds *and* Hearing *of* Fiſhes, *by Dr* Brockelſby, 883
XXVIII. *Of* Birds *of* Paſſage, *by Mr* Cateſby, 886
XXIX. *Divers means for preſerving* dead Birds *from Corruption*, *by* M. de Reaumur, *tranſlated from the French by Mr* Zollman, 891
XXX. *An Account of a* Quadruped *brought from* Bengal, *by Dr* Parſons, Page 898
XXXI. *A Nat. Hiſtory of the* Mus Alpinus *or* Marmot, *by Mr* Klein, 900
XXXII. *Concerning the* natural Heat *of* Animals, *by Dr* Mortimer, *ibid.*
XXXIII. 1. *Concerning a large* Stone *found in the Stomach of a* Horſe, *in a letter from Mr* ——— *to Mr* Wollaſton, 904
2. *Of a very large* Stone *found in the* Colon *of a* Horſe; *and of ſeveral Stones taken from the* Inteſtines *of a* Mare; *with ſome Exp. and Obſ. by Dr* Bailey, 905
3. *Concerning a* Stone *taken out of the Bladder of a* Dog; *which being cut aſunder had a Piece of* Dog's Graſs *in it's center*; *in a letter from Mr* Fidge *to Dr* Mortimer, 909
XXXIV. *A letter from Sir* H. Sloane, *to Mr* Folkes, *containing Accounts of the pretended* Serpent-Stone, *called* Pietra de cobra de cabelos, *and of the* Pietra de mombazza, *or the* Rhinoceros Bezoar, *together with the Figure of a* Rhinoceros *with a* double Horn, 910
XXXV. *Account of a Horſe bit by a* mad Dog; *in a letter from Dr* Starr *to Dr* Huxham, 913
XXXVI. 1. *Some Account of the* Diſtemper *raging among the* Cow kind *in the Neighbourhood of* London, *together with ſome Remedies propoſed for their recovery*, *by Dr* Mortimer, 916
2. *Further Obſervations by the ſame*, 917
3. *A third Account*, *by the ſame*, 920
4. *An Appendix to the foregoing Paper*, *by the ſame*, 922
5. *A letter from Dr* Parſons *to the* Pref. *ſerving to introduce a Remark from Mr* Milner, *concerning the burying of the* Cows *with or without Lime*, 925

XXXVII. *Concerning the* Ruſſia Caſtor, *in a letter from Dr* Mounſey *to Mr* Baker, Page 925

CHAP. II.

STRUCTURE, EXTERNAL PARTS, and COMMON TEGUMENTS of the BODY.

I. AN *Eſſay upon the Cauſes of the different Colours of People in different Climates, by Dr* Mitchel, 926
II. *Concerning a* Plica Polonica; *in a letter from Mr* Ames *to Dr* Mortimer, 950

CHAP. III.

The HEAD.

I. A Schirrus *of the* Cerebellum, *by Prof.* Haller, 950
II. *An Account of a remarkable Cure performed on the* Eye *of a young Woman in* Scotland, *by Dr* Hope, 951
III. *Cure of a Wound in the* Cornea, *and a Laceration of the* Uvea *in the* Eye *of a* Woman; *in a letter from Dr* Aery *to Dr* Mortimer, 954
IV. *Concerning a large Piece of a* Lath *being thruſt into a Man's* Eye; *who recovered of it, in a letter from Mr* Haſſel *to Mr* Daval, 955
V. *A Phyſiological Account of the Caſe of* Margaret Cutting, *who ſpeaks diſtinctly, though ſhe has loſt the* Apex *and* Body *of her* Tongue, *by Dr* Parſons, *ibid.*
VI. *The Caſe of* Henry Axford, *who recovered the Uſe of his Togue, after having been ſome Years dumb, by means of a frightful Dream, by Dr* Squire, Page 958
VII. *An Account of a very learned Divine, who was born with two Tongues; communicated by Dr* Mortimer, 959
VIII. *An Account of a Clergyman's Lady, who had a Stone under her* Tongue, *by* W. Freeman, *Eſq*; *ibid.*

CHAP. IV.

The NECK and THORAX.

I. AN *Account of the* Morbus Strangulatorius, *by Dr* Starr, 960
II. *A gibbous* Sternum, *by M.* Hubert, 964
III. *Experiments relating to Reſpiration, by Prof.* Haller, 965
IV. *The Caſe of a Lad who was ſhot through the Lungs; by Mr* Peters, *and Dr* Hallet, 966
V. *Obſervations on a Caſe of recovering a Man dead in appearance, by diſtending the Lungs with Air; by Dr* Fothergill, 969

CHAP. V.

The ABDOMEN.

I. AN *extraordinary* Cyſtis *in the Liver full of Water; by Dr* Jernegan, 971
II. *Of the Diſſection of a human Body, in which the Gall-Bladder was wanting, by Dr* Huber, 972
III. *The Caſe of Mr* Smith; *the Coats of whoſe* Stomach *were changed into an almoſt cartilaginous Subſtance; by Mr* Sayer, 973
IV. *The Operation of* Lithotomy *on Women; by* M. le Cat, 974
V. *An*

V. *An extraordinary* Calculus, *by Dr* Huxham, Page 976
VI. *Remarks on the* Operation *of cutting for the* Stone, *by* M. le Cat, *ibid.*
VII. *A Propoſal to bring ſmall paſſable* Stones ſoon *and with* Eaſe *out of the Bladder, by the Rev. Dr* Hales, 990
VIII. *A remarkable Caſe of a Perſon cut for the* Stone *in the new Way, commonly called the* Lateral, *by* W. Cheſelden, *Eſq*; 991
IX. *The Effects of the* Lixivium Saponis, *taken inwardly by a Man aged* 75, *who had the* Stone; *and in whoſe Bladder, after his deceaſe, were found* 214 Stones, *from* W. Cheſelden, *Eſq*; 992
X. *An Obſervation of an Operation made by the* High Apparatus, *in* 1743, *by Dr* Le Cat, 995
XI. *Part of a letter from Mr* Howell *to Mr* Watſon, *concerning the extracting a large* Stone *by an Aperture in the* Urethra, 999
XII. *A letter from Mr* Lucas, *concerning the Relief he found in the* Stone, *from the Uſe of* Alicant Soap *and* Lime-Water, 1000
XIII. *The Figures of ſome very extraordinary* Calculous Concretions *formed in the* Kidney *of a Woman*; *by Mr* Charles Lucas, *at* Dublin, 1001
XIV. *Extract of a letter from Mr* Heath *to Mr* Daval, *incloſing a Propoſal for entirely removing the* only real Defect *in the* lateral Operation *for the* Stone, *by Mr* Mudge, 1002
XV. *Concerning a Boy who had a* Calculus *formed between the* Glans *and the* Præputium, *by the Rev. Mr* Rob. Clarke, 1004
XVI. *An Account of a very large human* Calculus, *by Dr* Heberden, 1005
XVII. *A* Shuttle-Spire *taken out of the* Bladder *of a Boy, in a letter from Mr* Arderon *to Mr* Baker, 1006
XVIII. *The Caſe of a* Tumour *growing on the Inſide of the* Bladder, *ſucceſsfully extirpated, by Mr* Warner, Page 1006
XIX. *One of the* Ureters *grown up, by Dr* Huxham, 1007
XX. *An Obſervation of a* Steatoma *of the Ovary, and of Hairs found therein, by Prof.* Haller, 1008
XXI. *An* extra-uterine Conception; *by Dr* Myddleton, 1010
XXII. *An abſtract of the remarkable Caſe and Cure of a Woman, from whom a* Fœtus *was extracted, that had been lodged* 13 *Years in one of the* Fallopian Tubes; *ſent from* Riga, *by Dr* Mounſey, 1012
XXIII. 1. *An Account of the Bones of a* Fœtus *coming away by the* Anus; *communicated by* John Still Winthrop, *Eſq*; 1015
2. ——— *in a letter from Mr* James Simon, *to the* Preſ. 1016
XXIV. *An Account of a* Child *being taken out of the* Abdomen, *after having lain there upwards of* 16 *Years, during which time the Woman had* 4 *Children, all born alive*; *by Dr* Myddleton, 1017
XXV. *Concerning the Bones of a* Fœtus *being diſcharged through an Ulcer near the Navel*; *by Mr* Drake, 1018
XXVI. *A* Child *born with an* extraordinary Tumour *near the* Anus, *containing ſome Rudiments of an* Embryo *in it*; *by Dr* Huxham, 1019
XXVII. *Concerning a* Polypus *at the* Heart, *and a* ſchirrous Tumour *of the* Uterus; *by Dr* Templeman, 1021
XXVIII. *The* Extirpation *of an* Excreſcence *from the* Womb; *by Dr* Burton, 1023
XXIX. *An extraordinary* Impoſthume *in the* Stomach; *by Dr* Layard, *ibid.*
XXX. *Of an* Iliac Paſſion, *occaſioned by an*

an Appendix *in the* Ilion; *by Mr* Amyand, Page 1026
XXXI. *A* Rupture *of the* Navel; *by Mr* Taube (Dove), 1027
XXXII. *An uncommon* Dropsy *from the want of a Kidney; and a Description of a large* Saccus *that contained the Water, by Mr* Glass, *ibid.*
XXXIII. 1. *An Improvement in the Practice of* Tapping, *where that Operation, instead of a Relief for Symptoms, becomes an* absolute Cure *for an* Ascites; *by Mr* Warwick, 1030
2. *A method of conveying* Liquors *into the* Abdomen, *during the Operation of* Tapping; *proposed by the Rev. Dr* Hales, 1034
3. *A letter from Mr* Warwick *to Mr* Machin, *containing further Accounts of the Success of injecting* medicated Liquors *into the* Abdomen, *in the Case of an* Ascites. *ibid.*

CHAP. VI.

The HUMOURS and GENERAL AFFECTIONS of the BODY.

I. *Concerning the Use of the* Peruvian Bark *in the* Small-Pox; *by Dr* Wall, 1035
II. 1. *The Case of a Lady, who was delivered of a Child, which had the* Small-Pox *appeared in a Day or two after it's Birth; by Dr* Mortimer, 1041
2. *Some Accounts of the* Fœtus in utero *being differently affected by the* Small-Pox; *by Mr* Watson, 1042
III. 1. *A letter from Dr* Wilmot *to the* Pres. *serving to inclose the two following Papers;*
1. *Of the extraordinary Effects of* Musk, *in convulsive Disorders; by Dr* Wall, Page 1044
2. *A letter from* Andr. Reid, *Esq; to Dr* Wilmot, *concerning the Effects of the* Tonquinese *Medicine,* 1051
2. *A remarkable Instance of the happy Effect of* Musk, *in a very dangerous Case; by Dr* Parsons, 1054
IV. *The Case of a Person bit by a* mad Dog, *communicated to the* Pres. *by Mr* Ranby, *from Dr* Peters, 1056
V. *A short History of the Disease, that put an End to the Life of* F. Bolognini, *extracted by Dr* Mortimer, *from an Epistle written by Dr* De Camilis, *to the Abbot* De Revillas, 1059
VI. *Two Observations of a diseased Conformation of the Body found on Dissection; by Prof.* Haller, 1062
VII. *Of a Child born with the* Jaundice, *&c.* 1063
VIII. *An Account of the* Vomito Prieto, *or* Black-Vomit *of* S. America, *by Don* Antonio de Ulloa, *ibid.*
IX. 1. *Extract of a letter from Mr* B—B———r, *containing an Account in Pounds and Ounces, of the surprising Quantities of* Food, *devoured by a Boy 12 Years old, in 6 successive Days, who laboured under a* canine Appetite, 1066
2. *Of the same, by Dr* Cookson, 1068
X. *An Extract by Mr* Rolli, *of an* Italian *Treatise, written by the Rev.* Joseph Bianchini, *upon the Death of the Countess* Cornelia Zangari & Bandi. *To which are added Accounts of the Death of* Jo. Hitchell, *who was burned to Death by* Lightning; *and of* Grace Pett *at* Ipswich, *whose Body was consumed to a Coal,* 1069
XI. *Of a cleaving of the* Diaphragm, *and of the Situation of some of the* Viscera *being altered, observed in the Body of a Female*

Female Child of 10 *Months, in a letter from Dr* Fothergill *to Dr* Mead, Page 1077
XII. *Of* 2 *Men of extraordinary* Bulk *and* Weight; *by Mr* Catesby, 1083
XIII. *Of one who had no ear to Musick, naturally singing several Tunes in a* Delirium, *in a letter from Dr* Doddridge *to Mr* Baker, 1084
XIV. *A letter from Dr* Le Cat *to Dr* Mortimer, *concerning the Cure of* dry Gangrenes; *together with a Description of a new invented Instrument for the* Extirpation *of* Tumours *out of the Reach of the* Surgeon's *Fingers, ibid.*
XV. *Of the Passages of the* Semen; *by Dr* Haller, 1091

CHAP. VII.

BONES, JOINTS, and MUSCLES.

I. *AN Observation of a* Spina bifida; *by Mr* Aylett, 1093
II. *Some Observations on the* Spina ventosa, *by Mr* Amyand, 1094
III. *An Observation of a Fracture of the* os humeri, *by the Power of the Muscles only; by the same,* 1103
IV. *An extraordinary Case of a* Fracture *of the Arm; communicated by Mr* Freke, 1108
V. *A letter from Mr* Layard *to Dr* Mortimer, *inclosing an Account of a Fracture of the* os ilium *and it's cure,* 1109
VI. *The Case of a* young Child, *born with all it's Bones displaced; communicated by Mr* Davis *to Dr* Heineken, 1110
VII. *An Account of the Death of Dr* Greene, *who died of a Hurt he received as he was riding; by Dr* Cameron, 1111
VIII. *The Case of* Nicholas Reeks, *who was born with his* Feet turned inwards, *which came to rights after being some time used to sit cross-legged, transmitted from* Wm. Milner, *Esq; to Sir* Peter Thompson, Page 1113
IX. *An Account of a* Bristle, *that was lodged in a Gentleman's Foot, and caused a violent Inflammation; in a letter from Mr* Arderon *to Mr* Baker, 1114
X. *The* Crounian *Lectures on* Muscular Motion; *by Dr* Parsons,
Lecture I. *ibid.*
Lecture II. *Containing the* Author's *Scheme of* Muscular Motion, 1133
Lecture III. 1144
XI. Human Physiognomy *explained in the* Crounian Lectures *on* Muscular Motion; *by the same,*
Lecture I. 1150
Lecture II. 1162
XII. *The* Croonean *Lectures on* Muscular Motion; *by Dr* Langrish,
Lecture I. 1181
Lecture II. 1189
Lecture III. 1197

CHAP. VIII.

MONSTERS.

I. *SOME Account of the* gigantic Boy *at* Willingham *near* Cambridge,
1. *Rev. Mr* Almond's *letter,* 1205
2. *A letter from* Mr Dawkes *to Dr* Mead, 1206
II. *Extract of a letter from Mr* Arderon *to Mr* Baker, *containing an Account of a* Dwarf; *together with a Comparison of his Dimensions with those of a Child under* 4 *Years old; by* David Erskine Baker, 1207

III. *Description of a* monstrous Fœtus, *without any Distinction of Sex, by Mr* Baster, Page 1208
IV. *An Account of a* preternatural Conjunction *of two* Female Children, *in a letter from Dr* Parsons *to the* President, 1209
V. *An Account of double* Fœtus's *of* Calves, *by M.* Le Cat, 1216
VI. *Concerning a* Wether *giving* suck *to a* Lamb; *and of a* monstrous Lamb; *in a letter from Dr* Dodderidge *to Mr* Baker, 1218

CHAP. IX.

Period of Human Life.

An Abstract of the Bills of Mortality *in* Bridge-Town *in* Barbadoes, *communicated by the Rev. Mr* Clark, Page 1219

CHAP. X.

Pharmacy and Chymistry.

I. *Observations on the* Manna Persicum, *by Dr* Fothergill, *ibid.*
II. *Concerning the* Indian Poison, *sent over by* Don Ant. de Ulloa, *and mentioned by M.* De la Condamine, (*in his Account of the River of the* Amazons *in* S. America) *in a letter from Dr* Brocklesby, *to the* Pres. 1223
III. *Concerning a new Contrivance of applying* Receivers *to* Retorts *in Distillation; in a letter from Dr* Langrish *to Dr* Hales, 1225
IV. *An easy Method of procuring the* volatile Acid *of* Sulphur; *by Mr* Seehl, 1226

VOL. X. PART IV.

CONTAINING THE

HISTORICAL and MISCELLANEOUS PAPERS.

CHAP. I.

History and Antiquities.

I. *Concerning the* Chinese Chronology *and* Astronomy, *in a letter from the Rev. Mr* Costard, *to the Rev. Dr* Shaw, 1231
II. *Some Account of a curious* Tripos *and* Inscription *found near* Turin, *serving to discover the true Situation of the ancient City of* Industria, *by* David Erskine Baker, 1240
III. 1. *Concerning the Situation of the ancient Town* Delgovicia, *in a letter from Mr* Knowlton *to Mr* Catesby, 1245
2. *A Dissertation on the same, by Dr* Burton, 1246
3. *An Appendix to the foregoing Paper, by Mr* Drake, 1252
IV. 1. *An Account of a Dissertation published by Dr* Weidler, *concerning the vulgar* Numeral Figures, *as also some Remarks upon an* Inscription *cut formerly in a Window belonging to the Parish Church of* Rumsey *in* Hampshire, *by Prof.* Ward, 1254
2. *A brief Inquiry into the reading of* 2 *Dates in* Arabian Figures, *cut upon Stones*

Stones which were found in Ireland, *by the ſame*, Page 1260
3. *A brief Account of an ancient Date in* Arabian *Figures, at* Shalford Farm *(adjoining to* Waſing*) in the Pariſh of* Brimpton, *near* Aldermarſton *in* Berkſhire, *by the ſame*, 1264
V. 1. *An Explication of a* Roman Inſcription *found not long ſince on a Stone at* Silcheſter *in* Hampſhire, *by the ſame*, *ibid.*
2. *A Deſcription of the Town of* Silcheſter, *in it's preſent State, by the ſame*, 1267
VI. *An ancient* Roman Inſcription *at* Rocheſter *in* Northumberland, *and 2 others at* Riſingham; *by Mr.* Hunter, 1272
VII. *The Inſcription upon a* Roman Altar *found near* Stanhope *in the Biſhoprick of* Durham, *by the Rev. Mr* Birch, *ibid.*
VIII. *A* Roman Inſcription *found at* Bath, *communicated by the Rev. Dr* Stukely, *ibid.*
IX. *Remarks upon an ancient* Roman Inſcription, *found in that Part of* Italy, *which formerly belonged to the* Sabines; *and now in the Poſſeſſion of Dr* Rawlinſon, *by Prof.* Ward, 1273
X. *An Attempt to explain an ancient* Greek Inſcription *ingraven upon a curious Bronze Cup with* 2 *Handles, and publiſhed with a Draught of the Cup, by Dr* Pocoke, *in his* Deſcription of the Eaſt, *by the ſame*, 1278
XI. *An Explanation of an* ancient Inſcription *diſcovered at* Rutcheſter, *the laſt Station in* England, *upon the* Roman Wall, *by Dr* Taylor, 1284
XII. *Concerning* 2 ancient Camps *in* Hampſhire, *in a letter from Mr* Wright *to Mr* Theobald, 1294
XIII. *An Account of the preſent Condition of the* Roman Camp *at* Caſtor *in* Norfolk, *with a Plan of it, in a letter from Mr* Arderon *to Mr* Baker, Page 1295
XIV. *An attempt by Prof.* Ward, *to explain ſome Remains of Antiquity lately found in* Hertfordſhire, *and communicated to the* R. S. *by* Will. Freeman, *Eſq*; 1298
XV. *Concerning the ancient* Bridewell *at* Norwich, *by Mr* Arderon, 1304
XVI. 1. *Extract of a letter dated at* Rome, *from Mr* Hoare, *a young Statuary, to his Brother Mr* Hoare, *an eminent Painter at* Bath, *giving a ſhort Account of ſome of the principal antique* Pictures *found in the Ruins of* Herculaneum, *communicated by the Rev. Mr* Birch, 1305
2. *Remarks on the principal* Paintings *found in the ſubterraneous City of* Herculaneum, *aud at preſent in the Poſſeſſion of the King of* Naples, *by* ——— Blondeau, *Eſq*; *communicated by Dr* Stack, 1307
XVII. *Account of a Bas Relief of* Mithras *found at* York; *explained by the Rev. Dr* Stukely, *and communicated by Mr* Drake, 1311
XVIII. *An Account of an* ancient Shrine, *formerly belonging to the Abbey of* Croyland; *by Dr* Stukely, 1313
XIX. *An Abſtract of a Diſcourſe intitled* Reflections on the Medals of *Peſcennius Niger*, and upon ſome Circumſtances *in the Hiſtory of* his Life; *written in French by Mr* Claude Gros de Boze, *and ſent by him to Dr* Mead, 1314
XX. *A Deſcription of ſome clay Moulds or Concaves of ancient* Roman Coins, *found in* Shropſhire, *by Mr* Baker, 1320
XXI. *A brief Account of a* Roman Teſſera, *by Prof.* Ward, 1321
XXII. *A letter from Mr* G. Stovin, *to his Son, concerning the Body of a* Woman, *and an* antique Shoe *found in a Moraſs in the* Iſle *of* Axholme *in* Lincolnſhire, 1326

CHAP.

CHAP. II.

VOYAGES and TRAVELS.

I. *CO*ncerning *the Iſland of* Zetland, *in a letter from Mr* Preſton *to Mr* Ames, Page 1328

II. *A letter from* Robert More, *Eſq*; *to the* Preſ. *containing ſeveral curious Remarks in his Travels through* Italy, 1331

III. Obſervations *and* Experiments *made in* Sibiria, *extracted from the Preface to the* Flora Sibirica, *ſive* Hiſt. Plant. Sibiriæ cum tabulis æri inciſis *Auct.* D. Gmelin, *by Dr* Fothergill, 1333

CHAP. III.

MISCELLANEOUS PAPERS.

I. *AN eaſy Method of procuring the true Impreſſion or Figure of* Medals, Coins, &c. *by Mr* Baker, 1339

II. 1. *Concerning the* Bologna Bottles; *in a letter from Dr* Bruni *to Mr* Baker, 1343

2. *An Account of ſome Experiments, lately made in* Holland, *upon the Fragility of* unnealed Glaſs Veſſels; *communicated to the* Preſ. *ibid.*

III. 1. *An Account of* Glaſſes *of a new Contrivance, for preſerving pieces of* Anatomy *or* Nat. Hiſt. *in Spirituous Liquors, by* M. le Cat, 1349

2. *Addition to the preceding Paper, by the ſame, ibid.*

IV. *A letter from Dr* Lining *to Dr* Jurin, *ſerving to accompany ſome Additions to his* Statical Experiments, 1350

V. *A State of the* Engliſh Weights *and* Meaſures *of* Capacity, *as they appear from the Laws, as well Ancient a Modern, with ſome Conſiderations thereon; being an Attempt to prove, that the preſent* Averdepois *Weight is the* legal *and* ancient Standard *for the Weights and Meaſures of this Kingdom, by Mr* Reynardſon, Page 1356

VI. *An Account of two* Chineſe Paper Money Bills, *by* F. Anth. Gaubil, 1364

VII. 1. *Some Experiments on Subſtances reſiſting* Putrefaction, *by Dr* Pringle, 1365

2. *The ſame continued,* 1369

3. *The ſame continued,* 1373

VIII. *A Remark on* F. Hardouin's *Amendment of a Paſſage in* Pliny, 1378

IX. 1. *The Elements of a* Short Hand, *by Mr* Jeake, 1380

2. *A letter from Mr* Byrom *to the* Preſ. *containing ſome Remarks on Mr* Jeake's *Plan for* Short-Hand, 1384

3. *A letter from the ſame to the* Preſ. *containing ſome Remarks on Mr* Lodwick's Alphabet, 1386

X. *A Propoſal for* warming *Rooms by the Steam of* boiling Water *conveyed in Pipes along the Walls: And a Method of preventing* Ships *from* leaking, *whoſe bottoms are eaten by the Worms; by Colonel* Cook, 1391

XI. 1. *A Propoſal for checking in ſome Degree, the* Progreſs *of* Fires; *by Dr* Hales, *ibid.*

2. *An Addition to the former, by Dr* Mortimer, 1392

XII. *The great Benefit of* Ventilators, *by Dr* Hales, *ibid.*

XIII. *Extract of a letter from Mr* Arderon *to Mr* Baker, *giving an Account of the Weavers* Alarm, *ibid.*

THE

THE

Philoſophical Tranſactions

ABRIDGED.

PART I.

CONTAINING THE

Mathematical PAPERS.

CHAP. I.

ALGEBRA, ARITHMETICK, FLUXIONS, and *GEOMETRY.*

I. ALTHO' the Application of infinite Series, and the Quadrature of the conic Sections, to the inverſe Method of Fluxions has exerciſed the Pens of the moſt able Mathematicians, and produced many curious and uſeful Diſcoveries; yet nothing has been hitherto given, that I know of, whereby the Fluents of radical Multinomials and Series, which do not begin to converge till after the ſecond Term, can be determined, ſo as to be of Uſe in the Solution of Problems: the common Method, by expanding the given Expreſſion, being altogether impracticable in this Caſe.

Of the Fluents of Multinomials, *and Series affected by* radical Signs, *which do not begin to converge till after the ſecond Term; in a Letter from* T. Simpſon *F. R. S. to* W. Jones *Eſq; V. P. R. S.*

This little Eſſay is not merely an abſtracted uſeleſs Speculation, but may be apply'd to good purpoſe in many difficult and important Inquiries into

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into Nature; whereof I have put down one or two Instances, and shall further observe here, that most of the lunar Equations, given by Sir *I. Newton*, are only such Approximations as may be exhibited by the first Term of a Series derived by the Method here delivered.

Proposition. *The Fluent of* $\overline{a + cz^n}^{m} \times \dot{z}^{pn-1}\dot{z}$ *being given (either in algebraic Terms, or from the Quadrature of the Conic Sections, &c.) it is proposed, by means thereof, to approximate the Fluent of* $\overline{a + cx^n + dx^{2n} + ex^{3n} + fx^{4n}\ \&c.}^{m} \times \dot{x}^{pn-1}\dot{x}$; *supposing the Series not to converge till after the second Term.*

Make $cz^n = cx^n + dx^{2n} + ex^{3n}$ &c. and let $\mathcal{Q}$ be the given Fluent of $\overline{a + cz^n}^{m} \times z^{pn-1}\dot{z}$, answering to any proposed Value of x: Moreover let $y = x^{pn}$, or $y^{\frac{1}{p}} = x^n$, and let this Value of x^n be substituted in the first Equation, and it will become $cz^n = cy^{\frac{1}{p}} + dy^{\frac{2}{p}} + ey^{\frac{3}{p}}$ &c. whereof the Root y being extracted, we shall (by making $R = -\frac{pd}{c}$, $S = \frac{pp+3}{2} \times \frac{d2}{c2} - \frac{pe}{c}$, $T = \frac{-pp+4p+5}{6} + \frac{d3}{c3} + \frac{pp+4}{1} \times \frac{de}{c2} - \frac{pf}{c}$ &c.) have $y\ (x^{pn}) = z^{pn} + Rz^{pn+n} + Sz^{pn+2n}$ &c.

whence we also obtain $x^{pn-1}\dot{x} = z^{pn-1}\dot{z} + \frac{p+1}{p} \times Rz^{pn+n-1}\dot{z} + \frac{p+2}{p} \times Sz^{pn+2n-1}\dot{z}$ &c.

Let this Value, with that of $cx^n + dx^{2n} + ex^{3n}$ &c. (above given) be now substituted in the proposed Fluxion, and it will become

$$\overline{a + cz^n}^{m} \times z^{pn-1}\dot{z} + \frac{p+1}{p} \times Rz^{pn+n-1}\dot{z} + \frac{p+2}{p} \times Sz^{pn+2n-1}\dot{z}\ \&c.$$

Moreover, let v denote the Place, or Distance, of any Term, of this Expression, from the first (exclusive) then the Term itself (drawn into the common Multiplicator) will be denoted by $\overline{a + cz^n}^{m} \times \frac{p+v}{p} \times Az^{pn+vn-1}\dot{z}$; and the Fluent thereof will be truly expressed by

$$\frac{p+1}{p+m+1} \times \frac{p+2}{p+m+2} \times \frac{p+3}{p+m+3} \times \cdots \frac{p+v}{p+m+v} \times \overline{\frac{a}{c}}^{v} \times A$$

$\times AQ + \frac{\overline{p+vA}}{P} \times \frac{\overline{a+cz^n}^{m+1} \times z^{pn-n}}{p+m+v \times nc}$ into $z^{vn} -$

$\frac{p+v-1}{p+m+v-1} \times \frac{az}{c}^{vn-n} + \frac{p+v-1.\ p+v-2}{p+v+m-1.\ p+v+m-2}$

$\times \frac{a^2 z}{c^2}^{vn-2n}$ *&c.* continued to as many Terms as there are Units in v. Wherein let v be expounded by 1, 2, 3 *&c.* ſucceſſively, and R, S, T, *&c.* by A reſpectively: By which means the Fluent of the whole Expreſſion will be obtained.

Becauſe the Fluent of the general Term, when the Multiplicator *Carol.* 1.
$\overline{a+cz}^{m+1}$ becomes $= 0$, is barely $= \frac{p+1}{p+m+1} \times \frac{p+2}{p+m+2}$

$\times \frac{p+3}{p+m+3} \times \ldots\ldots \frac{p+v}{p+m+v} \times \overline{\frac{a}{-c}}^{v} \times AQ$ the Fluent of the whole Expreſſion will, therefore, in this Caſe be truly defined by

$$\frac{Q \times 1 - \frac{p+1.\ Ra}{p+m+1.\ C} + \frac{p+1.\ p+2.\ S2a}{p+m+1.\ p+m+2.\ C^2} -}{+m+1.\ p+m+2.\ p+m+3.\ c^3} \frac{p+1.\ p+2.\ p+3.\ Ta^3}{}$$

&c. Where Q denotes the Fluent of $\overline{a+cz^n}^{m} \times z^{pn-1}\dot{z}$, when $z^n = \frac{a}{-c}$.

But, if $m+1$ and p be, each of them, the Half of an odd affir- *Carol.* 2.
mative Number, and P be taken to denote the Periphery of a Circle whoſe Diameter is Unity, and $-c$ be put $= b$, then the Value of Q $\left(\text{or the Fluent of } \overline{a-bz^n}^{m} \times z^{pn-1}\dot{z}, \text{ when } z^n = \frac{a}{-b}\right)$ will be

$$= \frac{a^{p+m} P}{nb^p} \times$$

$$\frac{1.\ 3.\ 5.\ 7\ \&c.\ (\text{to } p - \frac{1}{2} \text{ Factors}) \times 1.\ 3.\ 5.\ 7\ \&c.\ \text{to } (m + \frac{1}{2} \text{ Factors})}{2.\ 4.\ 6.\ 8.\ 10.\ 12\ \&c.\ (\text{to } p+m \text{ Factors})}$$

Therefore the *Whole*, required, Fluent, of $\overline{a-bx^n+dx^{2n}+ex^{3n}\ \&c.}^{m} \times x^{pn-1}\dot{x}$ is, in this Caſe, equal to the Product of that Expreſſion into the following Series, $1 + \frac{p+1.\ Ra}{p+m+1.\ b} + \frac{p+1.\ p+2.\ Sa^2}{p+m+1.\ p+m+2.\ b^2}$ *&c.* Wherein R is to be taken $= \frac{pd}{b}$, $S = \frac{p.\ p+3}{2} \times \frac{d^2}{b^2} + \frac{pe}{b}$, $T = \frac{p.\ p+4.\ p+5}{6} \times \frac{d^3}{b^3} + \frac{p.\ p+4}{1} \times \frac{de}{b^2} + \frac{pf}{b}$, *&c.* according to what is above ſpecified.

The Uſe of what has been deliver'd above will, in ſome meaſure, appear from the Solution of the two following Problems, which I ſhall ſubjoin as Examples thereof. The firſt is;

To find the Time of Oſcillation in the Arch of a Cycloid, in a Medium reſiſting according to the duplicate ratio of the Velocity.

Let A denote the whole Arch of the Semi-Cycloid, or the Length of the Pendulum, a the Arch deſcribed in the whole Deſcent, and x any variable Part thereof deſcribed from the Beginning of the Deſcent; and let the Denſity of the Medium be, every-where, as $\frac{1}{b}$: Then the Fluxion of the Time will be found $=$

$$\overline{a - 1 + \frac{2a}{b} \times \frac{x}{2} - \frac{2x^2}{2.3b} + \frac{4x^3}{2.3.4b^2} - \frac{8x^4}{2.3.4.5b^3} \&c.}^{-\frac{1}{2}} \times \overline{2A}^{\frac{1}{2}} \times x^{\frac{1}{2}} \dot{x} \text{ *}$$

: which being compared with $\overline{a - bx^n + dx^{2n} + ex^{3n} \&c.}^m \times x^{pn-1} \dot{x}$ we ſhall, in this Caſe, have $n = 1$, $m = -\frac{1}{2}$, $p = \frac{1}{2}$, $a = a$, $b = \overline{1 \times \frac{2a}{b}} \times \frac{1}{2}$, $\frac{d}{b} = \frac{2}{3b}$, $\frac{e}{b} = -\frac{1}{3b^2}$, $\frac{f}{b} = \frac{2}{15b^3}$ *&c.* Whence

vide Corol. 2.

$R = \frac{1}{3b}$, $S = \frac{2}{9b^2}$, $T = \frac{8}{45b^3}$ *&c.* Alſo $\frac{a^{p+m}P}{nb^p} \times \frac{1.3.5.7(p-\frac{1}{2})}{2.4.6.} \times \frac{1.3.5.7(m+\frac{1}{2})}{8.10.(p+m)} = \frac{P}{b^{\frac{1}{2}}}$, and $1 + \frac{p+1.Ra}{p+m+1.b} + \frac{p+1.}{p+m+1.} \frac{p+2.Sa^2}{p+m+2.b^2} \&c. = 1 + \frac{a}{2bh} + \frac{5a^2}{12b^2h^2} + \frac{7a^3}{18b^3h^3} \&c.$

Whence we have $\overline{2A}^{\frac{1}{2}} \times \frac{P}{b^{\frac{1}{2}}} \times \overline{1 + \frac{a}{2bh} + \frac{5a^2}{nb^2h^2}}$, *&c.* for the Time of one Vibration of the Pendulum; which, by ſubſtituting $\overline{1 + \frac{2a}{b}} \times \frac{1}{2}$ for its Equal b, *&c.* becomes $PA^{\frac{1}{2}} \times \overline{1 \text{ *} + \frac{a^2}{6h^2} - \frac{2a^3}{9h^3}} \&c.$ From which it appears, that the Effect of the Reſiſtance on the Time of Vibration, in ſmall Arches, is nearly in the duplicate Ratio of thoſe Arches.

Princip. Prop. 27. B. 2. Sir *I. Newton* has, indeed, given a very different Solution to this Problem. But as the Concluſion here brought out exactly agrees with what I have elſewhere given, by a different Method, I have great Reaſon to believe I have no where fallen into an Error.

The

* The Inveſtigation of this, and the Fluxion in the following Example, are both given in my Eſſays.

The ſecond Example I ſhall give as an Illuſtration of the foregoing Method is,

To determine the Apſide Angle (or the Angle of the two Apſes at the Center) in an Orbit deſcribed by means of a centripetal Force, which varies according to any Power of the Diſtance.

In order to which, let the Velocity of the Body at the higher Apſe be to that whereby it might deſcribe a Circle at the ſame Diſtance from the Center, in the given Ratio of p to Unity; alſo let that Diſtance be denoted by Unity; and, ſuppoſing z to denote any other Diſtance, let the centripetal Force be univerſally expreſſed by z^n. Then the Fluxion of the Angle at the Center will be expreſſed by $\frac{-p\dot{z}}{z\sqrt{p^2 + \frac{2}{n+1} \times z^3 - p^2 - \frac{2z}{n+1}}^{n+3}}$. Put $a = 1 - p^2$, $v = \frac{n+3}{2}$ and $x = 1 - z^2$, and it will become $\frac{\frac{2}{1}\sqrt{1-a} \times \dot{x}}{1 - x \times \sqrt{ax + 1 - v\,x - \overline{1-x}|^v}}$ $= \frac{1}{1-v} = \frac{1}{2}\ 1 - a^{\frac{1}{2}}$ into $a - \frac{vx}{2} + \frac{v.v-2}{2.3} \times x^2 - \frac{v.v-2.v-3}{2.3.4} \times x^3$ &c. $\Big|^{-\frac{1}{2}} \times x^{-\frac{1}{2}}\dot{x} + x^{\frac{1}{2}}\dot{x} + x^{\frac{3}{2}}\dot{x}$ &c.

Now, to find the Fluent of the firſt Term hereof (drawn into the general Multiplicator) or $a - \frac{vx}{2} + \frac{v.v-2}{2.3.} \times x^2$ &c. $\Big|^{-\frac{1}{2}} \times x^{-\frac{1}{2}}\dot{x}$, we have (as before) $n = 1$, $m = -\frac{1}{2}$, $p = \frac{1}{2}$, $b = \frac{v}{2}$, $\frac{d}{b} = \frac{v-2}{3}$, $\frac{e}{b} = -\frac{v-2.v-3}{3.4}$, &c. Alſo $R = \frac{v-2}{6}$, $S = \frac{v-2.4v-5}{72}$; and conſequently the Fluent itſelf (when the Body arrives at the lower Apſe) $= \frac{P}{\sqrt{\frac{1}{2}v}} \times 1 + \frac{v-2}{2v} \times a + \frac{5.v-2.4v-5}{48v^2} \times a^2 + \frac{7.v-v.16v^2-37v+22}{6.48v^3}$ &c. After the ſame manner the Fluent of the ſecond Term will come out $= \frac{P}{\sqrt{\frac{1}{2}v}} \times \frac{a}{v} + \frac{5.v-2}{4v^2} \times a^2 +$

$+\frac{35.\ v-2.\ 2v-3}{48\ v^3} \times a^3$ &c. that of the third $= \frac{P}{\sqrt{\frac{1}{2}v}} \times \frac{3\ a^2}{2\ v^2} + \frac{35.\ v-2}{12\ v^3} \times a^3$ &c. &c. &c. Whence, by collecting theſe ſeveral Fluents together, we have $\frac{P}{\sqrt{\frac{1}{2}v}} \times 1 + \frac{1}{2}a + \frac{20\ v^2 - 5\ v + 2}{48\ v^2} \times a^2 + \frac{112\ v^3 - 63\ v^2 + 42\ v - 8}{6.\ 48\ v^3} \times a^3$ &c. for the Fluent of the whole Expreſſion: And this, drawn into $\frac{1}{2} \times -1 - \frac{a}{2} - \frac{a^2}{8}$ &c. $(= \frac{1}{2} \times \overline{1-a}^{\frac{1}{2}})$ will be $= \frac{P}{\sqrt{2v}} \times 1 * \frac{v-2.\ 2v-1}{48} \times \frac{a^2}{v^2} + \frac{\overline{v-2.\ 2v-1}^2}{72} \times \frac{a^3}{v^3}$ &c. $= \frac{P}{\sqrt{n+3}} \times 1 * \frac{n-1.\ n+2}{24} \times \frac{a^2}{\overline{n+3}^2} + \frac{\overline{n-1.\ n+2}^2}{18} \times \frac{a^3}{\overline{n+3}^3}$ &c. which, in Degrees, gives $\frac{180}{\sqrt{n+3}} \times 1 * + \frac{n-1.\ n+2}{24} \times \frac{a^2}{\overline{n+3}^2} + \frac{\overline{n-1.\ n+2}^2}{18.} \times \frac{a^3}{\overline{n+3}^3}$ &c. for the true Meaſure of the Angle required.

A letter from Mr Abr. De Moivre, *F. R. S. to* William Jones, *Eſq; F. R. S. concerning the eaſieſt method for calculating the value of annuities upon lives, from tables of obſervations. Preſented* June 7, 1744. N°. 473. p. 65. *May.* &c. 1744.

II. Altho' it has been an eſtabliſhed cuſtom, in the payment of annuities on lives, that the laſt rent is loſt to the heirs of the late poſſeſſor of an annuity, if the perſon happens to die before the expiration of the term agreed on for payment, whether yearly, half-yearly, or quarterly: nevertheleſs, in this treatiſe I have ſuppos'd, that ſuch a part of the rent ſhould be paid to the heirs of the late poſſeſſor, as may be exactly proportion'd to the time elaps'd between that of the laſt payment, and the very moment of the life's expiring; and this by a proper, accurate, and geometrical calculation.

I have been induced to take this method, for the following reaſons; firſt, by this ſuppoſition, the value of lives would receive but an inconſiderable increaſe; ſecondly, by this means, the ſeveral intervals of life, which, in the tables of obſervations, are found to have uniform decrements, may be the better connected together. It is with this view that I have framed the two following problems, with their ſolutions.

To

To find the value of an annuity, ſo circumſtantiated, that it ſhall be on a life of a given age; and that, upon the failing of that life, ſuch a part of the rent ſhall be paid to the heirs of the late poſſeſſor of an annuity, as may be exactly proportioned to the time intercepted between that of the laſt payment, and the very moment of the life's failing. Prob. I.

Let n repreſent the complement of life, that is, the interval of time between the given age, and the extremity of old-age, ſuppos'd at 86. Solution.

r the amount of 1 *l.* for one year.

α the logarithm of r.

P the preſent value of an annuity of 1 *l.* for the given time.

Q the value of the life ſought.

Then $\frac{1}{r-1} - \frac{P}{\alpha n} = Q$.

For, let z repreſent any indeterminate portion of n. Now the probability of the life's attaining the end of the interval z, and then failing, is to be expreſſed by $\frac{\dot{z}}{n}$, (as ſhewn in page 77, edit. 1. and in page 115, edit. 2. of my book of annuities upon lives) upon the ſuppoſition of a perpetual and uniform decrement of life. Demonſtration.

But it is well known, that if an annuity certain, of 1 *l.* be paid during the time z, its preſent value will be $P = \frac{1 - r^{-z}}{r-1}$ or $\frac{1}{r-1} - \frac{1}{\overline{r-1} \times r^z}$.

And, by the laws of the doctrine of chances, the expectation of ſuch a life, upon the preciſe interval z, will be expreſſed by $\frac{\dot{z}}{n \times r - 1} - \frac{\dot{z}}{nr^z \times r - 1}$; which may be taken for the ordinate of a curve, whoſe area is as the value of the life required.

In order to find the area of this curve, let $p = n \times \overline{r-1}$; and then the ordinate will become $\frac{\dot{z}}{p} - \frac{\dot{z}}{pr^z}$, a much more commodious expreſſion.

Now it is plain, that the fluent of the firſt part is $\frac{z}{p}$: but as the fluent of the ſecond part is not ſo readily diſcover'd, it will not be improper, in this place, to ſhew by what artifice I found it; for I do not know, whether the ſame method has been made uſe of by others: all

all that I can ſay, is, that I never had occaſion for it, but in the particular circumſtance of this problem.

Let, therefore, $r^z = x$; hence z log. r = log. x; therefore $\dot{z}$ log. r = (fluxion of the log. x =) $\frac{\dot{x}}{x}$, or $\alpha \dot{z} = \frac{\dot{x}}{x}$; conſequently $\dot{z} = \frac{\dot{x}}{\alpha x}$, and $\frac{\dot{z}}{r^z} = \frac{\dot{x}}{\alpha x x}$: but the fluent of $\frac{\dot{x}}{\alpha x x}$ is $(-\frac{1}{\alpha x} =) -\frac{1}{\alpha r^z}$; and therefore the fluent of $-\frac{\dot{z}}{p r^z}$ will be $+\frac{1}{p \alpha r^z}$.

The ſum of the two fluents will be $\frac{z}{p} + \frac{1}{p \alpha r^z}$; but, when $z = 0$, the whole fluent ſhould be $= 0$, let therefore the whole fluent be $\frac{z}{p} + \frac{1}{p \alpha r^z} + q = 0$.

Now, when $z = 0$, then $\frac{z}{p} = 0$, and $\frac{1}{\alpha p r^z}$ becomes $\frac{1}{\alpha p}$ (for $r^z = 1$,) conſequently $\frac{1}{\alpha p} + q = 0$; and $q = -\frac{1}{\alpha p}$: therefore the area of a curve, whoſe ordinate is $\frac{\dot{z}}{p} - \frac{\dot{z}}{p r^z}$ will be $\left(\frac{z}{p} - \frac{1}{\alpha p} + \frac{1}{\alpha p r^z} =\right) \frac{z}{p} - \overline{1 - \frac{1}{r^z}} \times \frac{1}{\alpha p}$.

But $P = \frac{1}{r - 1} - \frac{1}{\overline{r - 1} \times r^z}$; therefore $1 - \frac{1}{r^z} = \overline{r - 1} \times P$, and the expreſſion for the area becomes $\frac{z}{\overline{n \times r - 1}} - \frac{P}{\alpha n}$: And putting n inſtead of z, that area, or the value of the life, will be expreſſed by $\frac{1}{r - 1} - \frac{P}{\alpha n}$. *Q. E. D.*

Thoſe who are well verſed in the nature of logarithms, I mean thoſe that can deduce them from the doctrine of fluxions and infinite ſeries, will eaſily apprehend, that the quantity here called α, is that which ſome call the hyperbolic logarithm; others, the natural logarithm: it is what Mr *Cotes* calls, the logarithm whoſe modulus is 1: laſtly, it is by ſome called *Neper*'s logarithm. And, to ſave the reader ſome trouble in the practice of this laſt theorem, the moſt neceſſary natural logarithms, to be made uſe of in the preſent diſquiſition about lives, are the following:

If

If $r = 1.04$, then will $\alpha = 0.0392207$.
$r = 1.05$, - - - $\alpha = 0.0487901$.
$r = 1.06$, - - - $\alpha = 0.0582589$.

It is to be obſerved, that the theorem here found, makes the values of lives a little bigger, than what the theorem found in the firſt problem of my book of annuities on lives, does; for, in the preſent caſe, there is one payment more to be made, than in the other; however, the difference is very inconſiderable.

But, altho' it be indifferent which of them is uſed, on the ſuppoſition of an equal decrement of life to the extremity of old-age; yet, if it ever happens, that we ſhould have tables of obſervations, concerning the mortality of mankind, intirely to be depended upon, then it would be convenient to divide the whole interval of life into ſuch ſmaller intervals, as, during which, the decrements of life have been obſerved to be uniform, notwithſtanding the decrements in ſome of thoſe intervals ſhould be quicker, or ſlower, than others; for then the theorem here found would be preferable to the other; as will be ſhewn hereafter.

That there are ſuch intervals, Dr *Halley*'s tables of obſervations ſufficiently ſhew; for inſtance; out of 302 perſons of 54 years of age, there remain, after 16 years (that is, of the age of 70) but 142; the decrements from year to year having been conſtantly 10; and the ſame thing happens in other intervals; and it is to be preſumed, that the like would happen in any other good tables of obſervations.

But, in order to ſhew, in ſome meaſure, the uſe of the preceding theorem, it is neceſſary to add another problem; which, tho' its ſolution is to be met with in the firſt edition of my book of annuities on lives, yet it is convenient to have it inſerted here, on account of the connexion that the application of the preceding problem has with it.

In the mean time, it will be proper to know, *What part of the yearly rent ſhould be paid to the heirs of the late poſſeſſor of an annuity, as may be exactly proportioned to the time elapſed between that of the laſt payment, and the very moment of the life's expiring.* To determine this, put A for the yearly rent; $\frac{1}{m}$ for the part of the year intercepted between the time of the laſt payment, and the inſtant of the life's failing; r the amount of 1 *l.* at the year's end: then will $\frac{r^{\frac{1}{m}} - 1}{n - 1} A$, be the ſum to be paid.

To find the value of an annuity for a limited interval of life, during which the decrements of life may be conſidered as equal. Prob. II.

Let a and b repreſent the number of people living in the beginning and end of the given interval of years. Solution.

s repreſent that interval.
P the value of an annuity certain for that interval.
$\mathfrak{Q}$ the value of an annuity for a life ſuppoſed to be neceſſarily extinct in the time s; or (which is the ſame thing) the value of an annuity for a life, of which the complement is s.

Then $\mathfrak{Q} + \frac{b}{a} \times \overline{P - \mathfrak{Q}}$ will expreſs the value required.

Demonſtration. For, let the whole interval between a and b be fill'd up with arithmetical mean proportionals; therefore the number of people living in the beginning and end of each year of the given interval s will be repreſented by the following ſeries; *viz.*

$$a. \frac{sa - a + b}{s}. \quad \frac{sa - 2a + 2b}{s}. \quad \frac{sa - 3a + 3b}{s}. \quad \frac{sa - 4a + 4b}{s}.$$

&c. to b.

Conſequently, the probabilities of the life's continuing during 1, 2, 3, 4, 5, *&c.* years will be expreſſed by the ſeries,

$$\frac{sa - a + b}{sa}. \quad \frac{sa - 2a + 2b}{sa}. \quad \frac{sa - 3a + 3b}{sa}. \quad \frac{sa - 4a + 4b}{sa}.$$

&c. to $\frac{b}{a}$. Wherefore, the value of an annuity of 1 *l.* granted for the time s, will be expreſſed by the ſeries

$$\frac{sa - a + b}{sar} + \frac{sa - 2a + 2b}{sar^2} + \frac{sa - 3a + 3b}{sar^3} + \frac{sa - 4a + 4b}{sar^4}.$$

&c. to $+ \frac{b}{ar^s}$; this ſeries is diviſible into two other ſeries's, *viz.*

$$1ſt. \frac{s - 1}{sr} + \frac{s - 2}{sr^2} + \frac{s - 3}{sr^3} + \frac{s - 4}{sr^4}, \text{ \&c. to } + \frac{s - s}{sr^s}.$$

$$2d. \frac{b}{a} \times \overline{\frac{1}{sr} + \frac{2}{sr^2} + \frac{3}{sr^3} + \frac{4}{sr^4}, \text{ \&c. to } \frac{s}{sr^s}}.$$

Now, ſince the firſt of theſe ſeries's begins with a term whoſe numerator is $s - 1$, and the ſubſequent numerators each decreaſe by unity; it follows, that the laſt term will be $= 0$; and, conſequently, that ſeries expreſſes the value of a life neceſſarily to be extinct in the time s. The ſum of this ſeries may be eſteem'd as a given quantity; and is what I have expreſſed by the ſymbol $\mathfrak{Q}$ in problem 1.

The ſecond ſeries is the difference between the two following ſeries's,

$$\frac{b}{a} \times \overline{\frac{1}{r} + \frac{1}{r^2} + \frac{1}{r^3} + \frac{1}{r^4} + \text{ \&c. to } \frac{1}{r^s}}.$$

$\frac{b}{a}$

$$\frac{b}{a} \times \frac{s-1}{sr} + \frac{s-2}{sr^2} + \frac{s-3}{sr^3} + \frac{s-4}{sr^4} \text{ \&c. to } + \frac{s-s}{sr^s}.$$

Where, neglecting the commom multiplier $\frac{b}{a}$, the firſt ſeries is the value of an annuity certain to continue s years; which every mathematician knows how to calculate, or is had from tables already compoſed for that purpoſe: this value is what I have called P; and the ſecond ſeries is $\mathcal{Q}$.

Therefore $\mathcal{Q} + \frac{b}{a} \times \overline{P - \mathcal{Q}}$ will be the value of an annuity on a life for the limited time. $\mathcal{Q}$. *E. D.*

It is obvious, that the ſeries denoted by $\mathcal{Q}$, muſt of neceſſity have one term leſs than is the number of equal intervals contain'd in s; and therefore, if the whole extent of life, beginning from an age given, be divided into ſeveral intervals, each having it's own particular uniform decrements, there will be, in each of theſe intervals, the defect of one payment; which to remedy, the ſeries $\mathcal{Q}$ muſt be calculated by problem 1.

To find the value of an annuity for an age of 54, *to continue* 16 *years and no longer.* Example.

It is found, in Dr *Halley*'s tables of obſervations, that a is 302, and b 172: now $n = s = 16$; and, by the tables of the values of annuities certain, $P = 10.8377$; alſo (by problem 1.) $\mathcal{Q} = \left(\frac{1}{r-1} - \frac{P}{an} =\right) 6.1168$. Hence it follows (by this problem), that the value of an annuity for an age of 54, to continue during the limited time of 16 years, ſuppoſing intereſt at 5 *per cent. per annum*, will be worth $(\mathcal{Q} + \frac{b}{a} \times \overline{P - \mathcal{Q}} =) 8.3365$ years purchaſe.

From Dr *Halley*'s tables of obſervations, we find, that from the age of 49 to 54 incluſive, the number of perſons, exiſting at thoſe ſeveral ages, are, 357, 346, 335, 324, 313, 302, which comprehends a ſpace of five years; and, following the precepts before laid down, we ſhall find, that an annuity for a life of 49, to continue for the limited time of 5 years, intereſt being at 5*per cent. per annum*, is worth 4.0374 years purchaſe.

And, in the ſame manner, we ſhall find, that the value of an annuity on life, for the limited time comprehended between the ages of 42 and 49, is worth 5.3492 years purchaſe.

Now, if it were required to determine the value of an annuity on life, to continue from the age of 42 to 70, we muſt proceed thus:

It has been proved, that an annuity on life, reaching from the age of 54 to 70, is worth 8.3365 years purchaſe; but this value, being eſtimated from the age of 49, ought to be diminiſhed on two accounts:

becauſe of the probability of the life's reaching from 49 to 54, which probability is to be deduced from the table of obſervations, and is proportional to the number of people living at the end and beginning of that interval, which, in this caſe, will be found 302 and 357: The ſecond diminution proceeds from a diſcount that ought to be made, becauſe the annuity, which reaches from 54 to 70, is eſtimated 5 years ſooner, *viz.* from the age of 49, and therefore that diminution ought to be expreſſed by $\frac{1}{r^5}$; ſo that the total diminution of the annuity of 16 years will be expreſſed by the fraction $\frac{302}{357\,r^5}$, which will reduce it from 8.3365 years purchaſe to 5.5259; this being added to the value of the annuity to continue from 49 to 54, *viz.* 4.0374, will give 9.5633, the value of an annuity to continue from the age of 49 to 70. For the ſame reaſon, the value 9.5633, eſtimated from the age of 42, ought to be reduced, both upon account of the probability of living from 42 to 49, and of the diſcount of money for 7 years, at 5 *per cent. per annum*, amounting together to 3.8554, which will bring it down to 5.7079; to this adding the value of an annuity on a life to continue from the age of 42 to 49, found before to be 5.3492, the ſum will be 11.0571 years purchaſe, the value of an annuity to continue from the age of 42 to 70.

In the ſame manner, for the laſt 16 years of life, reaching from 70 to 86, when properly diſcounted, and alſo diminiſhed upon the account of the probability of living from 42 to 70, the value of thoſe laſt 16 years will be reduced to 0.8; this being added to 11.0571 (the value of an annuity to continue from the age of 42 to 70, found before), the ſum will be 11.8571 years purchaſe, the value of an annuity to continue from the age of 42 to 86; that is, the value of an annuity on a life of 42; which, in my tables, is but 11.57, upon the ſuppoſition of an uniform decrement of life, from an age given to the extremity of old-age, ſuppoſed at 86.

It is to be obſerved, that the two diminutions, above-mention'd, are conformable to what I have ſaid in the corollary to the ſecond problem of the firſt edition, printed in the year 1724.

Thoſe who have ſufficient leiſure and ſkill to calculate the value of joint lives, whether taken two and two, or three and three, in the ſame manner as I have done the firſt problem of this tract, will be greatly aſſiſted by means of the two following theorems:

If the ordinate of a curve be $\frac{z}{r^z}$; it's area will be $\frac{1}{a^z} - \frac{1}{a^z\,r^z} - \frac{z}{a\,r^z}$.

If the ordinate of a curve be $\frac{z^2}{r^z}$; it's area will be $\frac{2}{a^3} - \frac{2}{a^3\,r^z} - \frac{2z}{a^2\,r^z} - \frac{z^2}{a\,r^z}$.

I beg

I beg leave, in this place, to take notice, that in the theorem in line 12, page 63. of the ſecond edition of my book of annuities on lives, inſtead of P, it ought to be $\frac{p}{n} P$; where n and p repreſent the complements of the age, in the beginning and end of a given interval of time.

And I deſire the reader of that edition to adapt the fourth article of the rule put in words at length, in page 64, to the theorem ſo corrected: then the ſolution there given, and that in page 21. of the firſt edition, will perfectly agree; provided that the decrements of life be ſuppoſed, in both caſes, uniform, from an age given, to the extremity of old-age.

I muſt alſo take notice of an accidental error, that has crept into the 25th propoſition of the ſecond edition; which I chuſe to correct as follows;

1. Let the firſt line of the propoſition, and part of the ſecond line, as far as *A* excluſive, be eraſed.

2. Let the ſolution proceed thus: ſince the life of *A* is ſuppoſed to be worth 14 years purchaſe, when intereſt is at 4 *per cent. per annum*, it follows, from our tables, that *A* muſt be 35 years of age; therefore find, by the twenty-third propoſition, the value of an annuity of a life for 35 to continue for a limited time of 31 years: let that value be ſubducted from the value of an annuity certain, to continue 31 years; and the remainder will be the value of the reverſion.

III. The *Chineſe* have for many ages picqued themſelves on being the moſt wiſe of any nation in the world; but late experience and cloſer converſe with them hath found this pride to be ill-grounded. One-particular, in which they think they excel all mankind, is, their manner of accompting, which they do with an inſtrument compoſed of a number of wires with beads upon them, which they move backwards and forwards. This inſtrument they call a *Shwan-pan*.

An account of a new invented arithmetical inſtrument called a Shwan-pan, *or* Chineſe Accompt-Table; *by* Gamaliel Smethurſt. *Read* Jan. 29. 1748. N°. 491. p. 22. Jan. &c. 1749.

Now I truſt I have formed one on the plan of our 9 digits, that in no caſe falls ſhort of the *Chineſe Shwan-pan*, but in many excels theirs.

The *Chineſe*, according to the accounts of travellers, are ſo happy as to have their parts of an integer in their coins, &c. decimated, ſo can multiply or divide their integers and parts as if they were only integers. This gives them the advantage over *Europeans* in reckoning their money, &c. But then, as they have no particular place ſet apart for the leſſer denominations of coins, weights, meaſures, &c. their inſtrument can't be uſed in *Europe*, nor can it be ſo univerſally applied to arithmetick as mine, for I have provided for the different diviſions of an integer into parts.

This inſtrument hath the advantage of our digits in a great many Caſes. Firſt, the figures can be felt, ſo may be uſed by a blind man. If it had no other, this alone would be ſufficient to gain it the attention of mankind.

Another

Another advantage from it is, that, when attain'd, this method is much ſwifter than by our digits, and leſs liable to miſtakes: it is likewiſe not ſo burdenſome to the memory in working the rules of arithmetick, as by our digits, we being oblig'd to carry the tens in the mind from one place to another, which are ſet down by the *Shwan-pan*. —— One may work a whole night, without confuſing the head, or affecting the eyes in the leaſt.

It may be of great uſe to teach people the power of numbers, likewiſe to examine accompts by; for, as the perſon will, by the *Shwan-pan*, work it quite a different way, it will ſerve as if another perſon had gone thro' the accompt; if it proves right with the written one, they may reſt aſſured the work is true.

It may be a very pretty lure to lead young people to apply their minds to numbers *.

THEOREM.

An extract of a letter from William Jones, *Eſq*; *F. R. S. to* Martin Folks, *Eſq*; *Preſident of the* Royal Society; *containing a commodious diſpoſition of equations for exhibiting the relations of Goniometrical Lines. Preſented* 4 July 1747. No. 483. p. 560. March, &c. 1753.

IV. *In a circle whoſe radius is* r, *let there be two arcs,* A *the greater,* a *the leſs, each in the firſt quadrant*; *put* s, t, ſ, *and* v, *for the ſine, tangent, ſecant, and verſed ſine of an arc*; s`, t`, ſ`, *the ſine, tangent, ſecant of the complement, and* v`, *the verſed ſine of the ſupplement of that arc*; *let* $z = \frac{1}{2}\overline{A + a}$, $x = \frac{1}{2}\overline{A - a}$; *or if* z *and* x *be put for the arcs, it will be* A = z + x, a = z − x.

Then will the terms in any column of the following table, be proportional to their correſponding ones in any other column.

A TABLE of the Relations of Goniometrical Lines.

$2s,z$	$s,2z$ / $s,\overline{A+a}$	$v,2z$ / $v,\overline{A+a}$	$sA+sa$	$s`a-s`A$ / $vA-va$
$2s,x$	$sA-sa$	$sa-s`A$ / $vA-va$	$s,2x$ / $s,\overline{A-a}$	$v,2x$ / $v,\overline{A-a}$
$2s`,z$	$v`,2z$ / $v\overline{A+a}$	$s,2z$ / $s,\overline{A+a}$	$s`A+s`a$	$sA-sa$
$2s`,x$	$s`A+s`a$	$sA+sa$	$v`,2x$ / $v`\overline{A-a}$	$s,2x$ / $s,\overline{A-a}$
r	$s`,z$	s,z	$s`,x$	s,x
s,z	r	t,z		
t,z	s,z	$s,z-s`z$		
$s`,z$	$t`,z$	r		
$t,z+t,z$	$s`,z$	s,z		
s,x			r	t,x
t,x			s,x	$s,x-sx$

* The inventor produced one of theſe inſtruments before the *Society*, and work'd ſeveral queſtions in arithmetick upon it. It much reſembles the *abacus* of the ancients. *C. M.*

From

From hence, almost an infinite number of theorems may easily be derived; some of which are the following, given here as examples of the use of the table.

I. $s,z \times s,x = \frac{1}{2}r \times \overline{s`,a - s`,A} = \frac{1}{2}r \times \overline{s`,z - x - s`,z + x}$
$= \frac{s,z}{\int`,x}\,rr = \frac{s,x}{\int`,z}\,rr.$

$\overline{s`,z \times s`,x} = \frac{1}{2}r \times \overline{s`,a + s`,A} = \frac{1}{2}r \times \overline{s`,z - x + s`,z + x}$
$= \frac{s`,z}{\int,x}\,rr = \frac{s`,x}{\int,z}\,rr.$

II. If A, B, C, be any three angles; $Z = A + B$, $X = A - B$, $H = \frac{1}{2}A + B + C$.

Then $\frac{1}{2}r \times \overline{v,C - v,X} = s,\overline{\frac{1}{2}C + X} \times s,\overline{\frac{1}{2}C - X} = s,\overline{\frac{1}{2}A + C - B}$
$\times s,\overline{\frac{1}{2}B + C - A} = s,\overline{H - B} \times s,\overline{H - A}.$

And $\frac{1}{2}r \times \overline{v,Z - v,C} = s,\overline{\frac{1}{2}Z + C} \times s,\overline{\frac{1}{2}Z - C} = s,\overline{\frac{1}{2}A + B + C}$
$\times s,\overline{\frac{1}{2}A + B - C} = s,H \times s,\overline{H - C}.$

III. $\frac{ss,z}{s's`,z} = \frac{tt,z}{rr} = \frac{rr}{t't`,z} = \frac{v,2z}{v`,2z} = \frac{t,z}{t`,z}$; Or $\frac{ss,\frac{1}{2}z}{s's`,\frac{1}{2}z} = \frac{tt,\frac{1}{2}z}{rr}$
$= \frac{rr}{tt`,\frac{1}{2}z} = \frac{v,z}{v`,z} = \frac{t,\frac{1}{2}z}{t`,\frac{1}{2}z}.$

IV. $\frac{1}{2}r = \frac{ss,z}{v,2z} = \frac{ss,\frac{1}{2}z}{v,z} = \frac{s's`,z}{v`,2z} = \frac{s's`,\frac{1}{2}z}{v`,z}$; and $s,z = \frac{2ss,\frac{1}{2}z}{t,\frac{1}{2}z}$
$= \frac{2s's`,\frac{1}{2}z}{t`,\frac{1}{2}z}.$

V. $\frac{s,2}{v,z} = \frac{r}{t,\frac{1}{2}z} = \frac{t`,\frac{1}{2}z}{r} = \frac{v`,z}{s,z}.$

VI. $\frac{t,z}{t,x} = \frac{s,A + s,a}{s,A - s,a} = \frac{t`,x}{t`,z}$; and $\frac{rr}{t,z \times t,x} = \frac{t`,z}{t,x} = \frac{t`,x}{t,z}$
$= \frac{s`,a + s`,A}{s`,a - s`,A} = \frac{t`,z \times t`,x}{rr}.$

VII. $\frac{s,A}{s,a} = \frac{t,z + t,x}{t,z - t,x} = \frac{\overline{s,z + x}}{\overline{s,z - x}}$; if z and x are two arcs, then $A = z + x, a = z - x$.

VIII.

VIII. $s,z \pm x = \dfrac{s,z \times s',x \mp s',z \times s,x}{r} = \dfrac{t,z \pm t,x}{\int,z \times \int,x}$.

IX. $s',z \pm x = \dfrac{s',z \times s',x \mp s,z \times s,x}{r} = \dfrac{rr \mp t,z \times t,x}{\int,z \times \int,x} r$.

X. $t,z \pm x = \dfrac{t,z \pm t,x}{rr \mp t,z \times t,x} rr$; and $t',z \pm x = \dfrac{rr \mp t,z \times t,x}{t,z \pm t,x}$.

XI. $\int,z \pm x = \dfrac{\int,z \times \int,x}{rr \mp t,z \times t,x} r$; and $\int',z \pm z = \dfrac{\int,z \times \int,x}{t,z \pm t,x}$.

XII. In three equidifferent arcs A, z, a; where $z\ (= \frac{1}{2}\overline{A + a})$ is the mean arc, and x $(= \frac{1}{2}A - a)$ their common difference; put $p = \frac{s,x}{r}$, $q = \frac{s',x}{r}$; $P = 2p \times s,z$, $Q = 2q \times s',z$.

Then $s,A = P - s,a = Q + s,a$; And $s,a = P - s,A = s,A - Q$.

XIII. Let $d = v,A - v,a = s',a - s',A$; then $ss,A - ss,a = 2s',A + d \times d = 2s',a - d \times d$. *

XIV. Let A, B, C, &c. be the sines, a, b, c, &c. the co-sines, a', b', c', &c. the tangents, of the arcs, α, β, γ, &c. whose number is n; the radius being r; put S for the product of the n co-sines, S', S'', S''', &c. for the sum of the products made of every sine, every two, three, &c. sines, by the other $(n - 1,\ n - 2,\ n - 3,$ &c.) co-sines, where the co-sine noted by $n - n$ is unity.

Then the sine of $\overline{\alpha + \beta + \gamma + \delta,\ \&c.} = \overline{S' - S''' + S^{v} - S^{v\prime\prime},\ \&c.} \times \dfrac{1}{,n - 1}$.

And the co-sine of $\overline{\alpha + \beta + \gamma + \delta,\ \&c.} = \overline{S - S'' + S'^{v} - S^{v},\ \&c.} \times \dfrac{1}{,n - 1}$.

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* *Note*, When an arc is terminated in the second, third, or fourth quadrant, some of the signs (+ and −) of the terms in the preceding theorems, will, by the known rules, become contrary to what they now are.

XV.

XV. Alſo putting T' for the ſum of the tangents of the arcs, α, β, γ, &c. T'', T''', T'^{v}, &c. for the ſum of the products of every two, three, four, &c. tangents; and $A = T'$

$$B = AT'' - T'''$$
$$C = BT'' - AT'^{v} + T^{y}$$
$$D = CT'' - BT'^{y} + AT^{y\prime} - T^{y\prime\prime}$$
$$E = DT'' - CT'^{y} + BT^{y\prime} - AT^{v\prime\prime\prime} + T'^{x}$$

Put $R = \frac{1}{rr}$.

Then the tangent of $\overline{\alpha + \beta + \gamma + \delta, \&c.} = A + BR + CR^2 + DR^3 + ER^4$; &c.

XVI. Hence, the ſine, tangent, and ſecant, of any arc a, being repreſented by s, t, $ſ$, the co-ſine, co-tangent, and co-ſecant, by $s`$, $t`$, $ſ`$; thoſe of the arc na are expreſſed as in the following theorems.

Putting $n' = n.\frac{n-1}{2}$; $n'' = n'.\frac{n-2}{3}$; $n''' = n''.\frac{n-3}{4}$; $n'^{y} = n'''.\frac{n-4}{5}$; &c.

Sine of $na = \overline{nA - n''AP + n'^{v}BP - n^{y\prime}CP + n^{v\prime\prime\prime}DP, \&c.} \times \frac{s`^{n-1}}{r^{n-1}}$; where $P = \frac{ss}{s`s`}$; $A = s$; $B = AP$; $C = BP$; $D = CP$; &c.

Or $= ns - \frac{n-1}{2}.\frac{n-2}{3}AP + \frac{n-3}{4}.\frac{n-4}{5}BP - \frac{n-5}{6}.\frac{n-6}{7}\overline{CP\ \&c.} \times \frac{s`^{n-1}}{r^{n-1}}$; where A, B, C, &c. ſtand for the reſpective preceding terms.

Or $= ns + \frac{1+n}{2}.\frac{1-n}{3}AQ + \frac{3+n}{4}.\frac{3-n}{5}BQ + \frac{5+n}{6}.\frac{5-n}{7}CQ + \frac{7+n}{8}.\frac{7-n}{9}DQ$. &c. where $Q = \frac{ss}{rr}$; A, B, C, &c. ſtand as before.

XVII. Co-ſine of $na = \overline{1 - n`P + n'''P^2 - n^{y}P^3 + n^{y\prime\prime}P^4, \&c.} \times \frac{s`^{n}}{r^{n-1}}$, where $P = \frac{ss}{s`s`}$.

Or $= r + \frac{0+n}{1}.\frac{0-n}{2}AQ + \frac{2+n}{3}.\frac{2-n}{4}BQ + \frac{4+n}{5}.\frac{4-n}{6}CQ +$

$C\mathcal{Q} + \frac{6+n}{7} \cdot \frac{6-n}{8} D\mathcal{Q}$, &c. where $\mathcal{Q} = \frac{ss}{r}$; and A, B, C, &c. stand for the respective preceding terms.

Or put $M = \overline{\frac{2s}{r}}\Big|^n \times r$; $N = \frac{rr}{4s's'}$; $A = \frac{1}{2}$; $B = AN$; $C = BN$; $D = CN$, &c; $p = n$; $p' = n - 1$; $p'' = n - 2$, &c. And $a' = p$; $b' = p \cdot \frac{1}{2}p'''$; $c' = p \cdot \frac{1}{2}p'^{v} \cdot \frac{1}{3}p^{v}$; $d' = p \cdot \frac{1}{2}p^{v} \cdot \frac{1}{3}p^{y'} \cdot \frac{1}{4}p^{v'''}$; $e' = p \cdot \frac{1}{2}p^{y'} \cdot \frac{1}{3}p^{y''} \cdot \frac{1}{4}p^{y'''} \cdot p\frac{1}{5}p'^{x}$; &c.

The co-sine of $na = \overline{A - Ba' + Cb' - Dc' + Ed'}$, &c. $\times M$.

XVIII. Let $A = -n'' + nn'$

$B = +n'^{v} - nn''' + An'$

$C = -n^{v'} + nn^{v} + Bn' - An'''$

$D = +n^{v'''} - nn^{v''} + Cn' - Bn''' + An^{v}$ &c.

$$A' = \frac{1}{n} \cdot n'' - n'$$

$$B' = \frac{1}{n} \cdot \overline{n'' A' - n'^{y}} + n'''$$

$$C' = \frac{1}{n} \cdot \overline{n'' B' - n'^{y} A'} + n^{y'} - n^{y}$$

$$D' = \frac{1}{n} \cdot \overline{n'' C' - n'^{y} B' + n^{y'} A' - n^{y'''}} + n^{v''}$$ &c.

The tangent of $na = nt + At^3 r^{-2} + Bt^5 r^{-4} + Ct^7 r^{-6} + Dt^9 r^{-8}$ &c.

Or $= \overline{n + AN + BN^2 + CN^3 + DN^4 +}$ &c. $\times t$, where $N = \frac{tt}{rr}$.

Or $= na' + Ab' + Bc' + Cd' + De'$, &c. where $a' = t$; $b' = Na'$; $c' = Nb'$; $d' = Ne'$; &c.

Or $= \frac{n - n''N + n'^{v}N^2 - n^{y'}N^3 + n^{v'''}N^4, \text{ &c.}}{1 - n'N + n'''N^2 - n^{y}N^3 + n^{y''}N^4 . \text{ &c.}} \times t$.

Co-tangent of $na = \overline{r^2 + A't^2N + C't^2N^2 + D't^2N^3 + E't^2N^4}$ &c. $\times \frac{rr}{nt}$; where $N = \frac{tt}{rr}$.

Or $= \overline{1 + A'N + B'N^2 + C'N^3 + D'N^4 + E'N^5}$, &c. $\times \frac{1}{7} r^2 t'$; where $N = \frac{rr}{t't'}$.

Or

Or $= \frac{1 - n' N + n''' N^2 - n^v N^3 + n^{v'} N^4 - n'^x N^5, \&c.}{n - n'' N + n'^v N^2 - n^{v'} N^3 + n^{v'''} N^4 - n^x N^5, \&c.} \times \frac{rr}{t}$; where

$N = \frac{tt}{rr}$.

XIX. Let $A = n'$

$B = An' - n'''$

$C = Bn' - An''' + n^v$

$D = Cn' - Bn''' + An^v - n^{v'}$ &c.

$A' = \frac{1}{n} . n''$

$B' = \frac{1}{n} . \overline{n'' A' - n'^v}$

$C' = \frac{1}{n} . \overline{n'' B' - n'^v A + n^{v'}}$

$D' = \frac{1}{n} . \overline{n'' C' - n'^v B' + n^{v'} A' - n^{v'''}}$ &c.

Secant of $na = 1 + AN + BN^2 + CN^3 + DN^4 + EN^5$, &c. $\times M$.

Or $= \frac{1}{1 - n' N + n''' N^2 - n^v N^3 + n^{v''} N^4, \&c.} \times M$; where $N = \frac{tt}{rr}$,

$M = \frac{r \int n}{rn}$.

Co-secant of $na = 1 + A' N + B' N^2 + C' N^3 + D' N^4 + E' N^5$, &c. $\times$ M; where $N = \frac{tt}{rr}$, $M = \frac{rr \int n}{ntrn}$.

Or $= \frac{1}{n - n' N + n'^v N^2 - n^{v'} N^3 + n^{v'''} N, \&c.} \times M$; where $N = \frac{tt}{rr}$,

$M = \frac{rn - 2}{t \int n - 2}$.

XX. Let c be the chord of an arc (a) of the circumference of a circle, whose diameter is d. Put $N = \frac{cc}{dd}$.

The chord of $na = nc + \frac{1+n}{2} . \frac{1-n}{3} AN + \frac{3+n}{4} . \frac{3-n}{5} BN +$

$\frac{5+n}{6} \cdot \frac{5-n}{7} CN + \frac{7+n}{8} \cdot \frac{7-n}{9} DN$, &c. where A, B, C, &c. ſtand for the reſpective preceding terms.

As the preceding theorems are eaſily deduced from the firſt, ſo the following are moſt readily ſeen to be the immediate conſequences of theſe; and all depending upon no other principles than what are generally made uſe of in common computations.

XXI. Putting s, s', t, t', f, f', for the ſine, co-ſine, tangent, co-tangent, ſecant, co-ſecant, of an arc (a), and v it's verſed ſine; let $q' = \frac{1}{2}$; $q'' = \frac{1}{3}q'$; $q''' = \frac{1}{4}q''$; $q'^{v} = \frac{1}{5}q'''$; $q^{v} = \frac{1}{6}q'^{v}$; &c. $N = \frac{aa}{rr}$.

Then $s = \overline{1 - q''N + q'^{v}N^2 - q^{v\prime}N^3 + q^{v\prime\prime\prime}N^4 + q'^{x}N^5, \&c.} \times a.$

$= a - q''a^3r^{-2} + q'^{v}a^5r^{-4} - q^{v\prime}a^7r^{-6} + q^{v\prime\prime\prime}a^9r^{-8}$, &c.

$= a - \frac{1}{2.3}AN + \frac{1}{4.5}BN - \frac{1}{6.7}CN + \frac{1}{8.9}DN$, &c. where A, B, C, &c. ſtand for the reſpective preceding terms.

And $s' = r - q'a^2r^{-1} + q'''a^4r^{-3} - q^{v}a^6r^{-5} + q^{v\prime\prime}a^8r^{-7}$, &c.

$= \overline{1 - q'N + q'''N^2 - q^{v}N^3 + q^{v\prime\prime}N^4 \quad q'^{x}N^5, \&c.} \times r.$

$= r - \frac{1}{1.2}a^2r^{-1} + \frac{1}{3.4}AN - \frac{1}{5.6}BN + \frac{1}{7.8}CN$, &c. A, B, C, &c. as before.

XXII. Alſo $v = q'a^2r^{-1} - q'^{v}a^4r^{-3} + q^{v\prime}a^6r^{-5} - q^{v\prime\prime\prime}a^8r^{-7}$, &c.

$= \frac{1}{1.2}a^2r^{-1} - \frac{1}{3.4}AN - \frac{1}{5.6}BN - \frac{1}{7.8}CN - \frac{1}{9.10}DN$, &c.

$= \frac{1}{1.2}N - \frac{1}{3.4}AN - \frac{1}{5.6}BN - \frac{1}{7.8}CN$, &c. $\times r$.

A, B, C, &c. as before.

XXIII. Let $A = +q' \quad -q''$

$B = -q''' + q'^{v} + Aq'$

$C = +q^{v} \quad -q^{v\prime} + Bq' - Aq'''$

$D = -q^{v\prime\prime} + q^{v\prime\prime\prime} + Cq' - Bq''' + Aq^{v}$, &c.

And $A' = -A$

$B' = -B - AA'$

$C' = -C - BA' - AB'$

$D' = -D - CA' - BB' - AC'$, &c.

Tangent

Tangent $t = a + Aa^3r^{-2} + Ba^5r^{-4} + Ca^7r^{-6} + Da^9r^{-8}$, &c.

Or $= \overline{1 + AN + BN^2 + CN^3 + DN^4 + EN^5, \&c.} \times a.$

Co-tangent $t' = a^{-1}r^2 + Aa + B'a^3r^{-2} + C'a^5r^{-4} + D'a^7r^{-6}$, &c.

Or $= \overline{rr + Aa^2 + B'Na^2 + C'N^2a^2 + D'N^3a^2, \&c.} \times \frac{1}{a}$.

XXIV. Alſo let $\alpha = +q'$

$\beta = -q''' + \alpha q'$

$\gamma = +q^{v} - \alpha q''' + \beta q'$

$\delta = -q^{v\prime\prime} + \alpha q^{v} - \beta q''' + \gamma q'$, &c.

And $\alpha' = +q''$

$\beta' = -q^{v} + \alpha' q''$

$\gamma' = +q^{v\prime} - \alpha' q^{v} + \beta' q''$

$\delta' = -q^{v\prime\prime\prime} + \alpha' q^{v\prime} - \beta' q^{v} + \gamma' q''$, &c.

Secant $\int = r + \alpha a^2r^{-1} + \beta a^4r^{-3} + \gamma a^6r^{-5} + \delta a^8r^{-7}$, &c.

Or $= \overline{1 + \alpha N + \beta N^2 + \gamma N^3 + \delta N^4, \&c.} \times r.$

Co-ſecant $\int' = a^{-1}r^2 + \alpha' a + \beta' a^3r^{-2} + \gamma' a^5r^{-4} + \delta' a^7r^{-6}$, &c.

Or $= \overline{rr + \alpha' aa + \beta' Naa + \gamma' N^2aa + \delta' N^3aa, \&c.} \times \frac{1}{a}$. where $N = \frac{aa}{rr}$.

XXV. Putting $p' = \frac{1}{2}$; $p'' = \frac{1}{4}p'$; $p''' = \frac{5}{6}p''$; $p^{v} = \frac{7}{8}p'''$; $p^{v\prime} = \frac{9}{10}p^{v}$; &c. $N = \frac{ss}{rr}$.

Then arc $a = \overline{1 + \frac{1}{3}p'N + \frac{1}{5}p''N^2 + \frac{1}{7}p'''N^3 + \frac{1}{9}p^{v}N^4, \&c.} \times s.$

Or $= s + \frac{1}{3}p'AN + \frac{1}{5}p''BN + \frac{1}{7}p'''CN + \frac{1}{9}p^{v}DN, \&c.$

Or $= s + \frac{1.1}{2.3}AN + \frac{3.3}{4.5}BN + \frac{5.5}{6.7}CN + \frac{7.7}{8.9}DN, \&c.$

where A, B, C, &c. ſtand for the reſpective preceding terms.

XXVI. If v is the verſed ſine of an arc a, diameter being d, $M = \frac{v}{d}$, $R = \sqrt{dv}$.

Then arc $a = \overline{1 + \frac{1.1}{2.3}M + \frac{3.3}{4.5}AM + \frac{5.5}{6.7}BM + \frac{7.7}{8.9}CM, \&c.} \times R$; A, B, C, &c. are as before.

XXVII.

XXVII. And putting $N=\frac{tt}{rr}$, $A=t$, $B=AN$, $C=BN$, $D=CN$, &c.

Then arc $a=t-\frac{1}{3}AN+\frac{1}{5}BN-\frac{1}{7}CN+\frac{1}{9}DN+\frac{1}{11}EN$, &c.

Or $=1-\frac{1}{3}N+\frac{1}{5}N^2-\frac{1}{7}N^3+\frac{1}{9}N^4+\frac{1}{11}N^5$, &c. $\times t$.

XXVIII. Alſo, if c is the chord of an arc (a); and $N=\frac{cc}{dd}$.

Then arc $a=c+\frac{1.1}{2.3}AN+\frac{3.3}{4.5}BN+\frac{5.5}{6.7}CN+\frac{7.7}{8.9}DN$, &c.

where A, B, C, &c. ſtand for the reſpective preceding terms.

An Eſſay on Quantity; *occaſioned by reading a Treatiſe, in which* Simple *and* Compound Ratio's *are applied to* Virtue *and* Merit, *by the Rev. Mr* Reid; *communicated in a Letter from the Rev.* Henry Miles *D. D. & F. R. S. to* Martin Folkes, *Eſq*; *Pr. R. S. Read* Nov. 3 1748. N°. 489 p. 505, October, &c. 1748.

SECT. I. *What Quantity is.*

V. Since mathematical demonſtration is thought to carry a peculiar evidence along with it, which leaves no room for further diſpute; it may be of ſome uſe, or entertainment at leaſt, to inquire to what ſubjects this kind of proof may be applied.

Mathematics contain properly the doctrine of meaſure; and the object of this ſcience is commonly ſaid to be quantity; therefore quantity ought to be defined, what may be meaſured. Thoſe who have defined quantity to be whatever is capable of more or leſs, have given too wide a notion of it, which I apprehend has led ſome perſons to apply mathematical reaſoning to ſubjects that do not admit of it.

Pain and pleaſure admit of various degrees, but who can pretend to meaſure them? Had this been poſſible, it is not to be doubted but we ſhould have had as diſtinct names for their various degrees, as we have for meaſures of length or capacity; and a patient ſhould have been able to deſcribe the quantity of his pain, as well as the time it began, or the part it affected. To talk intelligibly of the quantity of pain, we ſhould have ſome ſtandard to meaſure it by; ſome known degree of it ſo well aſcertained, that all men, when they talked of it, ſhould mean the ſame thing; we ſhould alſo be able to compare other degrees of pain with this, ſo as to perceive diſtinctly, not only whether they exceed or fall ſhort of it, but how far, or in what proportion; whether by an half, a fifth, or a tenth.

Whatever has quantity, or is meaſurable, muſt be made up of parts, which bear proportion to one another, and to the whole; ſo that it may be increaſed by addition of like parts, and diminiſhed by ſubtraction, may be multiplied and divided; and, in a word, may bear any proportion to another quantity of the ſame kind, that one line or number can bear to another. That this is eſſential to all mathematical quantity, is evi-

dent

dent from the firſt elements of algebra, which treats of quantity in general, or of thoſe relations and properties which are common to all kinds of quantity, Every algebraical quantity is ſuppoſed capable not only of being increaſed and diminiſhed, but of being exactly doubled, tripled, halfed, or of bearing any aſſignable proportion to another quantity of the ſame kind. This then is the characteriſtick of quantity; whatever has this property may be adopted into mathematicks; and it's quantity and relations may be meaſured with mathematical accuracy and certainty.

SECT. 2. *Of Proper and Improper Quantity.*

There are ſome quantities which may be called *proper*, and others *improper*. This diſtinction is taken notice of by *Ariſtotle*; but it deſerves ſome explication.

I call that *proper* quantity which is meaſured by it's own kind; or which of it's own nature is capable of being doubled or tripled, without taking in any quantity of a different kind as a meaſure of it. Thus a line is meaſured by known lines, as inches, feet, or miles; and the length of a foot being known, there can be no queſtion about the length of two feet, or of any part or multiple of a foot. And this known length, by being multiplied or divided, is ſufficient to give us a diſtinct idea of any length whatſoever.

Improper quantity is that which cannot be meaſured by it's own kind; but to which we aſſign a meaſure by the means of ſome proper quantity that is related to it. Thus velocity of motion, when we conſider it by itſelf, cannot be meaſured. We may perceive one body to move faſter, another ſlower; but we can have no diſtinct idea of a proportion or *ratio* between their velocities, without taking in ſome quantity of another kind to meaſure them by. Having therefore obſerved, that by a greater velocity a greater ſpace is paſſed over in the ſame time, by a leſs velocity a leſs ſpace, and by an equal velocity an equal ſpace; we hence learn to meaſure velocity by the ſpace paſſed over in a given time, and to reckon it to be in exact proportion to that ſpace: and having once aſſigned this meaſure to it, we can then, and not till then, conceive one velocity to be exactly double, or half, or in any other proportion to another; we may then introduce it into mathematical reaſoning without danger of confuſion, or error, and may alſo uſe it as a meaſure of other improper quantities.

All the kinds of proper quantity we know, may, I think, be reduced to theſe four, extenſion, duration, number, and proportion. Though proportion be meaſurable in it's own nature, and therefore hath proper quantity; yet as things cannot have proportion which have not quantity of ſome other kind, it follows, that whatever has quantity muſt have it in one or other of theſe three kinds, extenſion, duration, or number. Theſe are the meaſures of themſelves, and of all things elſe that are meaſurable.

Number is applicable to ſome things, to which it is not commonly applied by the vulgar. Thus, by attentive conſideration, lots and chances of various kinds appear to be made up of a determinate number of chances that are allowed to be equal; and by numbering theſe, the values

lues and proportions of thoſe which are compounded of them may be demonſtrated.

Velocity, the quantity of motion, denſity, elaſticity, the *vis inſita*, and *impreſſa*, the various kinds of centripetal forces, and different orders of fluxions, are all improper quantities; which therefore ought not to be admitted into mathematics, without having a meaſure of them aſſigned. The meaſure of an improper quantity ought always to be included in the definition of it; for it is the giving it a meaſure that makes it a proper ſubject of mathematical reaſoning. If all mathematicians had conſidered this as carefully as Sir *I. Newton* appears to have done, ſome labour had been ſaved both to themſelves and to their readers. That great man, whoſe clear and comprehenſive underſtanding appears, even in his definitions, having frequent occaſion to treat of ſuch improper quantities, never fails to define them, ſo as to give a meaſure of them, either in proper quantities, or in ſuch as had a known meaſure. This may be ſeen in the definitions prefixed to his *Princip. Phil. Nat. Math.*

It is not eaſy to ſay how many kinds of improper quantity may in time, be introduced into mathematics, or to what new ſubjects meaſures may be applied: but this I think we may conclude, that there is no foundation in nature for, nor can any valuable end be ſerved by, applying meaſure to anything but what has theſe two properties. Firſt, it muſt admit of degrees of greater and leſs. Secondly, it muſt be aſſociated with, or related to, ſomething that has proper quantity, ſo as that when one is increaſed, the other is increaſed, when one is diminiſhed, the other is diminiſhed alſo; and every degree of the one, muſt have a determinate magnitude or quantity of the other, correſponding to it.

It ſometimes happens, that we have occaſion to apply different meaſures to the ſame thing. Centripetal force, as defined by *Newton*, may be meaſured various ways, he himſelf gives different meaſures of it, and diſtinguiſhes them by different names, as may be ſeen in the above-mentioned definitions.

In reality, I conceive that the applying of meaſures to things that properly have not quantity, is only a fiction or artifice of the mind, for enabling us to conceive more eaſily, and more diſtinctly to expreſs and demonſtrate, the properties and relations of thoſe things that have real quantity. The propoſitions contained in the two firſt books of *Newton*'s *principia* might perhaps be expreſſed and demonſtrated, without thoſe various meaſures of motion, and of centripetal and impreſſed forces which he uſes: but this would occaſion ſuch intricate and perplexed circumlocutions, and ſuch a tedious length of demonſtrations as would fright any ſober perſon from attempting to read them.

Sect 3. *Corol.* 1. From the nature of quantity we may ſee what it is that gives mathematics ſuch advantage over other ſciences, in clearneſs and certainty; namely, that quantity admits of a much greater variety of relations than any other ſubject of human reaſoning; and at the ſame time, every relation or proportion of quantities may, by the help of lines and numbers, be

be ſo diſtinctly defined, as to be eaſily diſtinguiſhed from all others, without any danger of miſtake. Hence it is, that we are able to trace it's relations through a long proceſs of reaſoning, and with a perſpicuity and accuracy which we in vain expect in ſubjects not capable of menſuration.

Extended quantities, ſuch as lines, ſurfaces, and ſolids, beſides what they have in common with all other quantities, have this peculiar, That their parts have a particular place and diſpoſition among themſelves: a line may not only bear any aſſignable proportion to another, in length or magnitude, but lines of the ſame length may vary in the diſpoſition of their parts; one may be ſtreight, another may be part of a curve of any kind or dimenſion, of which there is an endleſs variety, The like may be ſaid of ſurfaces and ſolids. So that extended quantities, admit of no leſs variety with regard to their form, than with regard to their magnitude: and as their various forms may be exactly defined and meaſured, no leſs than their magnitudes, hence it is that geometry, which treats of extended quantity, leads us into a much greater compaſs and variety of reaſoning than any other branch of methematicks. Long deductions in algebra for the moſt part are made, not ſo much by a train of reaſoning in the mind, as by an artificial kind of operation, which is built on a few very ſimple principles: But in geometry, we may build one propoſition upon another, a third upon that, and ſo on, without ever coming to a limit which we cannot exceed. The properties of the more ſimple figures can hardly be exhauſted, much leſs thoſe of the more complex ones.

It may I think be deduced from what hath been above ſaid, that mathematical evidence is an evidence *ſui generis*, not competent to any propoſition which does not expreſs a relation of things meaſurable by lines or numbers. All proper quantity may be meaſured by theſe, and improper quantities muſt be meaſured by thoſe, that are proper. SECT 4. *Coroll* 2.

There are many things capable of more and leſs, which perhaps are not capable of menſuration. Taſtes, ſmells, the ſenſations of heat and cold, beauty, pleaſure, all the affections and appetites of the mind, wiſdom, folly, and moſt kinds of probability, with many other things too tedious to enumerate, admit of degrees, but have not yet been reduced to meaſure, nor, as I apprehend, ever can be. I ſay, moſt kinds of probability, becauſe one kind of it, *viz.* the probability of chances is properly meaſurable by number, as is above obſerved.

Although attempts have been made to apply mathematical reaſoning to ſome of theſe things, and the quantity of virtue and merit in actions has been meaſured by ſimple and compound *ratio's*; yet I do not think that any real knowledge has been ſtruck out this way: it may perhaps, if diſcretely uſed, be a help to diſcourſe on theſe ſubjects, by pleaſing the imagination, and illuſtrating what is already known; but until our affections and appetites ſhall themſelves be reduced to quantity, and exact meaſures of their various degrees be aſſigned, in vain ſhall we eſſay to meaſure virtue and merit by them. This is only to ring changes upon

words, and to make a ſhew of mathematical reaſoning, without advancing one ſtep in real knowledge.

SECT. 5. *Coroll.* 3.

I apprehend the account that hath been given of the nature of proper and improper quantity may alſo throw ſome light upon the controverſy about the force of moving bodies, which long exerciſed the pens of many mathematicians, and for what I know is rather dropped than ended; to the no ſmall ſcandal of mathematics, which hath always boaſted of a degree of evidence, inconſiſtent with debates that can be brought to no iſſue.

Though philoſophers on both ſides agree with one another, and with the vulgar in this, that the force of a moving body is the ſame, while it's velocity is the ſame, is increaſed, when it's velocity is increaſed, and diminiſhed, when that is diminiſhed. But this vague notion of force, in which both ſides agree, though perhaps ſufficient for common diſcourſe, yet is not ſufficient to make it a ſubject of mathematical reaſoning: In order to that, it muſt be more accurately defined, and ſo defined, as to give us a meaſure of it, that we may underſtand what is meant by a double or a triple force. The *ratio* of one force to another cannot be perceived but by a meaſure; and that meaſure muſt be ſettled not by mathematical reaſoning, but by a definition. Let any one conſider force without relation to any other quantity, and ſee whether he can conceive one force exactly double to another; I am ſure I cannot, nor ſhall, till I ſhall be endowed with ſome new faculty; for I know nothing of force but by it's effects, and therefore can meaſure it only by it's effects. Till force then is defined, and by that definition a meaſure of it aſſigned, we fight in the dark about a vague idea, which is not ſufficiently determined to be admitted into any mathematical propoſition. And when ſuch a definition is given, the controverſy will preſently be ended.

SECT. 6. *Of the Newtonian meaſure of force.*

You ſay, the force of a body in motion is as it's velocity: either you mean to lay this down as a definition as *Newton* himſelf has done; or you mean to affirm it as a propoſition capable of proof. If you mean to lay it down as a definition, it is no more than if you ſhould ſay, I call that a double force which gives a double velocity to the ſame body, a triple force which gives a triple velocity, and ſo on in proportion. This I intirely agree to; no mathematical definition of force can be given that is more clear and ſimple, none that is more agreeable to the common uſe of the word in language. For, ſince all men agree, that the force of the body being the ſame, the velocity muſt alſo be the ſame; the force being increaſed or diminiſhed, the velocity muſt be ſo alſo, what can be more natural or proper, than to take the velocity for the meaſure of the force?

Several other things might be advanced to ſhew that this definition agrees beſt with the common popular notion of the word Force. If two bodies meet directly with a ſhock, which mutually deſtroys their motion without producing any other ſenſible effect, the vulgar would pronounce, without heſitation, that they met with equal force; and ſo they do, according to the meaſure of force above laid down: for we find by experience, that in this caſe their velocities are reciprocally as their quantities

tities of matter. In Mechanics, where by a machine two powers or weights are kept in *equilibrio*, the vulgar would reckon that theſe powers act with equal force, and ſo by this definition they do. The power of gravity being conſtant and uniform, any one would expect that it ſhould give equal degrees of force to a body in equal times, and ſo by this definition it does. So that this definition is not only clear and ſimple, but it agrees beſt with the uſe of the word Force in common language, and this I think is all that can be deſired in a definition.

But if you are not ſatisfied with laying it down as a definition, that the force of a body is as it's velocity, but will needs prove it by demonſtration or experiment; I muſt beg of you, before you take one ſtep in the proof, to let me know what you mean by force, and what by a double or a triple force. This you muſt do by a definition which contains a meaſure of force. Some primary meaſure of force muſt be taken for granted, or laid down by way of definition; otherwiſe we can never reaſon about it's quantity. And why then may you not take the velocity for the primary meaſure as well as any other? you will find none that is more ſimple, more diſtinct, or more agreeable to the common uſe of the word Force: and he that rejects one definition that has theſe properties, has equal right to reject any other. I ſay then, that it is impoſſible, by mathematical reaſoning or experiment, to prove that the force of a body is as it's velocity, without taking for granted the thing you would prove, or ſomething elſe that is no more evident than the thing to be proved.

SECT. 7. *Of the* Leibnitzian *meaſure of Force.*

Let us next hear the *Leibnitzian*, who ſays, that the force of a body is as the ſquare of it's velocity. If he lays this down as a definition, I ſhall rather agree to it, than quarrel about words, and for the future ſhall underſtand him, by a quadruple force to mean that which gives a double velocity, by nine times the force that which gives three times the velocity, and ſo on in duplicate proportion. While he keeps by his definition, it will not neceſſarily lead him into any error in Mathematics or Mechanics. For, however paradoxical his concluſions may appear, however different in words from theirs who meaſure force by the ſimple *ratio* of the velocity; they will in their meaning be the ſame: juſt as he who would call a foot twenty-four inches, without changing other meaſures of length, when he ſays a yard contains a foot and a halt, means the very ſame as you do, when you ſay a yard contains three feet.

But tho' I allow this meaſure of force to be diſtinct, and cannot charge it with falſhood, for no definition can be falſe, yet I ſay in the firſt place, it is leſs ſimple than the other; for why ſhould a duplicate *ratio* be uſed where the ſimple *ratio* will do as well? In the next place, this meaſure of force is leſs agreeable to the common uſe of the word Force, as hath been ſhewn above; and this indeed is all that the many laboured arguments and experiments, brought to overturn it, do prove. This alſo is evident, from the paradoxes into which it has led it's defenders.

We are next to conſider the pretences of the *Leibnitzian*, who will undertake to prove by demonſtration, or experiment, that force is as the

ſquare of the velocity. I aſk him firſt, what he lays down for the firſt meaſure of force? the only meaſure I remember to have been given by the philoſophers of that ſide, and which ſeems firſt of all to have led *Leibnitz* into his notion of force, is this: the height to which a body is impell'd by any impreſſed force, is, ſays he, the whole effect of that force, and therefore muſt be proportional to the cauſe: but this height is found to be as the ſquare of the velocity which the body had at the beginning of it's motion.

In this argument I apprehend that great man has been extremely unfortunate. For, 1*ſt*, whereas all proof ſhould be taken from principles that are common to both ſides, in order to prove a thing we deny, he aſſumes a principle which we think farther from the truth; namely, that the height to which the body riſes is the whole effect of the impulſe, and ought to be the whole meaſure of it. 2*dly*, His reaſoning ſerves as well againſt him as for him: for may I not plead with as good reaſon at leaſt thus? the velocity given by an impreſſed force is the whole effect of that impreſſed force; and therefore the force muſt be as the velocity. 3*dly*, Suppoſing the height to which the body is raiſed to be the meaſure of the force, this principle overturns the concluſion he would eſtabliſh by it, as well as that which he oppoſes. For, ſuppoſing the firſt velocity of the body to be ſtill the ſame; the height to which it riſes will be increaſed, if the power of gravity is diminiſhed; and diminiſhed, if the power of gravity is increaſed. Bodies deſcend ſlower at the equator, and faſter towards the poles, as is found by experiments made on pendulums. If then a body is driven upwards at the equator with a given velocity, and the ſame body is afterwards driven upwards at *Leipſick* with the ſame velocity, the height to which it riſes in the former caſe will be greater than in the latter; and therefore according to his reaſoning, it's force was greater in the former caſe; but the velocity in both was the ſame; conſequently the force is not as the ſquare of the velocity any more than as the velocity.

SECT. 8. *Reflections on this controverſy.*

Upon the whole, I cannot but think the controvertiſts on both ſides have had a very hard taſk; the one to prove, by mathematical reaſoning and experiment, what ought to be taken for granted; the other by the ſame means to prove what might be granted, making ſome allowance for impropriety of expreſſion, but can never be proved.

If ſome mathematician ſhould take it in his head to affirm, that the velocity of a body is not as the ſpace it paſſes over in a given time, but as the ſquare of that ſpace; you might bring mathematical arguments and experiments to confute him; but you would never by theſe force him to yield, if he was ingenuous in his way; becauſe you have no common principles left you to argue from, and you differ from one another, not in a mathematical propoſition, but in a mathematical definition.

Suppoſe a philoſopher has conſider'd only that meaſure of centripetal force which is proportional to the velocity generated by it in a given time, and from this meaſure deduces ſeveral propoſitions. Another

ther philoſopher in a diſtant country, who has the ſame general notion of centripetal force, takes the velocity generated by it, and the quantity of matter together, as the meaſure of it. From this he deduces ſeveral concluſions, that ſeem directly contrary to thoſe of the other. Thereupon a ſerious controverſy is begun, whether centripetal force be as the velocity, or as the velocity and quantity of matter taken together. Much mathematical and experimental duſt is raiſed; and yet neither party can ever be brought to yield; for they are both in the right, only they have been unlucky in giving the ſame name to different mathematical conceptions. Had they diſtinguiſhed theſe meaſures of centripetal force as *Newton* has done, calling the one *Vis centripetæ quantitatis acceleratrix*, the other *quantitas motrix*; all appearance of contradiction had ceaſed, and their propoſitions, which ſeem ſo contrary, had exactly tallied.

CHAP. II.

OPTICKS.

I. I have obſerved, in Mr *Baker's Microſcope made eaſy, edit.* 1743, *p.* 47, that Mr *Martin* has invented a Micrometer, to be applied to any Microſcope whatſoever. I have for ſome years made uſe of another ſort of Micrometer, which I have applied to one of Mr *Scarlet*'s Microſcopes, and placed it in the *focus* of the firſt eye-glaſs. It is a very ſmall piece of the thinneſt black ſilk, divided into very minute ſquares, and is extended on a little ring of wood or paper, in ſuch a manner, that it may conveniently be placed in the *focus* of the firſt eye-glaſs. Theſe ſquares indeed are not all of the ſame magnitude. But, as this conduces greatly to the more eaſy and convenient enumeration of them, which would be impoſſible if they were all of the ſame magnitude, ſo it is little or no hindrance in deducing the concluſions. For as often as I have counted 20, 30, or 40 of theſe ſquares, according to one line of the Micrometer, or fine ſilk. I have proceeded in counting the whole line, and let me begin to count from which end I will, I have always compared it exactly with ſome certain object placed under the Microſcope; and thus I have found the number of little ſquares to anſwer to the diameter of the object ſo juſtly, that there is very ſeldom half a ſquare too much or too little, which may very ſafely be ſlighted in ſuch an incomprehenſible ſubtility of objects.

Of the Application of a Micrometer to a Microſcope; *by* Sam Chriſtian Hollman, *Profeſſ. Pub. Ord. in the Univerſity of* Gottingen. N°. 475. p. 239, Jan. &c. 1745.

When by repeated experiments I had found, that the diameter of an object was inlarged at leaſt 27 times, I allowed the augmentation to be only 25 times, that I might be certain that the augmentations of the following glaſſes, found by my Micrometer, were not greater, but leſs than the truth, when I had them found, that N°. 1 of the ſame Microſcope magnified 250 times, and that the *animalcula ſeminalia humana*, without

without their tails, appeared hardly ſo big as a large cheeſe-mite does to the naked eye, it became evident, that 15,625,000 of theſe animalcules were contained in the ſpace of a cheeſe-mite. And yet I have obſerved much ſmaller animalcules than theſe, in an infuſion of common pepper, or even of common hay, after it had ſtood for ſome days. By the uſe of the ſame Micrometer, I alſo found two ways of determining the quantity of ſeminal animalcules in the milt of a fiſh, more accurately than had been done by *Leuwenhoeck*. I ſhall only add at preſent, that one cubical decimal line of a Rheniſh foot, in the milt of a carp, contained above 244,140,625 ſeminal animalcules; and that the whole milt of a carp, weighing not quite 2 Norrinberg pounds, which had 1084 grains, made about 2080 cubical decimal lines, as I found by a hydroſtatical experiment. That whole milt therefore contained above 507,812,500,000 ſeminal animalcules. But if we ſuppoſe the half of this milt only to conſiſt of animalcules, and the other half to be a fluid in which they live, which will eaſily be allowed to exceed the truth, by all thoſe who have obſerved how ſmall a proportion of fluid there is in the ſeed of this fiſh, before it has been diluted by water; there will, even upon this ſuppoſition, be more than 253,906,250,000 living animalcules in the ſeed of a carp, weighing leſs than 2 Norrinberg pounds; which tho' it is beyond the reach of our imagination, does not exceed the power of the infinite creator.

Gottingen, Oct. 15, 1744.

Of fallacious Viſion thro' compound Microſcopes; by Philip Fredrick Gmelin, *Med. Licent. Wurterbergenſis.* N°. 476. p. 386, April &c. 1745. *Preſented* May 9. 1745.

II. Being informed by a friend, that if a common ſeal was applied to the *focus* of a compound Microſcope, or optical tube, which has 2 or 3 convex, or plano-convex glaſſes, that part which is cut the deepeſt in, would appear very convex, and ſo on the contrary; and that ſometimes, but very ſeldom, it would appear in the ſame ſtate, as to the naked eye, I was deſirous to make the obſervation myſelf; and found it conſtantly to happen, as my friend told me. I thought the experiment worthy of being farther proſecuted: and accordingly, on the 16th of laſt *April*, the morning not being very clear, but in a pretty light chamber, I viewed a watch hanging againſt a plain wall, thro' the left part of an optical tube: the whole of it appeared concave, and fixed into the wall. I alſo obſerved ſome flies, that were running about the wall, and they appeared in like manner. I alſo viewed a ſmall globe of a Thermometer filled with red ſpirit: and this alſo ſeemed hollow, and fixed within the frame. I found the ſame to happen with the raiſed parts of garments of all colours, and with the brazen protuberances of a ſmall cabinet; all which appeared concave, and deeply ſunk into the cloth and wood. I alſo viewed a ſmall ſtags head, cut in wood, and hanging horizontally on the wall; this alſo appeared concave, and fixed into the wall.

After this I obſerved a ball of one of Fahrenheit's Thermometors, full of quickſilver: but it did not change it's natural convexity; nor did the empty glaſs ball of the inverted Thermometer, hanging againſt the wall, tho'

tho' the lower ball of the ſame filled with red ſpirit, and that alſo of Fahrenheit's filled with ſpirit loſt their convexity. Hence I preſently concluded, that white or ſhining uncoloured bodies appeared under the *focus* of this tube in the ſame manner as they appear to the naked eye, at the ſame time I muſt fairly acknowledge, that an aſſiſting friend has ſometimes made obſervations directly oppoſite to mine in the ſame circumſtances: nay, in a darker day I myſelf have found my obſervations quite contrary to thoſe which I had made the day before. Hence, tho' the obſervation with the ſeal held conſtantly the ſame, I imagined there muſt be ſome particular circumſtances hitherto unobſerved, in which theſe objects appeared thus perverted. I therefore endeavoured to diſcover ſome certain laws, according to which theſe perverted objects always appeared when expoſed to theſe *foci*, and ſome others, according to which they conſtantly appeared as when they were expoſed to the naked eye. After various experiments I partly obtained my end.

As often as I viewed any object, riſing upon a plane, of what colour ſoever, provided it was neither white nor ſhining, with the eye and optical tube directly oppoſite to it, the elated parts appeared depreſſed, and the depreſſed parts elated, as it happened in the ſeal, as often as I held the tube perpendicularly, and brought it in ſuch a manner, that it's whole ſurface almoſt covered the laſt glaſs orb of the tube; and in like manner it happened under the compound Microſcope. But as often as I viewed any of the other objects depending perpendicularly from a perpendicular plane, in ſuch a manner, that the tube was ſupported in a horizontal ſituation directly oppoſite to it, the ſame always happened, and the appearance was not altered, when the object hung obliquely or even horizontally. I was mightily delighted with the obſervation of a tobacco pipe, which had a porcellane bowl of a ſnowy whiteneſs, and a tube of horn almoſt black, and hung obliquely from a horn; the bowl preſerved it's natural convexity, and the tube was deeply ſunk, and ſeemed to be almoſt immerſed in the wall. I alſo obſerved, that when I placed the watch horizontally on a horizontal plane, and then looked on it perpendicularly, near the window, it no longer appeared ſo depreſſed, and ſurrounded with a ſhady ring; whence I began to ſuſpect, that all theſe fallacies were owing to ſhade, juſt as Painters can elevate or depreſs a figure by making the ground lighter or deeper. Thus when the raiſed object was ſo placed between the windows, that it might be illuminated on all ſides, it did not change it's convexity. But at laſt I diſcovered a method of making objects always appear with their natural convexity. If any object hung againſt a wall, or was contiguous to it in any ſituation whatſoever, I viewed ſidewiſe in ſuch a manner as not to oppoſe the tube directly againſt it, but below the eminence near the plain at ſome diſtance. By theſe means the protuberances of the cabinet, and other objects, always appeared to me with their true natural convexity. With regard to the ſeal, I held it in ſuch a manner, that

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the whole circumference was perpendicular, or rather a little inclined. Then I applied the lower rim of the tube exactly to the upper margin of the diſk of the ſeal, ſo that the tube formed an obtuſe angle with the ſeal; then carefully preſerving the ſame ſituation, I very gently moved the tube from the rim of the ſeal upon it's face; and thus I always ſaw the ſeal with it's true natural face. But why all theſe things happen exactly after this manner, I do not pretend to determine, nor why white, or uncoloured tranſparent ſhining bodies, riſing in any manner above any plane, afford an exception from this rule of Viſion, and do not appear depreſſed when viewed after the method above mentioned.

CHAP. III.

ASTRONOMY.

A letter to George *Earl of* Macclesfield, *concerning an* apparent Motion *obſerved in ſome of the* fixed Stars; *by the Rev.* James Bradley, *D.D. Aſtron. Reg. F. R. S.* N°. 485. p. 1. Jan. 1747-8. *Read* Jan. 7, 1747.

I. THE great exactneſs, with which inſtruments are now conſtructed, hath enabled the Aſtronomers of the preſent age to diſcover ſeveral changes in the poſitions of the heavenly bodies; which, by reaſon of their *ſmallneſs*, had eſcaped the notice of their predeceſſors. And altho' the cauſes of ſuch motions have always ſubſiſted, yet philoſophers had not ſo fully conſider'd, what the effects of thoſe known cauſes would be, as to demonſtrate *a priori* the *phænomena* they might produce; ſo that theory itſelf is here, as well as in many other caſes, indebted to practice, for the diſcovery of ſome of it's moſt elegant deductions. This points out to us the great advantage of cultivating *this*, as well as every other branch of natural knowledge, by a regular ſeries of obſervations and experiments.

The progreſs of Aſtronomy indeed has always been found, to have ſo great a dependence upon accurate obſervations, that, till ſuch were made, it advanced but ſlowly: for the firſt conſiderable improvements that it received, in point of theory, were owing to the renowned *Tycho Brahè*; who far exceeding thoſe that had gone before him, in the exactneſs of his obſervations, enabled the ſagacious *Kepler* to find out ſome of the principal laws, relating to the motion of the heavenly bodies. The invention of teleſcopes and pendulum-clocks affording proper means of ſtill farther improving the *praxis* of Aſtronomy; and theſe being alſo ſoon ſucceeded by the wonderful diſcoveries made by our great *Newton*, as to it's theory; the ſcience, in both reſpects, had acquired ſuch extraordinary advancement, that future ages ſeemed to have little room left, for making any great improvements. But, in fact, we find the caſe to be very different; for, as we advance in the means of making more nice inquiries, new points generally offer themſelves, that demand our attention. The ſubject of my preſent letter

to

to your Lordſhip, is a proof of the truth of this remark: for, as ſoon as I had diſcovered the cauſe, and ſettled the laws of the aberrations of the fixed ſtars, ariſing from the motion of light, *&c.* * my attention was again excited by another *new phænomenon*, *viz.* an annual change of declination in ſome of the fixed ſtars; which appeared to be ſenſibly *greater* about that time, than a preceſſion of the equinoctial points of 50″ in a year would have occaſioned. The quantity of the difference, tho' ſmall in itſelf, was rendered perceptible, thro' the exactneſs of my inſtrument, even in the firſt year of my obſervations; but being then at a loſs to gueſs, from what cauſe that greater change of declination proceeded, I endeavoured to allow for it in my computations, by making uſe of the *obſerved* annual difference, as mentioned in the ſame Paper.

From *that* time to the preſent, I have continued to make obſervations at *Wanſted*, as opportunity offered, with a view of diſcovering the laws and cauſe of this *phænomenon*: for, by the favour of *Matthew Wymondeſold*, Eſq; my inſtrument has remained, where it was firſt erected; ſo that I have been able, without any interruption, which the removal of it to another place would have occaſioned, to proceed on with my intended ſeries of obſervations, for the ſpace of twenty years: a term ſomewhat exceeding the whole period of the changes, that happen in this *phænomenon*.

When I ſhall mention the *ſmall* quantity of the deviation, which the ſtars are ſubject to, from the cauſe that I have been ſo long ſearching after; I am apprehenſive, that I may incur the cenſure of ſome perſons, for having ſpent ſo much time in the purſuit of ſuch a ſeeming trifle: but the candid lovers of ſcience will, I hope, make due allowance for that natural ardour, with which the mind is urged on towards the diſcovery of truths, in themſelves perhaps of *ſmall* moment, were it not that they tend to illuſtrate others of greater uſe.

The apparent motions of the heavenly bodies are ſo complicated, and affected by ſuch a variety of cauſes; that in many caſes it is extremely difficult to aſſign to each it's due ſhare of influence; or diſtinctly to point out, what part of the motion is the effect of one cauſe, and what of another: and whilſt the joint effects of *all* are only attended to, great irregularities and ſeeming inconſiſtencies frequently occur; whereas, when we are able to allot to each particular cauſe it's proper effect, harmony and uniformity uſually enſue.

Such ſeeming irregularities being alſo blended with the unavoidable errors, to which aſtronomical obſervations muſt be always liable, as well from the imperfection of our ſenſes, as of the inſtruments that we uſe, have often very much perplex'd thoſe, who have attempted to ſolve the *phænomena*: and till means are diſcovered, whereby we can ſeparate and diſtinguiſh the *particular* part of the whole motion, that is owing to each

* See Vol. vi. Part I. Chap. iii. Sect. 5.

reſpective cauſe, it will be impoſſible, to be well aſſured of the truth of any ſolution. For theſe reaſons, we generally find, that the more exact the inſtruments are, that we uſe, and the more regular the ſeries of obſervations is, that we take; the ſooner we are enabled to diſcover the cauſe of any new *phænomenon*. For when we can be well aſſured of the limits, wherein the errors of the obſervations are contain'd; and have reduced them within as narrow bounds as poſſible, by the perfection of our inſtruments; we need not heſitate to aſcribe ſuch apparent changes, as manifeſtly exceed thoſe limits, to ſome other cauſes. Upon theſe accounts it is incumbent upon the *practical* Aſtronomer, to ſet out at firſt with the examination of the correctneſs of his inſtruments; and to be aſſured that they are ſufficiently exact for the uſe he intends to make of them: or at leaſt he ſhould know, within what limits their errors are confined.

This practice has, in an eminent manner, been lately recommended by your Lordſhip's noble example; who having, out of a ſingular regard for the ſcience of Aſtronomy, erected an obſervatory, and furniſhed it with as complete an *apparatus* of inſtruments, as our beſt artiſts could contrive; would not fully rely on their exactneſs, till their diviſions had undergone the ſtricteſt re-examination: whereby they are probably now render'd as perfect in their kind, as any extant, or as human ſkill can at preſent produce.

The lovers of *this* ſcience in general, cannot but acknowledge their obligations to your Lordſhip on this account; but I find myſelf more particularly bound to do it; ſince, by means of your moſt accurate obſervations, I have been enabled to ſettle ſome *principal elements*; which I could not at preſent otherwiſe have done, for want of an inſtrument at the Royal Obſervatory, *proper* for that purpoſe: for the large *mural quadrant*, which is there fixed to obſerve objects lying ſouthward of the zenith, however *perfect* an inſtrument it may be in itſelf, is not alone ſufficient to determine, with proper exactneſs, either the *latitude* of the Obſervatory, or the quantity of refraction correſponding to different altitudes: for it being too heavy to be conveniently removed; and the room wherein it is placed, being too ſmall to admit of it's being turned to the oppoſite ſide of the wall, whereon it now hangs; I cannot, by *actual* obſervations of the circumpolar ſtars, ſettle thoſe neceſſary points; and therefore have endeavoured to do it, by comparing my own with your Lordſhip's obſervations: and until this defect in the *apparatus* belonging to the Royal Obſervatory be removed, we muſt be indebted to your Lordſhip, for the knowledge of it's true ſituation.

A mind intent upon the purſuit of any kind of knowledge, will always be agreeably entertained, with what can ſupply the moſt proper means of attaining it: ſuch, to the practical Aſtronomer, are exact and well-contriv'd inſtruments; and I reflect with pleaſure on the opportunities I have enjoyed, of cultivating an acquaintance and friendſhip with the perſon, that, of all others, has moſt contributed to their improvement.

ment. For I am ſenſible, that if my own endeavonrs have, in any reſpect, been effectual to the advancement of Aſtronomy; it has principally been owing to the advice and aſſiſtance given me by Mr *George Graham*; whoſe great ſkill and judgment in mechanicks, join'd with a complete and practical knowlenge of the uſes of aſtronomical inſtruments, enable him to contrive and execute them in the moſt perfect manner.

The Gentlemen of the *Royal Academy* of *Sciences*, to whom we are ſo highly obliged for their exact admeaſurement of the quantity of a degree under the arctic circle, have already given the world very convincing proofs of *his* care and abillities in thoſe reſpects; and the particular delineation, which they have lately publiſhed, of the ſeveral parts of the *ſector*, which he made for them, hath now rendered it needleſs, to enter upon any minute deſcription of *mine* at *Wanſted*; both being conſtructed upon the ſame principles, and differing in their component parts, chiefly on account of the different purpoſes, for which they were intended.

As mine was originally deſigned to take only the *differences* of the zenith diſtances of ſtars, in the various ſeaſons of the year, without any view of diſcovering their *true* places; I had no occaſion to know exactly, what point on the limb correſponded to the *true* zenith: and therefore no proviſion was made in my ſector, for the changing of it's ſituation for that purpoſe. Neither was it neceſſary that the diviſions or points on the arc ſhould be ſet off, with the utmoſt accuracy, equidiſtant from each other; becauſe, when I obſerve any particular ſtar, the ſame ſpot or point being firſt biſected by the plumb-line, and then the ſcrew of the Micrometer turn'd until the ſtar appears upon the middle of the wire, that is fixed in the common focus of the glaſſes of the Teleſcope; I can thereby collect, how far the ſtar is from that given point at the time of obſervation: and afterwards, by comparing together the ſeveral obſervations that are made of it, I am able to diſcover what apparent change has happen'd. The quantity of the viſible alteration, in the poſition of the ſtars, being expreſſed by revolutions and parts of a revolution, of the ſcrew of the Micrometer; I endeavoured to determine, with great care, the true angle anſwering thereto: and after various trials, I thoroughly ſatisfied myſelf, both of the equality of the threads of the ſcrew, and of the preciſe number of ſeconds correſponding to them.

But altho' theſe points could be ſettled with great certainty, I was nevertheleſs obliged to make one ſuppoſition; which perhaps to ſome perſons may ſeem of too great moment in the preſent inquiry, to be admitted without an evident proof from facts and experiments. For I *ſuppoſe*, that the line of collimation of my Teleſcope has invariably preſerved the ſame direction, with reſpect to the diviſions upon the arc, during the whole courſe of my obſervations, And indeed it was on account of the objections, which might have been raiſed againſt ſuch a *poſtulate*, that I thought it neceſſary, to continue my ſeries of obſervations for ſo many

years, before I publiſhed the concluſions, which I ſhall at preſent endeavour to draw from them.

Whoever compares the reſult of the ſeveral trials, that have been made by the gentlemen of the *Academy* of *Sciences*, for determining the zenith point of their ſector, ſince their return from the north; will, I preſume, allow that *mine* is not an unreaſonable or precarious *ſuppoſition*: ſince it is evident, from their obſervations, that the line of collimation of that inſtrument underwent no ſenſible change in it's direction, during the ſpace of more than a whole year; altho' it was ſeveral times taken down, and ſet up again in different and remote places; whereas *mine* hath always remained ſuſpended in the ſame place.

But beſides ſuch a ſtrong argument for the probabillity of the truth of my *ſuppoſition*, I have the ſatisfaction of finding it *actually* verified by the obſervations themſelves; which plainly prove, that at the end of the full period of the deviations which I am going to mention, the ſtars are found to have the ſame poſitions by the inſtrument, as they ought to *have*, ſuppoſing the line of collimation to have continued unaltered from the time when I firſt began to obſerve.

I have already taken notice, in what manner this *phænomenon* diſcover'd itſelf to me at the end of my firſt year's obſervations, *viz.* by a *greater* apparent change of declination in the ſtars near the equinoctial colure, than could ariſe from a preceſſion of 50″ in a year; the mean quantity now uſually allowed by Aſtronomers. But there appearing at the ſame time, an effect of a quite contrary nature, in ſome ſtars near the ſolſtitial colure, which ſeem'd to alter their declination *leſs* than a preceſſion of 50″ required; I was thereby convinced, that all the *phænomena*, in the different ſtars, could not be accounted for, merely by ſuppoſing, that I had aſſumed a wrong quantity for the preceſſion of the equinoctial points.

At firſt, I had a ſuſpicion, that ſome of theſe ſmall apparent alterations in the places of the ſtars, might poſſibly be occaſioned by a change, in the materials, or in the poſition of the parts of my ſector: but, upon conſidering how firmly the arc, on which the diviſions or points are made, is faſtened to the plate, wherein the wire is fixed that lies in the focus of the object-glaſs; I ſaw no reaſon to apprehend, that any change could have happened in the poſition of that wire and thoſe points. The ſuſpenſion therefore of the plummet being the moſt likely cauſe, from whence I conceived any uncertainty could ariſe; and the wire of which had been broken three or four times in the firſt year of my obſervations: I attempted to examine, whether part of the 'forementiond apparent motions might not have been owing, to the different plumb-lines that had been made uſe of. In order to determine this, I adjuſted a particular point of the arc to the plumb-line, with all the exactneſs I could; and then taking off the old wire, I immediately hung on another, with which the ſame ſpot was again compared. I repeated the experiment three or four times, and thereby fully ſatisfied myſelf, that no ſenſible

ſible error could ariſe from the uſe of different plumb-lines; ſince the various adjuſtments of the ſame point agreed with each other, within leſs than half a ſecond.

Having then, from ſuch trials, ſufficient reaſon to conclude, that theſe *ſecond* unexpected deviations of the ſtars, were not owing to any imperfection of my inſtrument; after I had ſettled the laws of the aberrations ariſing from the motion of light, *&c.* I judged it proper to continue my obſervations of the ſame ſtars; hoping that, by a regular and longer ſeries of them, carried on thro' ſeveral ſucceeding years, I might, at length, be enabled to diſcover the *real* cauſe of ſuch apparent inconſiſtencies.

As I reſided chiefly at *Wanſted*, after my ſector was erected there in 1727, till the beginning of *May* 1732, when I removed from thence to *Oxford*: I had, during my abode at *Wanſted*, frequent opportunities of repeating my obſervations; and thereby diſcovered ſo many particulars relating to theſe *phænomena*, that I began to gueſs what was the real cauſe of them.

It appeared from my obſervations, that, during this interval of time, ſome of the ſtars near the ſolſtitial colure, had changed their declinations 9″ or 10″ *leſs*, than a preceſſion of 50″ would have produced; and, at the ſame time, that, others near the equinoctial colure, had altered theirs about the ſame quantity *more*, than a like preceſſion would have occaſioned: the north pole of the equator ſeeming to have approached the ſtars, which come to the meridian with the ſun, about the vernal equinox and the winter ſolſtice; and to have receded from thoſe, which come to the meridian with the ſun, about the autumnal equinox and the ſummer ſolſtice.

When I conſider'd theſe circumſtances, and the ſituation of the aſcending node of the moon's orbit, at the time when I firſt began my obſervations; I ſuſpected, that the moon's action upon the equatorial parts of the earth might produce theſe effects: for, if the preceſſion of the equinox be, according to Sir *I. Newton*'s principles, cauſed by the actions of the ſun and moon upon thoſe parts; the plane of the moon's orbit being at *one* time, above ten degrees *more* inclined to the plane of the equator, than at *another*; it was reaſonable to conclude, that the part of the whole annual preceſſion, which ariſes from her action, would in different years be varied in it's quantity; whereas the plane of the ecliptic, wherein the ſun appears, keeping always nearly the ſame inclination to the equator; *that* part of the preceſſion, which is owing to the ſun's action, may be the ſame every year: and from hence it would follow, that, altho' the *mean* annual preceſſion, proceeding from the joint actions of the ſun and moon, were 50″; yet the *apparent* annual preceſſion might ſometimes exceed, and ſometimes fall ſhort, of that mean quantity, according to the various ſituations of the nodes of the moon's orbit.

In 1727, when my inſtrument was firſt ſet up, the moon's aſcending node was near the beginning of *aries*; and conſequently, her orbit was as much inclined to the equator, as it can at any time be; and then the *ap-*

parent

parent annual preceſſion was found, by my firſt year's obſervations, to be greater than the *mean:* which proved, that the ſtars near the equinoctial colure, whoſe declinations are moſt of all affected by the preceſſion, had changed *theirs*, above $\frac{1}{10}$ more than a preceſſion of 50″ would have cauſed. The ſucceeding years obſervations proved the ſame thing; and in 3 or 4 years time the difference became ſo conſiderable, as to leave no room to ſuſpect, that it was owing to any imperfection, either of the inſtrument or obſervations.

But ſome of the ſtars, which I had obſerved, that were near the ſolſtitial colure, having appeared to move, during the ſame time, in a manner contrary to what they ought to have done, by an increaſe in the preceſſion; and the deviations in them being as remarkable as in the others, I perceived that ſomething more, than a meer change in the quantity of the preceſſion, would be requiſite to ſolve this part of the *phænomenon*. Upon comparing my obſervations of ſtars near the ſolſtitial colure, that were almoſt oppoſite to each other in right aſcenſion, I found, that they were equally affected by this cauſe; for whilſt γ *draconis* appeared to have moved northward, the ſmall ſtar, which is the 35th *Camelopardali Hevel.* in the *Britiſh* catalogue, ſeem'd to have gone as much towards the ſouth: which ſhew'd, that this apparent motion, in both thoſe ſtars, might proceed from a nutation in the earth's axis; whereas the compariſon of my obſervations of the ſame ſtars, *formerly* enabled me to draw a different concluſion, with reſpect to the cauſe of the annual aberrations ariſing from the motion of light. For the apparent alteration in γ *draconis*, from *that* cauſe, being as great again as in the other ſmall ſtar, proved, that *that phænomenon* did not proceed from a *nutation* of the earth's axis; as, on the contrary, *this* may. Upon making the like compariſon between the obſervations of other ſtars, that lie nearly oppoſite in right aſcenſion, whatever their ſituations were with reſpect to the cardinal points of the equator, it appeared, that their change of declination was nearly equal, but contrary; and ſuch as a nutation or motion of the earth's axis would effect.

The moon's aſcending node being got back towards the beginning of *capricorn* in 1732, the ſtars near the equinoctial colure appeared, about that time, to change their declinations no more, than a preceſſion of 50″ required; whilſt ſome of thoſe near the ſolſtitial colure altered *theirs* above 2″ in a year leſs, than they ought. Soon after, I perceived the annual change of declination of the former to be diminiſhed, ſo as to become *leſs* than 50″ of preceſſion would cauſe; and it continued to diminiſh till 1736, when the moon's aſcending node was about the beginning of *libra*, and her orbit had the *leaſt* inclination to the equator. But by this time, ſome of the ſtars near the ſolſtitial colure had altered their declinations 18″ *leſs*, ſince 1727, than they ought to have done from a preceſſion of 50″. For γ *draconis*, which in thoſe 9 years ſhould have gone about 8″ more *ſoutherly*, was obſerved in 1736, to appear 10″ more *northerly*, than it did in 1727.

As

As this appearance in *γ draconis*, indicated a diminution of the inclination of the earth's axis to the plane of the ecliptic; and as ſeveral Aſtronomers have ſuppoſed *that* inclination to diminiſh regularly: if this *phænomenon* depended upon ſuch a cauſe, and amounted to 18″ in 9 years, the obliquity of the ecliptic would, at that rate, alter a whole minute in 30 years; which is much faſter than any obſervations, *before* made, would allow. I had reaſon therefore to think, that *ſome part* of this motion at the leaſt, if not the *whole*, was owing to the moon's action upon the equatorial parts of the earth; which I conceived, might cauſe a libratory motion of the earth's axis. But as I was unable to judge, from only 9 years obſervations, whether the axis would entirely recover the ſame poſition, that it had in 1727. I found it neceſſary to continue my obſervations thro' a whole period of the moon's nodes; at the end of which I had the ſatisfaction to ſee, that the ſtars returned into the ſame poſitions again; as if there had been no alteration at all in the inclination of the earth's axis: which fully convinced me, that I had gueſſed rightly as to the cauſe of the *phænomena*. This circumſtance proves likewiſe, that if there be a gradual diminution of the obliquity of the ecliptic; it does not ariſe only from an alteration in the poſition of the earth's axis, but rather from ſome change in the plane of the ecliptic itſelf: becauſe the ſtars, at the end of the period of the moon's nodes, appeared in the ſame places, with reſpect to the equator, as they ought to have done, if the earth's axis had retained the ſame inclination to an invariable plane.

During the courſe of my obſervations, as Mr *Machin* was employed in conſidering the theory of gravity, and it's conſequences, with regard to the celeſtial motions; I acquainted him with the *phænomena* that I had obſerved: and at the ſame time mentioned, *what* I ſuſpected to be the cauſe of them. He ſoon after ſent me a table, containing the quantity of the annual preceſſion in the various poſitions of the moon's nodes, as alſo the correſponding nutations of the earth's axis; which was computed upon the *ſuppoſition*, that the *mean* annual preceſſion is 50″, and that the whole is governed by the pole of the moon's orbit only: and therefore he imagined, that the numbers in the table would be too *large*; as in fact they were found to be. But it appeared, that the changes which I had obſerved, both in the annual preceſſion and nutation, kept the ſame law, as to increaſing and decreaſing, with the numbers of his table. Thoſe were calculated upon the ſuppoſition, that the pole of the equator, during a period of the moon's nodes, moved round in the periphery of a little circle, whoſe center was 23° 29′ diſtant frrm the pole of the ecliptic; having itſelf alſo an angular motion of 50″ in a year, about the ſame pole: the north pole of the equator was conceived to be in *that* part of the ſmall circle, which is fartheſt from the N. pole of the ecliptic, at the time when the moon's aſcending node is in the beginning of *aries*: and in the oppoſite point of it, when the ſame node is in *libra*.

Such

Such an hypotheſis will account for an acceleration and retardation of the annual preceſſion; as alſo for a nutation of the earth's axis: and if the diameter of the little circle be ſuppoſed equal to 18″; which is the whole quantity of the nutation, as collected from my obſervations of *γ draconis:* then all the *phænomena* in the ſeveral ſtars which I obſerved, will be very nearly ſolved by it.

Fig. 1. Let *P* repreſent the mean place of the pole of the equator, about which point, as a center, ſuppoſe the true pole to move in the circle *A B C D*, whoſe diameter is 18″. Let *E* be the pole of the ecliptic, and *E P* be equal to the mean diſtance between the poles of the equator and ecliptic; and ſuppoſe the true pole of the equator to be at *A*, when the moon's aſcending node is in the beginning of *aries*; and at *B*, when the node gets back to *capricorn*; and at *C*, when the ſame node is in *libra:* at which time the N. pole of the equator being nearer the N. pole of the ecliptic, by the whole diameter of the little circle *A C* equal to 18″; the obliquity of the ecliptic will then be ſo much *leſs* than it was, when the moon's aſcending node was in *aries*. The point *P* is ſuppoſed to move round *E*, with an equal retrograde motion, anſwerable to the mean preceſſion ariſing from the joint actions of the ſun and moon: while the true pole of the equator moves round *P*, in the circumference *A B C D*, with a retrograde motion likewiſe, in a period of the moon's nodes, or of eighteen years, and ſeven months. By this means, when the moon's aſcending node is in *aries*, and the true pole of the equator at *A*, is moving from *A* towards *B:* it will approach the ſtars, that come to the meridian with the ſun about the vernal equinox; and recede from thoſe that come with the ſun near the autumnal equinox, *faſter* than the *mean* pole *P* does. So that, while the moon's node goes back from *aries* to *capricorn*, the *apparent* preceſſion will ſeem ſo much *greater* than the *mean*; as to cauſe the ſtars, that lie in the equinoctial colure, to have altered their declination 9″, in about 4 years and 8 months, *more* than the mean preceſſion would do: and in the ſame time, the N. pole of the equator will ſeem to have approached the ſtars, that come to the meridian with the ſun at our winter ſolſtice, about 9″; and to have receded as *much* from thoſe, that come with the ſun at the ſummer-ſolſtice.

Thus the *phænomena* before recited are in general conformable to this
Fig. 2. hypotheſis. But to be more particular; let *S* be the place of a ſtar, *P S* the circle of declination paſſing thro' it, repreſenting it's diſtance from the mean pole, and *γ P S* it's mean right aſcenſion. Then if *O* and *R* be the points, where the circle of declination cuts the little circle *A B C D*; the *true* pole will be neareſt that ſtar at *O*, and fartheſt from it at *R*; the whole difference amounting to 18″, or to the diameter of the little circle. As the true pole of the equator is ſuppoſed to be at *A*, when the moon's aſcending node is in *aries*; and at *B*, when that node gets beck to *capricorn*; and the angular motion of the true pole about *P*, is likewiſe ſuppoſed equal to that of the moon's node about *E*, or the pole of the ecliptic: ſince, in theſe caſes, the true pole of the equator

is

is 90 degrees before the moon's aſcending node, it muſt be ſo in all others.

When the true pole is at *A*, it will be at the ſame diſtance from the ſtars that lie in the equinoctial colure, as the mean pole *P* is, for I neglect at preſent the caſe of ſuch ſtars as are *very* near the pole of the equator; and as the true pole recedes back from *A* towards *B*, it will approach the ſtars, that lie in that part of the colure repreſented by *P* ♈; and recede from thoſe, that lie in *P* ♎; not indeed with an *equable* motion; but in the *ratio* of the *ſine* of the diſtance of the moon's node from the beginning of *aries*. For if the node be ſuppoſed to have gone backwards from *aries* 30°, or to the beginning of *piſces*; the point, which repreſents the place of the true pole, will in the mean time, have moved in the little circle, thro' an arc, as *AO*, of 30° likewiſe: and would therefore in effect have approached the ſtars that lie in the equinoctial colure *P* ♈, and have receded from thoſe that lie in *P* ♎, $4'' \frac{1}{2}$; which is the *ſine* of 30° to the *radius AP*. For if a perpendicular fall from *O* upon *PA*, it may be conceived as part of a great circle, paſſing thro' the true pole and any ſtar lying in the equinoctial colure. Now the ſame proportion, that holds in theſe ſtars, will obtain likewiſe in all others; and from hence we may collect a general rule, for finding how much nearer or farther, any particular ſtar is, to or from, the *mean* pole, in any given poſition of the moon's node.

For, if *from the R. aſcenſion of the ſtar, we ſubſtract the diſtance of the moon's aſcending node from aries; then the radius will be to the ſine of the remainder, as* 9″, *is to the number of ſeconds, that the ſtar is nearer to, or farther from the true, than the mean pole.* When that remainder is *leſs* than 180°, the ſtar is *nearer* to the true, than to the mean pole; and the contrary, when it is *greater* than 180°.

This motion of the *true* pole, about the *mean* at *P*, will alſo produce a change in the R. aſcenſions of the ſtars, and in the places of the equinoctial points; as well as in the obliquity of the ecliptic: and the quantity of the equations, in either of theſe caſes, may be eaſily computed for any given poſition of the moon's nodes. But as it may be needleſs, to dwell longer on the explication of the hypotheſis; I ſhall now proceed to ſhew it's correſpondency with the *phænomena*, relating to the alterations of the polar diſtances of ſome of the ſtars which I have obſerved: by laying before your Lordſhip the obſervations themſelves, together with the computations that are neceſſary; in order to form a right judgment about the cauſe of theſe appearances.

I have endeavoured to find the exact quantity of the *mean* preceſſion of the equinoctial points, by comparing my own obſervations made at *Greenwich*, with thoſe of *Tycho Brahè* and others, which I judged to be moſt proper for that purpoſe. But as many of the ſtars, which I compared, gave a different quantity; I ſhall aſſume the mean reſult; which gives a preceſſion of one degree in ſeventy-one years and an half: this agreeing very well likewiſe with my obſervations that were taken at

Wanſted. The numbers in the following tables, which expreſs the change of declination in each ſtar, are computed upon the ſuppoſition, that the *mean* obliquity of the ecliptic was 23°. 28′. 30″, and that it continued the *ſame*, during the whole courſe of my obſervations. And as the moon's aſcending node was in the beginning of *aries* about *March* 27th 1727, I have reduced the place of each ſtar to *that* time; by allowing the proper change of declination from that day, to the day of each reſpective obſervation.

It being alſo neceſſary to make an allowance for the *aberrations* of light; I have again examined my obſervations, that were moſt proper to determine the tranſverſe axis of the ellipſis, which each ſtar ſeems to deſcribe; and have found it to be neareſt to 40″; which number I therefore make uſe of in the following computations.

The diviſions or points upon the limb of my ſector are placed five minutes of a degree from each other; and are numbered ſo, as to ſhew the polar diſtances nearly; the *true* polar diſtance exceeding that, which is ſhewn by the inſtrument, about 1′. 35″. When I firſt began to obſerve, I generally made uſe of *that* point on the limb, which was neareſt to the ſtar's polar diſtance, without regarding whether it was more northerly, or more ſoutherly than the ſtar: but as it ſometimes happened, that the original point, with which I at firſt compared the ſtar, became, in proceſs of time, pretty remote from it; I afterwards brought the plummet to another point, that was nearer to it; and carefully examined, what number of revolutions of the ſcrew of the Micrometer, *&c.* correſponded to the diſtance between the different points, that I had made uſe of: by which means I was able to reduce all the obſervations of the ſame ſtar to the ſame point, without ſuppoſing the ſeveral diviſions to be accurately 5′ aſunder.

I have expreſſed the diſtance of each ſtar from the point of the arc, with which it was compared, in *ſeconds* of a degree and *tenth parts* of a ſecond, exactly as it was collected from the obſervations; altho' I am ſenſible, that the obſervations themſelves are liable to an error of more than a *whole* ſecond; becauſe I meet with ſome, that have been made within two or three days of each other, that differ 2″, even when they are not marked as *defective* in any reſpect.

It would be too tedious, to ſet down the whole number of the obſervations that I have made; and therefore I ſhall give only enough of them, to ſhew their correſpondency with the 'forementioned hypotheſis in the ſeveral years, wherein any were made of the ſtars here recited. When *ſeveral* obſervations have been taken of the ſame ſtar, within a few days of each other; I have either ſet down the mean reſult, or *that* obſervation which beſt agreed with it. I have likewiſe commonly choſen thoſe, that were made near the ſame ſeaſon of the year, in ſuch ſtars as gave me the opportunity of making that choice; particularly in *γ draconis*, which was generally obſerved about the end of *Auguſt* or the beginning of *September*; *that* being the uſual time, when I went to

Wanſted

Wanſted on purpoſe to obſerve both *that*, and alſo ſome of the ſtars in the *great Bear*. But the weather proving cloudy at that ſeaſon in 1744, prevented my making a ſingle obſervation, either of γ *draconis*, or any other ſtar, while I was there; which is the cauſe of one vacancy in a ſeries of 20 ſucceeding years, wherein that particular ſtar had been obſerved. Such ſtars, as were either not viſible in the day-time, towards the beginning of *September*, or came at ſuch hours in the night, as would have incommoded the family of the houſe wherein the inſtrument is fixed, were but ſeldom obſerved, after I went to reſide at *Oxford*: which is the reaſon, why the ſeries of obſervations of *thoſe* is ſo imperfect, as ſometimes to leave a chaſm for ſeveral years together. But notwithſtanding this, I doubt not, but upon the whole they will be found ſufficient, to ſatisfy your Lordſhip of the general correſpondency between the *hypotheſis* and the *phænomena*, in the ſeveral ſtars; however different their ſituations are, with reſpect to the cardinal points of the equator.

As I made more obſervations of γ *draconis* than of any other ſtar; and it being likewiſe very near the zenith of *Wanſted*; I will begin with the recital of ſome of them. The point upon the limb, with which this ſtar was compared, was 38°. 25′ from the N. pole of the equator, according to the numbers of the arc of my *ſector*. The firſt column, in the following table, ſhews the year and the day of the month, when the obſervations were made; the next gives the number of *ſeconds*, that the ſtar was found to be *S.* of 38°. 25′: the third contains the alterations of the polar diſtance, which the *mean* preceſſion, at the rate of one degree in 71 $\frac{1}{2}$ years, would cauſe in this ſtar, from *March* 27th 1727, to the day on which the obſervation was taken: the fourth ſhews the aberrations of light: the fifth, the equations ariſing from the 'forementioned hypotheſis: and the ſixth gives the *mean* diſtance of the ſtar from the point with which it was compared, found, by collecting the ſeveral numbers, according to their ſigns, in the 3d, 4th, and 5th columns, and applying them to the *obſerved diſtances* contain'd in the ſecond.

If the obſervations had been perfectly exact, and the ſeveral equations of their *due* quantity; then all the numbers in the laſt column would have been equal; but ſince they differ a little from one another; if the *mean* of all be taken, and the extremes are compared with it, we ſhall find no greater difference, than what may be ſuppoſed to ariſe from the uncertainty of the obſervations themſelves; it no where amounting to more than 1″ $\frac{1}{2}$. The hypotheſis therefore ſeems, in this ſtar, to agree extremely well with the obſervations here ſet down; but as I had made above 300 of it; I took the trouble of comparing each of them with the hypotheſis: and altho' it might have been expected, that, in ſo large a number, ſome great errors would have occurred; yet there are very few, only 11, that differ from the mean of theſe ſo much as 2″; and not one that differs ſo much as 3″. This ſurpriſing agreement, therefore, in ſo long a ſeries of obſervations, taken in all the various ſeaſons of the year, as well as in the different poſitions of the moon's

nodes, ſeems to be a ſufficient proof of the truth, both of *this* hypotheſis, and alſo of *that* which I formerly advanced, relating to the aberrations of light; ſince the polar diſtance in this ſtar may differ, in certain circumſtances, almoſt a minute, *viz.* $56''\frac{1}{2}$, if the corrections reſulting from both theſe hypotheſes are neglected; whereas, when thoſe equations are rightly applied, the mean place of the ſtar comes out the ſame, as nearly, as can be reaſonably expected.

γ *Draconis*	South of ° ′ 38. 25	Preceſſion.	Aberration.	Nutation.	Mean Diſt.
	″	″	″	″	″
1727 September 3	70.5	— 0.4	+ 19.2	— 8.9	80.4
1728 March 18	108.7	— 0.8	— 19.0	— 8.6	80.3
September 6	70.2	— 1.2	+ 19.3	— 8.1	80.2
1729 March 6	108.3	— 1.6	— 19.3	— 7.4	80.0
September 8	69.4	— 2.1	+ 19.3	— 6.4	80.2
1730 September 8	68.0	— 2.9	+ 19.3	— 3.9	80.5
1731 September 8	66.0	— 3.8	+ 19 3	— 1.0	80.5
1732 September 6	64.3	— 4.6	+ 19.3	+ 2.0	81.0
1733 Auguſt 29	60.8	— 5.4	+ 19.0	+ 4.8	79.2
1734 Auguſt 11	62.3	— 6.2	+ 16 9	+ 6.9	79.9
1735 September 10	60.0	— 7.1	+ 19.3	+ 7.9	80.1
1736 September 9	59.3	— 8.0	+ 19.3	+ 9.0	79.6
1737 September 6	60.8	— 8.8	+ 19.3	+ 8.5	79.8
1738 September 13	62.0	— 9.6	+ 19.3	+ 7.0	78.7
1739 September 2	66.6	— 10.5	+ 19.2	+ 4.7	80.0
1740 September 5	70 8	— 11.3	+ 19 3	+ 1.9	80.7
1741 September 2	75.4	— 12.1	+ 19.2	— 1.1	81.4
1742 September 5	76.7	— 12.9	+ 19.3	— 4.0	79.1
1743 September 2	81 6	— 13.7	+ 19.1	— 6 4	80.6
1745 September 3	86.3	— 15.4	+ 19.2	— 8.9	81.2
1746 September 17	86.5	— 16.2	+ 19.2	— 8.7	80.8
1747 September 2	86.1	— 17.0	+ 19.2	— 7.6	80.7

I made about 250 obſervations of β *draconis*; which I find correſpond as well with the hypotheſis, as thoſe of γ; but ſince the poſitions of both theſe ſtars, in reſpect to the ſolſtitial colure, differ but little from each other; it will be needleſs to ſet down the obſervations of β. I ſhall therefore proceed to lay before your Lordſhip, ſome obſervations of a ſmall ſtar, that is almoſt oppoſite to γ *draconis* in R. aſcenſion, being the 35th *Camelopardali Hevel.* in the *Britiſh* catalogue. Mr *Flamſteed*, indeed, has not given the R. aſcenſion of this ſtar; but *that* being neceſſary to be known, in order to compute the change of it's declination ariſing from the preceſſion of the equinox; I compared the time of it's tranſit over the meridian, with that of ſome other ſtars near the

ſame

ſame parallel; whereby I found, that it's R. aſcenſion was 85°. 54′. ½ at the beginning of the year 1737.

This ſmall ſtar was compared with the ſame point of the limb of my ſector, as *γ draconis*; and the ſecond column, in the following table, ſhews how many *ſeconds* it was found to be S. of that point, at the time of each reſpective obſervation. The other columns contain, as in the foregoing table, the equations that are neceſſary to find, what it's *mean* diſtance from the ſame point would have been on *March* 27th 1727, which is exhibited in the laſt column. The whole number of my obſervations of this ſtar did not much exceed 40; the greateſt part of which were made before 1730; in ſome of the following years none were taken; and only a ſingle one in any other, except in 1739. However, their correſpondency ſeems ſufficient to evince the truth of the hypotheſis: for if the mean of theſe, contain'd in the table, be taken, not one, among the reſt of the obſervations, will differ from it more than 2″.

35th *Camelopard. Hevelii.*	South of ° ′ 38. 25	Preceſſion.	Aberration.	Nutation.	Mean Diſt. South.
	″	″	″	″	″
1727 October 20	73.6	+ 0.9	— 6.7	+ 8.9	76.7
1728 January 12	60.8	1.2	+ 6.1	8.8	76.9
March 1	57.8	1.4	+ 9.4	8 7	77.3
September 26	75.2	2.3	— 8.8	8 1	76.8
1729 February 26	56.4	2.8	+ 9.4	7.6	76 2
1730 March 3	57.8	4.4	9.4	5.4	77 0
1731 February 5	59 1	5.6	8 5	+ 3.0	76.2
1733 January 31	64.1	8.7	8.2	— 2.9	78.1
1738 December 30	61.8	17.2	4.3	6.5	76.8
1739 February 4	56.9	17.3	8.5	6.3	76 4
1740 January 20	56.0	18.6	7.0	— 4.0	77.6
1747 February 27	32 3	28.5	9 4	+ 8 4	78.6

The obſervations of the foregoing ſtars are the moſt proper, to prove the change of the inclination of the earth's axis to the plane of the ecliptic; thoſe which follow, will ſhew in what manner the ſtars, that lie near the equinoctial colure, are affected, as well as others, that are differently ſituated, with reſpect to the cardinal points of the equator. Some of theſe ſtars are indeed more remote from the zenith, than I would have choſen, if there had been others, of equal luſtre, in more proper poſitions; becauſe experience has long ſince taught me, that the obſervations of ſuch ſtars, as lie near the zenith, do generally agree beſt with one another, and are therefore the fitteſt to prove the truth of any hypotheſis. I ſhall begin with thoſe near the vernal equinox.

equinox. α *Caſſiopeæ* was compared with the point marked 34°. 55′; and at firſt was found to be more *ſoutherly*, but afterwards became more *northerly* than that point, as in the following table; the laſt column of which ſhews it's mean diſtance *S.* of that point on *March* 27, 1727. The obſervation of *Dec.* 23, 1738, differs 3″ from the *mean* of the others; as does alſo another, that was taken 5 days after this; neither of which being marked as uncertain, I judged it proper to inſert one of them; altho' they give the mean place of the ſtar near 2″ more *northerly* than any other, in a ſeries of above 100; *all* of which correſpond, with the *mean* of theſe here recited, within leſs than 2″; excepting *two*, that give the ſtars mean diſtance almoſt 3″ more *ſoutherly*; but theſe laſt mentioned are marked as dubious; and indeed they appear to have been bad, by comparing them with ſeveral others, that were made near the ſame time, from which they differ almoſt 2″.

α *Caſſiopeæ*.	South of ° ′ 34. 55	Preceſſion.	Aberration.	Nutation.	Mean Diſt. South
	″	″	″	″	″
1727 September 9	55.0	+ 9.0	+ 2.2	+ 2.4	68.6
1728 September 17	30.8	29 4	+ 4.6	5.2	70 0
1729 June 8	35.7	43.8	— 16.3	6.8	70.0
December 3	N. 9 4	53.5	+ 16.5	7.7	68.3
1730 June 11	S. 13 8	64.0	— 16 2	8 4	70.0
December 9	N. 30 8	73.8	+ 16.3	8.8	68.1
1732 January 8	N. 49.2	95.4	12.9	8.9	68.0
1733 January 21	64.8	116.0	+ 10 0	7.9	69.1
1734 June 13	62.8	143.8	— 16.1	5.0	69 9
December 11	105 4	153.7	+ 16.2	+ 3.7	68.2
1738 December 23	176.3	234.0	+ 15.2	— 7.2	65.7
1740 June 2	169.1	262.8	— 16.5	— 8.9	68.3
1747 February 27	332.3	397.0	+ 0.2	+ 4 7	69 6

Altho' I have taken no obſervation of τ *Perſei* ſince *Jan.* 22, 1740; yet, as this ſtar is very near the zenith, and a ſufficient number were made about the times when the equation, reſulting from the hypotheſis, was at it's *maximum*; I judged it proper to inſert ſome of them in the next table; the laſt column of which ſhews, how much the ſtar's *mean* diſtance was *S.* of 38°. 20′. on *March* 27, 1727. Among near 60 obſervations I meet with 2 only, that differ from the mean of theſe ſo much as 2″; and thoſe differ almoſt as much from the mean of others, that were taken near the ſame time: ſo that the hypotheſis ſeems to correſpond, in general, with the obſervations of this ſtar as well, as with either of the foregoing.

τ Perſei.			South of ° ′ 38. 20	Preceſſion	Aberration.	Nutation.	Mean Diſt. South
			″	″	″	″	″
1727	September	16	60.1	+ 7.4	— 3.2	+ 6.7	71.0
	December	29	39.7	11.9	+ 12 9	7.2	71.7
1728	December	21	22.5	27.2	12 8	8.7	71.2
1729	December	2	S. 9.2	42.0	11.5	9.0	71.7
1731	January	3	N. 8.2	59.0	12.8	8.3	71.9
1732	January	8	22.0	74.8	12 7	6.7	72.2
1733	January	21	34.6	91 0	11.7	+ 4.3	72.4
1738	December	23	117.0	183 4	12.8	— 9.0	70.2
1740	January	22	132.5	200.2	11.7	8.6	70.8

After the laſt recited obſervations, it may perhaps ſeem needleſs to add thoſe of *α Perſei*, which is farther from the zenith; but however, as this ſtar lies very nearly at an equal diſtance from the equinoctial and ſolſtitial colures, and the ſeries of obſervations of it is ſomewhat more complete, than that of *τ Perſei*; I ſhall inſert one at leaſt, for each year wherein it has been obſerved; whereby it may appear, that the hypotheſis ſolves the *phænomena* of ſtars in this ſituation, as exactly as in others: for if a *mean* be taken of the numbers in the laſt column of the following table, which expreſſes the *mean* diſtance of the ſtar *S.* of 41°. 5′. on *March* 27, 1727, it will agree within 2″ with every one of 80 obſervations, that have been made of this ſtar.

α Perſei			South of ° ′ 41. 5	Preceſſion.	Aberration.	Nutation.	Mean Diſt. South.
			″	″	″	″	″
1727	December	29	79.4	+ 10.5	+ 11.4	+ 7.9	109.2
1728	April	7	87.5	14.3	— 0.8	8.2	109.2
	July	5	94.6	17.7	— 11.4	8.5	109.4
	December	12	65.7	23.8	+ 10.6	8.8	108.9
1729	December	3	53.4	37.2	9.7	8.9	109.2
1731	January	3	38.6	52.3	11.4	7.8	110.1
1732	January	8	26.8	66.2	+ 11.4	+ 5.9	110.3
1734	July	11	S. 21.3	101.0	— 11.4	— 1.1	109.8
1738	December	24	N. 56.3	162.6	+ 11.2	9.0	108.5
1740	January	21	71.8	177.4	10.9	— 8.2	108.3
1747	February	27	182.5	275.4	6.6	+ 8.5	108.0

Having already given examples of ſtars, lying near both the ſolſtices and the *vernal* equinox; I ſhall now add the obſervations of *one*, that is not far from the *autumnal* equinox, *viz.* *η urſæ majoris*, the brighteſt ſtar in that part of the heavens, which approaches the zenith of *Wanſted* within a degree; and which, by reaſon of it's luſtre and poſition, gave me

me the opportunity of making my ſeries of obſervations of *it*, more complete than of many others. This ſtar was compared with the point marked 39°. 15′. and was *S.* of it as in the following table; wherein your Lordſhip will ſee, that the obſervations of the years 1740 and 1741 give the polar diſtances 3″ greater, than the *mean* of the other years. Had there been only a ſingle obſervation taken in either of thoſe years, part of this apparent difference might have been ſuppoſed to ariſe from their uncertainty; but as there were 8 obſervations taken within a week, either before or after *June* 3, 1740, which agree well with each other; and three were made within 20 days in *Sept.* 1741, which likewiſe correſponded with each other; I am inclined to think, that the 'forementiond differences muſt be owing to ſomething elſe, beſides the error of the obſervations. This *phænomenon* therefore may deſerve the conſideration of thoſe gentlemen, who have employed their time in making computations relating to the quantity of the effects, which the power of gravity may, on various occaſions, produce. For I ſuſpect, that the poſition of the moon's apogee, as well as of her nodes, has ſome relation to the apparent motions of the ſtars that I am now ſpeaking of.

My ſeries of obſervations of ſeveral ſtars abound, of late years, with ſo many and long interruptions; that I cannot pretend to *determine* this point; but probably the differences before taken notice of in the obſervations of *α Caſſiopeæ*, and ſome others that I have found likewiſe among the obſervations of *other* ſtars, that are not here recited, may be owing to ſuch a cauſe; which, altho' it ſhould not have any large ſhare of influence, may yet, in certain circumſtances, diſcover a defect in a hypotheſis, that pays no regard at all to *it*. But whether theſe differences do ariſe from the cauſe already hinted at; or whether they proceed from any defect of the hypotheſis itſelf in any other reſpect; it will not be very *material* in point of practice; ſince *that* hypotheſis, as it was before laid down, appears to be ſufficient to ſolve all the *phænomena*, to as great a degree of exactneſs, as we can in general *hope* or *expect* to make obſervations. For if I take the *mean* of all the numbers in the laſt column of the following table for *η urſæ majoris*, and compare it with any one of 164 obſervations that were taken of it, the difference will not exceed 3″.

η Urſæ Majoris	South of ° ′ 39. 15	Preceſſion.	Aberration.	Nutation.	Mean Diſt. South.
	″	″	″	″	″
1727 October 17	153.3	— 10.2	+ 1.0	— 5.2	138.9
1728 January 24	176.4	15.2	— 17.6	5.8	137.8
July 17	150.8	23.9	+ 17.8	6.9	137.8
October 11	170.6	28.2	+ 2.6	7.3	137.7
1729 January 16	196.6	33.1	— 17.8	7.8	137.9
July 21	170.4	42.4	+ 17.8	8.4	137.4
1730 July 19	189.6	60.6	+ 17.8	9.0	137.8
December 28	232.4	68.7	— 16.7	8.9	138.1
1731 September 18	218.1	81.9	+ 9.4	8.4	137.2
1732 January 10	250.7	87.7	— 17.7	8.0	137.3
April 13	238.7	92.3	— 0.8	7.7	137.9
1734 July 11	255.7	133.3	+ 17.6	— 2.3	137.7
1735 September 10	280.8	154.6	+ 11.4	+ 1.2	138.8
1736 September 8	294.7	172.8	11.6	4.1	137.6
1737 July 3	303.0	187.8	17.2	6.1	138.5
1738 June 29	319.0	205.8	16.8	7.9	137.9
1739 April 25	348.0	220.8	2.5	8.8	138.5
1740 June 3	360.3	241.1	12.8	8.9	140.9
1741 September 23	390.9	265.0	7.9	+ 7.4	141.2
1745 September 5	466.7	337.1	12.4	— 3.3	138.7
1746 September 20	492.0	356.2	8.8	5.9	138.7
1747 September 2	507.2	373.5	13.2	7.8	139.1

You may perceive, by inſpecting the tables which contain the obſervations of *α Caſſiopeæ* and *η urſæ majoris*; that the greateſt differences that occur therein may be diminiſhed, by ſuppoſing the *true* pole of the equator to move round the point *P*, in an *ellipſis*, inſtead of a circle. For if the tranſverſe axis, lying in the direction *AC*, be 18″, and the conjugate, as *DB*, be about 16″; the equations, reſulting from ſuch an hypotheſis, will make the numbers in the laſt columns agree with each other, nearer than as they now ſtand. But ſince this would not entirely remove the inequalities, in all the poſitions of the moon's nodes; I ſhall refer the more accurate determination of the *locus* of the *true* pole to theory; and at preſent only give the equations for the preceſſion of the equinoctial points, and the obliquity of the ecliptic, as alſo the real quantity of the annual preceſſion, to every 5th degree of the place of the moon's aſcending node, in the following tables; juſt as they reſult from the hypotheſis, as at firſt laid down; it appearing, from what has already been remark'd, that theſe will be ſufficiently exact for practice in all caſes.

The Equation of the Equinoct. Points.

☽'s ☊ from ♈	Sig. O / Sig. VI	I / VII	II / VIII	Subſt / Add
°	//	//	//	°
0	0.0	1.3	19.6	30
5	2.0	13.0	20.5	25
10	3.9	14.5	21.2	20
15	5.8	16.0	21.8	15
20	7.7	17.3	22.2	10
25	9.6	18.5	22.5	5
30	11.3	19.6	22.6	0
Subſt.	Sig. V	IV	III	☽'s ☊
Add	Sig. X	X	IX	from ♈

The Equation of the Obliquity of the Ecliptick

☽'s ☊ from ♈	Sig. O / Sig. VI	I / VII	II / VIII	Add / Subſt
°	//	//	//	°
0	9.0	7.8	4.5	30
5	9.0	7.4	3.8	25
10	8.9	6.9	3.1	20
15	8.7	6.4	2.3	15
20	8.5	5.8	1.6	10
25	8.2	5.2	0.8	5
30	7.8	4.5	0.0	0
Add	Sig. V	IV	III	☽'s ☊
Subſt.	Sig. XI	X	IX	from ♈

The Annual Preceſſion of the Equinoctial Points.

☽'s ☊ from ♈	Sig. O	I	II	III	IV	V	
°	//	//	//	//	//	//	°
0	58.0	57.0	54.2	50.3	46.5	43.7	30
5	57.9	56.6	53.6	49.7	46.0	43.4	25
10	57.9	56.2	53.0	49.0	45.5	43.2	20
15	57.7	55.7	52.3	48.4	45.0	43.0	15
20	57.5	55.2	51.7	47.7	44.5	42.8	10
25	57.3	54.7	51.0	47.1	44.1	42.8	5
30	57.0	54.2	50.3	46.5	43.7	42.7	0
	Sig. XI	X	IX	VIII	VII	VI	☽'s ☊ from ♈

Sir *I. Newton*, in determining the quantity of the annual preceſſion from the theory of gravity, upon ſuppoſition that the equatorial is to the polar diameter of the earth as 230 is to 229, finds the ſun's action ſufficient to produce a preceſſion of $9''\frac{1}{8}$ only; and, collecting from the tides the proportion between the ſun's force and the moon's to be as 1 to $4\frac{1}{2}$, he ſettles the mean preceſſion, reſulting from their joint actions, at $50''$. But ſince the difference between the polar and equatorial diameter is found, by the late obſervations of the gentlemen of the *Academy of Sciences*, to be greater than what Sir *Iſaac* had computed it to be; the preceſſion, ariſing from the ſun's action, muſt likewiſe be greater than what he has ſtated it at, nearly in the ſame proportion. From whence it will follow, that the moon's force muſt bear a leſs proportion to the ſun's than $4\frac{1}{2}$ to 1; and perhaps the *phænomena*, which I have now been giving an account of, will ſupply the beſt *data* for ſettling this matter.

As

As I apprehend, that the obſervations already ſet down will be judged ſufficient, to prove in general the truth of the hypotheſis before advanced; I ſhall not trouble your Lordſhip with the recital of more, that I made of ſtars lying at greater diſtances from the zenith; thoſe not being ſo proper, for the reaſon before-mentioned, to eſtabliſh the point that I had chiefly in view. But as it may perhaps be of ſome uſe to future Aſtronomers, to know what were the *mean* differences of declination, at a given time, between ſome ſtars, that lie nearly oppoſite to one another in right aſcenſion, and not far from either of the *Colures*; I ſhall ſet down the reſult of the compariſon of a few, that differ ſo little in declination, that I could determine the quantity of that difference with great certainty.

By the *mean* of 64 obſervations, that were made of *α Caſſiopeæ* before the end of 1728, I collect, after allowing for the preceſſion, aberration and nutation as in the foregoing tables; that the *mean* diſtance of this ſtar was 68″.7 *S.* of 34°. 55′, on *March* 27, 1727. By a like compariſon of 40 obſervations, taken of *γ urſæ majoris* during the ſame interval of time, I find this ſtar was, at the ſame time, 39″.6 *S.* of 34°. 45′. I carefully meaſured, with the ſkrew of the micrometer, the diſtance between the points, with which theſe ſtars were compared; and found them to be 9′. 59″ from each other, or one ſecond leſs than they ought to have been. Hence it follows, that the *mean* difference of declination between theſe two ſtars, was 10′. 28″.1, on *March* 27, 1727.

By the *mean* of 65 obſervations, that were taken of *β Caſſiopeæ*, before the end of the year 1728, this ſtar was 25″.8 *N.* of 32°. 20′, on the 27th day of *March* 1727: and by the *mean* of 52 obſervations, *ε urſæ majoris* was 87″.6 *S.* of 32°. 30′ at the ſame time. The diſtance between theſe points was found to be 9′. 59″.3; from whence it follows, that the *mean* difference of declination between theſe two ſtars was 11′. 52″.7 on *March* 27, 1727.

By the *mean* of 100 obſervations, taken before the end of the year 1728, the *mean* diſtance of *γ draconis* was 79″.8 *S.* of 38°. 25′ on *March* 27, 1727; and by the *mean* of 35 obſervations, the 35th *camelopard. Hevel.* was *S.* of the ſame ſpot 76″.4. So that the mean polar diſtance of *γ draconis* was only 3″.4 greater, than that of 35th *camelopard. Hevel.* But as the equation for the nutation, in both theſe ſtars, was then near the *maximum*, and to be applied with contrary ſigns; the *apparent* polar diſtance of *γ draconis* was 21″.4 greater, on *March* 27, 1727.

The differences of the polar diſtances of the ſtars, as here ſet down, may be preſumed, both on account of the radius of the inſtrument and the number of obſervations, to be very exactly determined, to the time when the moon's aſcending node was at the beginning of *Aries*; and if a like compariſon be hereafter made, of obſervations taken of the ſame ſtars, near the ſame poſition of the moon's nodes; future Aſtronomers

may be enabled, to ſettle the quantity of the mean preceſſion of the equinox, ſo far as it affects the declination of theſe ſtars, with great certainty: and they may likewiſe diſcover, by means of the ſtars, near the ſolſtitial colure, from what cauſe the apparent change in the obliquity of the ecliptic really proceeds, if the mean obliquity be found to diminiſh gradually.

The forementioned points indeed can be ſettled only on the ſuppoſition, that the angular diſtances of theſe ſtars do continue always the ſame, or that they have no real motion in themſelves; but are at reſt in abſolute ſpace. A ſuppoſition, which though uſually made by Aſtronomers, nevertheleſs ſeems to be founded on too uncertain principles, to be admitted in all caſes. For if a judgment may be formed, with regard to this matter, from the reſult of the compariſon of our beſt modern obſervations, with ſuch as were formerly made with any tolerable degree of exactneſs; there appears to have been a real change in the poſition of ſome of the fixed ſtars, with reſpect to each other; and ſuch, as ſeems independent of any motion in our own ſyſtem, and can only be referred to ſome motion in the ſtars themſelves. *Arcturus* affords a ſtrong proof of this: for if it's preſent declination be compared with it's place, as determined either by *Tycho* or *Flamſteed*; the difference will be found to be much greater, than what can be ſuſpected to ariſe from the uncertainty of their obſervations.

It is reaſonable to expect, that other inſtances of the like kind muſt alſo occur among the great number of the viſible ſtars: becauſe their relative poſitions may be altered by various means. For if our own ſolar ſyſtem be conceived to change it's place, with reſpect to abſolute ſpace; this might, in proceſs of time, occaſion an apparent change in the angular diſtances of the fixed ſtars; and in ſuch a caſe, the places of the neareſt ſtars being more affected, than of thoſe that are very remote; their relative poſitions might ſeem to alter; tho' the ſtars themſelves were really immoveable. And on the other hand, if our own ſyſtem be at reſt, and any of the ſtars really in motion, this might likewiſe vary their apparent poſitions; and the more ſo, the nearer they are to us, or the ſwifter their motions are, or the more proper the direction of the motion is, to be rendered perceptible by us. Since then the relative places of the ſtars may be changed from ſuch a variety of cauſes, conſidering that amazing diſtance at which it is certain ſome of them are placed, it may require the obſervations of many ages, to determine the laws of the apparent changes, even of a ſingle ſtar: much more difficult therefore muſt it be, to ſettle the laws relating to all the moſt remarkable ſtars.

When the cauſes, which affect the places of all the ſtars in general are known; ſuch as the preceſſion, aberration, and nutation; it may be of ſingular uſe, to examine nicely the relative ſituations of particular ſtars: and eſpecially of thoſe of the greateſt luſtre, which, it may be preſumed lie neareſt to us, and may therefore be ſubject to more ſenſible changes; either

either from their own motion, or from that of our ſyſtem. And if at the ſame time that the brighter ſtars are compared with each other, we likewiſe determine the relative poſitions of ſome of the *ſmalleſt* that appear near them, whoſe places can be aſcertained with ſufficient exactneſs; we may perhaps be able to judge to what cauſe the change, if any be obſervable, is owing. The uncertainty that we are at preſent under, with reſpect to the degree of accuracy wherewith former Aſtronomers could obſerve, makes us unable to determine ſeveral things, relating to the ſubject that I am now ſpeaking of: but the improvements, which have of late years been made in the methods of taking the places of the heavenly bodies, are ſo great, that a few years may hereafter be ſufficient, to ſettle ſome points; which cannot now be ſettled, by comparing even the earlieſt obſervations with thoſe of the preſent age.

It were to be wiſhed therefore, that ſuch perſons as are provided with proper inſtruments, would attempt to determine, with great care, the preſent relative poſitions of ſeveral of the principal ſtars, in various parts of the heavens; eſpecially of thoſe, that are leaſt affected by refraction: *that* Cauſe having many times ſo uncertain an influence on the places of objects, that are very remote from the zenith; that wherever *It* is concerned, the concluſions, deduced from obſervations that are *much* affected by it, will always remain doubtful, and too precarious, in many caſes, to be relied upon.

The advantages, ariſing from different perſons attempting to ſettle the ſame points of Aſtronomy near the ſame time, are ſo much the greater; as a concurrence in the reſult, would remove all ſuſpicion of incorrectneſs in the inſtruments made uſe of. For which reaſon, I eſteem the curious *apparatus* at *Shirburn Caſtle*, and the obſervations there taken, as a moſt valuable *criterion*, whereby I may judge of the accuracy of thoſe, that are made at the *Royal Obſervatory*: and as a lover of ſcience I cannot but wiſh, that our nation abounded with more frequent examples, of perſons of like rank and ability with your Lordſhip, equally deſirous of promoting *This*, as well as every other branch of natural knowledge, that tends to the honour and benefit of our country.

Greenwich, Dec. 31, 1747.

II. Theſe declinations are taken from various obſervations, made with a quadrant of 3 feet, in *June* 1737, 1738, *&c.* at *Quito* in *America*, in 0° 13′ 16″ *S.* Lat. in a place 11″ more to the *S.* and the place of obſervation of the ſolſtices, *Dec.* 1736 and *June* 1737, the latitude of which I had already determined in my diſcourſe concerning the diſtance of the tropicks. In calculating theſe declinations, I made uſe of *M. Bouguer*'s table of refractions for the height of *Quito*, inſerted in the *Memoires de l'Acad.* 1738.

Declinations of ſome Southern Stars *of the 1ſt and 2d magnitude, in* June 1738, *with the method of finding the time at ſea in the night, by the aſpect of the* Southern Croſs, *by* M. de la Conda-

	°	′	″
In the ſhip *Argo* α *Canopus*, a ſtar of the 1ſt magnitude, and the greateſt of the fixed ſtars, excepting *Sirius*.	52	34	16

One

mine, *R. S. Lond. & Acad. R. Sc. Par. Soc.* No. 492. p. 139. *April, &c.* 1749. *Read* May 11, 1749.

	°	′	″
One in the preceding or occidental arm of the *ſouthern croſs* (*Bayero* γ) of a middle magnitude between the 2d and 3d.	57	17	32
ζ a ſtar in the foot of the *croſs*, is the neareſt to the pole of the 4 ſtars which conſtitute the *croſs*, being viewed by a teleſcope, appears double, but to the naked eye ſingle, and of the 1ſt magnitude.	61	38	57
ε the moſt northern in the top of the *croſs*, of the 2d magnitude, ξ in the following arm of the 2d magnitude.	58	15	5
In the preceding or occidental foot of the *centaur* γ of the 1ſt magnitude.	59	5	35
In the following foot of the *centaur* α of the 1ſt magnitude.	59	44	56

When we obſerved at *Panama*, in *Jan.* 1736, we found by repeated trials, that about 2′ are to be added to the declination of the ſtar *canopus* in the *Britiſh Catalogue*, to make the latitude of the place taken by the obſervations of that ſtar to agree with that taken from the altitudes: and this remark was confirmed by all the following obſervations, and particularly by thoſe on which the above declination of *canopus* depends: whence it appears to be greater by 2′ 2″ than in the *Britiſh Catalogue*. All the ſtars above-mentioned are very bright, and the moſt viſible of any in the *S.* hemiſphere, that are not ſeen in *Europe*.

In moſt planiſpheres the ſouthern croſs is variouſly repreſented: in ſome it's ſituation is from *N.* to *S.* in others from *N. E.* to *S. W.* *Pardie*'s celeſtial chart of the ſouthern part of the heavens, gives two ſchemes of the ſouthern croſs, one in the former, the other in the latter direction, the firſt of which is the true one. The ſouthern croſs therefore, when it is in the meridian, appears upright, that is, perpendicular to the horizon, and therefore may ſerve mariners to find out the hour without any ſenſible error, the difference of time being known between it and the ſun's paſſing the meridian, may be eaſily accommodated to practice by the following method.

From repeated obſervations reduced to the preſent year 1749, I gather, that there are about 4′ 30″ between the mediation of the ſtars ζ and ε in the foot and head of the ſouthern croſs, and that the firſt reaches the meridian about 13′ after it has culminated the firſt point of *Aries* in the northern hemiſphere. Therefore from the table of mediation of the firſt point of *Aries* in the *Connoiſſance des Tems*, the true hour at night by ſea will be eaſily obtained by viewing the ſouthern croſs, and obſerving at what hour it ſhall appear upright and perpendicular to the horizon, or rather when the time will permit, by obſerving with a plumb line held in the hand the very moment when the ſtars ζ in the foot, and α in the head of the *ſouthern croſs*, ſhall appear equally diſtant from the perpendicular; the latter on the eaſt ſide, and the former on the weſt. For at the point of time when this poſition of the line ſhall happen, there will ſcarce be an error of 1′ the true hour, if you add 15′ to the hour of mediation of the

the firſt point of *Aries*, which will be determined by the abovementioned table, the difference of the meridians of the calculator and obſerver being amended.

The ſtar ζ therefore in the foot of the croſs appears the greateſt of the 4, becauſe when ſeen by the naked eye it unites with another ſmall one, which comes to the meridian 4″ or 5″ after it, and when obſerved by a teleſcope is 1′ 31″ more to the S; the diſtance being meaſured by a micrometer.

The following or eaſtern foot of the *centaur* α, a ſtar alſo of the firſt magnitude, which ſeems to equal or perhaps exceed *capella* in brightneſs and magnitude, is alſo double, and conſiſts of two ſtars, the ſmaller of which is ſcarce diſcovered to emerge from the greater by a good teleſcope of 3 feet. The latter is alſo more northern, and the other a little more ſouthern.

Feuilleé, who obſerved them both with a teleſcope of 16 feet, determines the greater to be of the third and the leſs of the fourth magnitude; which I have not been able to confirm by my own obſervations. But the ſame author erroneouſly calls the foot of the *centaur*, in which theſe united ſtars appear, the northern. The declination of the ſame ſtar was obſerved by *Feuilleé*, *Feb.* 26. 1710. in the city of *Conception* in *Chili*, and was determined by him to be greater than the declination of the other foot by 39′.

III. The chief thing required in the preſent calculation, is to meaſure arches of parallel circles in a ſphere, by degrees and minutes of a great circle. It is paſt all controverſy, that the circumferences of circles are in a *ratio* of their diameters and ſemidiameters. Let the ſemidiameter of a great circle be the whole ſine, and the ſemidiameter of a parallel circle the coſine of declination: it will be eaſy to determine how many ſeconds of the great circle are contained in a degree of the parallel circle, its declination being determined. For as *radius* to the number of ſeconds of one degree in the great circle, ſo 3600, or the coſine of declination, to the number of ſeconds contained in one degree of the parallel circle. On repeating the calculation, we have found, that the arcs of one degree of parallel circles, proceeding from one degree of declination to 29, are equal to the following numbers:

A new method of calculating Eclipſes *of the* Sun; *or any* Occultations *of the* Stars *by the* Moon; *by* Chriſtian Lewis Gerſten, *F. R. S. and Prof. Math.* Gieſen. N°. 473. p. 22. May, &c. 1744. *Preſented* May 10, 1744.

Introduction.

Sect. 1.

Deg. of Decl.	Arcs of par. Circ. I	II	III	Deg. of Decl.	Arcs of par. Circ. I	II	III	Deg. of Decl.	Arcs of par. Circ. I	II	III
1	59.	59.	27.	11	58.	53.	51.	21	56.	0.	53.
2	59.	57.	48.	12	58.	41.	19.	22	55.	37.	51.
3	59.	55.	3.	13	58.	27.	43.	23	55.	13.	49.
4	59.	51.	13.	14	58.	13.	3.	24	54.	48.	45.
5	59.	46.	18.	15	57.	57.	19.	25	54.	22.	42.
6	59.	40.	16.	16	57.	40.	32.	26	53.	55.	39.
7	59.	33.	9.	17	57.	22.	41.	27	53.	27.	37.
8	59.	24.	57.	18	57.	3.	48.	28	52.	58.	36.
9	59.	15.	40.	19	56.	43.	51.	29	52.	28.	37.
10	59.	5.	18.	20	56.	22.	53.				

From

From theſe, by ſimple addition, and cutting off the fourths, we compoſed a table of the reduction of parallel arcs to minutes, ſeconds, &c. of a great circle, to every degree of declination from 1 to 29: by the help of which, we may reduce any arcs in parallel circles leſs than one degree, to minutes and ſeconds of a great circle, the intermediate declination of which, and their values alſo are found without much difficulty by the help of an additional table. We have retained the thirds in the table, that when they amount to above 50, a ſecond may be put in their room. By way of example we ſhall give a part of the table, namely of a parallel circle, the declination of which is 18 degrees.

Arcs of par. Cir.	Parts of a great Circle.			Arcs of par. Cir.	Parts of a great Circle.			Arcs of par. Cir.	Parts of a great Circle.		
′	′	″	‴	′	′	″	‴	′	′	″	‴
″	″	‴	⁗	″	″	‴	⁗	″	″	‴	⁗
1	0	57	3	21	19	58	19	41	38	59	35
2	1	54	7	22	20	55	23	42	39	56	39
3	2	51	11	23	21	52	27	43	40	53	43
4	3	58	15	24	22	49	31	44	41	50	47
5	4	45	19	25	23	46	35	45	42	47	51
6	5	42	22	26	24	43	38	46	43	44	54
7	6	39	26	27	25	40	42	47	44	41	58
8	7	36	30	28	26	37	46	48	45	39	2
9	8	33	34	29	27	34	50	49	46	36	6
10	9	30	38	30	28	31	54	50	47	33	10
11	10	27	41	31	29	28	57	51	48	30	13
12	11	24	45	32	30	26	1	52	42	27	17
13	12	21	49	33	31	23	5	53	50	24	21
14	13	18	53	34	32	20	9	54	51	21	25
15	14	15	57	35	33	17	13	55	52	18	29
16	15	13	0	36	34	14	16	56	53	15	32
17	16	10	4	37	35	11	20	57	54	12	36
18	17	7	8	38	36	8	24	58	55	9	40
19	18	4	12	39	37	5	28	59	56	6	44
20	19	1	16	40	38	2	32	60	57	3	48

Example. Let $53' 47''$ of this parallel circle be converted into parts of a great circle:

$53'$	$=$	$50'$	$24''$ $21'''$
$45''$	$=$		42 47
The Sum		51	7

will be the value ſought.

Sect. 2. Small portions of circles parallel to the equator, when they may be ſafely taken for right ones, are cut by circles of declinations at right angles. Wherefore a ſmall ſpherical triangle, one ſide of which is a portion of a circle of declination, another a portion of a parallel circle, may be

be taken for a plain rectangular triangle, and its hypothenuſe may be ſafely determined by the Pythagoric theorem, or other rules of plain Trigonometry. But when this hypothenuſe is the diagonal of any ſpherical quadrilineal, which is effected by the ſection of two circles of declination, by two parallel to the equator, the greater of the parallel arcs, and more remote from the pole, is to be choſen for the baſe of the rectangular triangle, when the queſtion is to find the hypothenuſe.

Tables of the parallaxes of the altitude of the moon are conſtructed two ways; according to *Streete*'s 12th precept, prefixed to the Caroline tables, and by his 13th precept alſo. For the diſtance of the moon from the earth, the *ratio* of this diſtance to the ſemidiameter of the earth, which is immediately known by the horizontal parallax, is ſufficient. The firſt way determines the parallaxes to the altitudes ſeen above the ſenſible horizon. For the eclipſes of the ſun, and appulſes of the moon to the ſtars, the firſt way is to be choſen, and not the latter, which would introduce very great errors into our calculation. As I judged an accurate table of the altitude of parallaxes to be a thing of the greateſt moment, I conſtructed a new one for my own uſe to the altitude of 70°. with which however I afterwards found the Lanſbergian table, *p.* 48, *& ſeq.* to agree well enough. But that which is extant in the Ludovician tables, N°. XXV, regards the altitudes ſeen, but not the true, and therefore is not fit for theſe uſes without reduction. Let it be obſerved, that the parallaxes of the ſame true altitude, but of different diſtances of the moon from the earth, are proportional to the diſtances themſelves, and conſequently to the horizontal parallaxes. Sect. 3.

The following tables exhibit the parallaxes of altitude, both according to our table, and that of *Lanſbergius*; and the numbers being either augmented or diminiſhed in proportion to the other horizontal parallaxes are ſufficient for any caſes whatſoever.

True Alt.	Parall. Alt. our Table.		Parall. Lanſberg.		True Alt.	Parall. Alt. our Table.		Parall. Lanſberg		True Alt.	Parall. Alt. our Table.		Parall. Lanſberg.	
1	60	0	59	59	17	57	41	57	4	33	50	48	50	48
2	59	59	59	59	18	57	23	57	23	34	50	14	50	14
3	59	58	59	57	19	57	4	57	3	35	49	39	49	40
4	59	56	59	54	20	56	44	56	43	36	49	3	49	4
5	59	52	59	50	21	56	23	56	22	37	48	26	48	27
6	59	47	59	46	22	56	0	56	0	38	47	48	47	49
7	59	41	59	40	23	55	37	55	36	39	47	9	47	10
8	59	34	59	33	24	55	12	55	11	40	46	30	46	31
9	59	26	59	24	25	54	47	54	46	41	45	49	45	51
10	59	17	59	14	26	54	21	54	20	42	45	7	45	9
11	59	6	59	4	27	53	54	53	53	43	44	25	44	26
12	58	55	58	53	28	53	25	53	25	44	43	42	43	42
13	58	42	58	41	29	52	56	52	56	45	42	58	42	58
14	58	28	58	28	30	52	26	52	25	46	42	13	42	13
15	58	14	58	14	31	51	54	51	53	47	41	28	41	27
16	57	58	57	58	32	51	22	51	21	48	40	41	40	37

True Alt.	Parall. Alt. our Table.		Parall. Lanſberg.		True Alt.	Parall. Alt. our Table.		Parall. Lanſberg.		True Alt	Parall. Alt. our Table.		Parall. Lanſberg.	
49	39	54	39	54	57	33	10	33	10	64	26	44	26	44
50	39	6	39	7	58	32	17	32	16	65	25	47	25	47
51	38	17	38	18	59	31	23	31	22	66	24	49	24	49
52	37	28	37	28	60	30	28	30	28	67	23	50	23	50
53	36	38	36	37	61	29	33	29	33	68	22	51	22	51
54	35	47	35	46	62	28	37	28	37	69	21	52	21	52
55	34	55	34	55	63	27	41	27	41	70	20	52	20	52
56	34	3	34	3										

Sect 4. The longitude and latitude of a ſtar being given, its right aſcenſion and declination is given by trigonometrical rules. But as that requires a tireſome analyſis of triangles, it is better to make uſe of tables conſtructed on purpoſe. We have in *Flamſted*'s *Hiſt. Cæleſtis*, two of *Abraham Sharp*'s, by which there is a converſion made not only from right aſcenſion and declination into longitude and latitude, but alſo from longitude and latitude into right aſcenſion and declination. Thoſe which are laſt in order *, lead the ſhorteſt way of all, and therefore we have hitherto made uſe of them in this our calculation.

Precepts of the calculation. 1. When it is known by the uſual methods that there will be an eclipſe of the ſun, let the time of conjuction, the longitude and latitude of the moon, the true horary motion thereof, the parallax, and the horizontal diameter, and the horary motion of the ſun and its diameter, be found by theoretical tables.

2. By the help of tables, from the given longitude and latitude, let the right aſcenſion and declination of the ſun and moon be determined.

3. The mean time being converted into apparent, if the point of conjuction happens before noon; then an hour beforehand find out by the horary motion reduced to the ecliptic, the longitudes of the ſun and moon, the latitude of the moon, and the right aſcenſions and declinations of each of the points. If it happens after noon, then the ſame muſt be done an hour after the conjuction.

4. Let the time of conjunction, and that alſo one hour diminiſhed be ſubtracted from 24 hours, when that happens, that there may be an interval of time from the moment of conjunction, or from an hour before the conjunction to noon. In the afternoon hours the time itſelf gives the interval.

5. Let the diſcovered intervals of time be converted into degrees and minutes of the equator; and thus we have the angles of the circle of declination paſſing through the center of the ſun with the meridian of the place.

6. The right aſcenſion of the moon may at any time be either greater or leſs than the right aſcenſion of the ſun. In the morning hours, if it is leſs, then the difference between the right aſcenſions of the ſun and moon is to be ſubtracted from the angle of the circle of declination found in the pre-

* Page 34 and 74, Tom. III.

preceding number: if it is greater, then the difference muſt be added to the ſame angle, and then we have the angle of the circle of declination paſſing through the centre of the moon with the meridian of the place. In the afternoon hours the contrary is to be done.

7. From the angles found by the preceding number, the declinations of the ſun and moon by the ſecond number, and the latitude of the place by the rules of ſpherical Trigonometry, the true altitudes of the ſun and moon in both caſes may be computed; and then alſo,

8. The angles of the circles of declination, paſſing through the centre of the moon in both caſes with the vertical circles. The ſeconds are ſlighted in this and the preceding number.

9. The true altitudes of the moon being found by number 7, it's horizontal parallax by number 1, the parallaxes of the altitude of the moon are found by the tables of parallaxes of altitude. As we may with *Flamſted* allot to the ſun a horizontal parallax of 10″, the horizontal parallax of the moon muſt firſt be diminiſhed by this quantity.

10. As *radius* to number of ſeconds contained in parallax of altitude found by the preceding number, ſo ſine of the angle found in num. 8. to the fourth proportional number, ſhewn by the calculus, I call it the *parallax of the right aſcenſion in a parallel circle.*

11. To proceed, as *radius* to the ſame number of ſeconds contained in the parallax of altitude; ſo the co-ſine of the angle found in num. 8. to the fourth proportional, which is the *parallax of the declination of the moon.* In both caſes, that is, at the very time of conjunction, and an hour before or after this computation is to be made.

12. Let the right aſcenſions of the ſun and moon in both caſes be diſpoſed according to the natural order of the numbers. Let the difference between the right aſcenſions of the ſun be added to the firſt right aſcenſion of the moon, and we ſhall have the firſt right aſcenſion of the ſun; there will then remain two right aſcenſions of the moon, and one of the ſun.

13. The declinations of the ſun either increaſe or decreaſe. In the firſt caſe, let the difference of them be added to that declination of the moon, which agrees with the leaſt right aſcenſion. In the other, let it be ſubtracted, and the mutual diſtance of the luminaries will be as if the ſun without moving, looked upon the progreſſive moon for the entire ſpace of an hour.

14. Let each right aſcenſion be ſubtracted, the leaſt from the biggeſt, and let the differences be carefully noted.

15. Let the parallaxes of declination be ſubtracted from the declinations of the moon, if they are northern; but added if they are ſouthern. Thus are found the viſible declinations of the moon.

16. Let the differences found in num. 14. which are now conceived to be in a parallel circle, be reduced by the table of reduction *, to minutes

* Introd. §. 1.

nutes and ſeconds of a great circle. The declination of the parallel is the ſame with the viſible declination of the moon or ſun. This it quite to be abſtracted from the number and diſtance of the points of right aſcenſion from the beginning of *Aries*: for that is not the concern at preſent, but only the poſition and diſtance of the luminaries from each other.

17. If the moon comes on before noon, then let the parallaxes of right aſcenſion found in the parallel circle, num. 10. be added to the competent places of the moon. But if it happens after noon, ſubtract inſtead of adding. When this is done, the poſitions of the luminaries are determined, and their viſible places, at the time of the true conjunction, and an hour before and after it, whence what follow may be made out with little or no difficulty. For,

18. In every caſe from what has been found ariſes a rectangular triangle, the baſe of which is the diſtances of the apparent places of the moon in the parallel circle; the *cathetus* the difference of it's viſible declinations; the hypothenuſe gives the orbit ſeen; and the poſition of the ſun, whether it falls within or without the triangle, will be alſo ſufficiently determined. The triangle itſelf never ariſes to ſuch a magnitude, as to hinder it's being taken for a plain and rectilineal one. Hence by a moſt eaſy and ſimple conſtruction, with the help of a pair of compaſſes and a ſcale, may be determined the leaſt diſtance of the centres and points in the orbit, the greateſt darkneſs and the end ſo exactly, if a proper ſcale is made uſe of, as hardly to err above a ſecond. But this, and all the reſt may be performed by the rules of plain Trigonometry.

19. When the ſum of the apparent ſemidiameters of the ſun and moon falls without the bounds of the hypothenuſe of this triangle, then it muſt be continued till it meets; and the reſt muſt be performed after the uſual manner, to obtain the time of the beginning and end of the eclipſe. But then, when the points of meeting are too far diſtant from the points of the triangle already determined, the *calculus* will want correction, if the time of the beginning and end is exactly required. For the apparent path of the moon in a right line, and alſo the equable motion ſeen, of which neither is ſtrictly true, tho' the way ſeen for the ſpace of an hour generally diverges ſo little from rectitude in eclipſes, that it may be taken for a right line without any conſpicuous error. But the ſame cannot be ſaid of the equality of ſwiftneſs. There muſt therefore be made a *calculus* of correction, which will be explained better by an example than by rules.

Example. *May* 12, 1706. there happened an eclipſe of the ſun. The quantity, beginning, greateſt darkneſs, and end, are ſought to the longitude and latitude of the obſervatory of *Paris*. According to the *Ludovician* tables, a conjunction of the ſun and moon happened *May* 11, $21^{h}.\ 49'.\ 13''.$ mean time. To this time, according to the ſame tables,

1. The

	°	′	″
1. The true place of ☉ and ☽ in the ecliptick —	51	6	48
Longitude of ☽ in the orbit — — —	51	8	22
Place of ☊ — — —	44	14	59
Argument of latitude — — —	6	53	23
N. Latitude of ☽ — — —		36	7
Horary motion of ☉ — — —		2	25
Semidiameter of ☉ — — —		15	54
Horary motion of ☽ — — —		37	13
Horary motion of ☽ reduced to the ecliptick —		37	5
Horizontal ſemidiameter of ☽ — — —		16	31
Horizontal parallax of ☽ — — —		60	29
According to *Sharp*'s table.			
Right aſcenſion of ☉ — — —	48	37	57
N. declination of ☉ — — —	18	3	32
Right aſcenſion of ☽ — — —	47	53	27
N. declination of ☽ — — —	18	25	58

The equation of time, according to the *Ludovician* tables, is $8' 18''$. It muſt be added to the mean, to make it apparent. Therefore the true time of conjuction is $21^h 57' 31''$.

2. At 1^h before conjunction longit. ☉ $= 51° 4' 23''$. Longit. ☽ $= 50° 29' 43''$ N. Lat. ☽ $= 32' 53''$, conſequently the increaſe of Latitude in the ſpace of one hour $= 3' 15''$. Right aſcenſion of ☉ by *Sharp*'s tables $= 48° 40' 24''$. Declination of ☉ $= 18° 4' 10''$. Right aſcenſion of ☽ $= 48° 30' 21''$. Declination of ☽ $= 18° 38' 59''$.

3. The interval from the moment of conjunction $21^h 57' 31''$ to noon is $= 2^h 2'' 29''$, which being converted into arcs of the equator is $= 30° 37' 15''$. From 1^h before ♁ to noon there are $3^h 2' 29''$ to which the arc of the equator $45° 37' 15''$ anſwers. Therefore, according to rule 5, we have the angles of the circles of declination paſſing thro' the centre of ☉, with the meridian of the place in both caſes.

4. The R. aſc. of ☉ precedes the R. aſc. of ☽ in theſe two caſes: therefore by prec. 6. the differences are to be ſubſtracted from the angles found; namely, in ♁ the difference of the R. aſc. of ☽ from the R. aſc. of ☉ is $10' 3''$. One hour before ♁ the difference is $= 43' 38''$. Therefore when theſe arcs are ſubducted, there remains for the angle of the circle of declination paſſing thro' the centre of ☽ in ♁, $30° 27' 12''$, and 1^h before ♁, $44° 53' 37''$.

5. From theſe angles, the elevation of the pole of the obſervatory at *Paris* $= 48° 50'$, and the declinations of ☽, follow the altitudes of ☽. Particularly in ♁, alt. ☽ $= 51° 5'$. 1^h before ♁ alt. ☽ $= 42° 52'$. The angles alſo of the circles of declination with the vertical ones at ♁, $32° 4'$ at 1^h before ♁, $39° 19'$.

6. According to our table *, to the horizontal parallax $60' 29''$, parall

* Introd. §. 3.

alt.

alt. ☽ in ♂ = 38′. 31″ not being ſubtracted from the horizontal parall. of ☉, which in this example we have purpoſely omitted. The parall. of R. aſc. in the parallel circle = 20′ 27″. The parall. of declination is = 32′ 38″ by prec. 10 and 11. But at 1ʰ before ♂, the parall. of altitude = 44′ 53″, parall. R. aſc. in parall. circ. = 28′ 26″, parall. declin. = 34′ 43″.

7. Now follows, by prec. 12, the diſpoſition and ſubtraction of R. aſcenſions and declinations competent to R. aſcenſions.

	R. Aſc. ° ′ ″	Comp. Decl. ° ′ ″
At 1ʰ before ♂ —	☽ 47 53 35	18 26 0
At the very ♂ —	☽ 48 30 21	18 38 59
At 1ʰ before ♂ —	☉ 48 37 57	18 3 32
At the very ♂ —	☉ 48 40 24	18 4 10
Diff. between R. Aſc. ☉	2 27.	Between declin. ☉ 38

	R. Aſc. ° ′ ″	Declin. ° ′ ″
At 1ʰ before ♂ —	☽ 47 56 2	18 26 38
In the very ♂ —	☽ 48 30 21	18 38 59
Of ☉ unmoved	48 40 24	18 4 10
Diff. *a*	34 19	Parall. 34 43
Diff. *b*	44 22	Declin. 32 38

Declin. ſeen { ☽ 17 51 55 ; ☽ 18 6 21 ; ☉ 18 4 10

8. According to prec. 16, the difference *a* reduced to parts of a great circle is = 32′ 39″; the difference *b* = 42′ 13″. The firſt is the diſtance of the places of the moon in both caſes, the latter the diſtance of the ſun unmoved, from the firſt place of the moon in a parallel circle, the declination of which is 17° 51′ 55″; or, which is not very different, 17° 52′.

9. The parallax of R. aſc. in a parallel circle in ♂ = 20′ 27″ (numb. 6.) being added, by prec. 17. to the ſecond place of the moon, 32′ 39″ makes 53′ 6″. Therefore the firſt place of the moon = parall. R. aſc. at 1ʰ before ♂. Therefore, in the parallel circle, the places of the luminaries ſeen are the following.

	′ ″
At 1ʰ before ♂ ☽	28 26 = A
☉ unmoved	42 13 = B
In the very ♂ ☽	53 6 = C

Diff. between A and B = 13 47
A and C = 24 40

If

If the laſt declination is ſubtracted from the declinations ſeen, in this caſe ☽ 17° 51′ 55″ remains for ☉ 12′ 15″; for ☽ in ♂ 14′ 26″.

10. Now let *b c* be a portion of a circle parallel to the declination 17° 51′ 55″, and therein the point *c* the centre of ☽ at 1ʰ before ♂, *d* the place of ☉, *b* the place of ☽ in ♂; then *d c* = 13′ 47″; *b c* = 24′ 40″. From the points *d* and *b*, erect the perpendiculars *a f* and *a b*; of which the former = 12′ 15″, the difference of the laſt declination; the latter, = 14′ 26″ of the greateſt, *f* will be the centre of the ſun unmoved, *a* the centre of the moon in the very ♂, the right line *a c* the viſible path of the moon at the diſtance of one hour. Fig 3.

11. From the point *f* to *ac* a perpendicular *gf* being let fall determines the quantity of the eclipſe, and the point *g* the greateſt darkneſs. Moreover, if we take with a pair of compaſſes, the ſpace *nf* and *fm* = to the ſum of the apparent ſemidiameters of ☉ and ☽, and if from the ſame point *f* be cut the hypothenuſe produced *mn* of the triangle *abc*, we ſhall have the determination of the points *n* and *m*, in which the beginning and end of the eclipſe happens.

12. By the trigonometrical calculation we have *cg* = 18′ 4″; *gf* = 3′ 37″; *ac* = 28′ 34″. Now if we ſay as *ac* to *gc*, ſo the time by *ac* = 1ʰ to the time by *gc*, there reſults 37′ 37″; this time being added to 20ʰ 57′ 31″ (1ʰ before ♂) makes the point of greateſt obſcuration 21ʰ 35′ 26″.

13. The horizontal ſemidiameter of ☽ is = 10′ 31″ (num. 1.); but corrected by *de la Hire*, tab. 24. is = 16′ 43″. The ſemidiameter of ☉ is = 15′ 54″. The ſum of the ſemidiameters of ☉ and ☽ = 32′ 37″; *gf* being ſubtracted from this ſum, there remains the deficient part = 29′ 0″, this being reduced to ecliptical digits gives the quantity of the eclipſe 10° 56′.

14. To determine the beginning and the end, from *gf*, *fn* and *fm*, are to be ſought *gn* and *gm*. I make *fn* equal to the ſum of the apparent ſemidiameters (num. 13.) diminiſhed by one or two ſeconds, but *fm* equal to the ſame augmented by one or two ſeconds; and ſo *fn* = 32′ 35″; *fm* = 32′ 39″. Wherefore *gn* = 32′ 22″; *gm* = 32′ 25″; the time by *gn* = 1ʰ 7′ 58″; which being ſubtracted from the point of greateſt darkneſs, ſhews the beginning of the eclipſe, 20ʰ 27′ 28″: the time by *gm* = 1ʰ 8′ 5″; which being added to the greateſt obſcuration gives the end 22ʰ 43′ 31″.

15. One hour before ♂ = 20ʰ 57′ 31″; the time of the beginning = 20ʰ 27′ 28″; therefore the beginning differs from 1ʰ before ♂ 30′ 3″. To this difference of time anſwers the motion of ☽ in long. 18′ 34″; increm. lat. ☉ 1′ 37″; motion of ☉ in long. 1′ 12″: theſe being ſubducted from long. and lat. at 1ʰ before ♂, there is left at the time of the beginning, long. ☉ = 51° 3′ 11″; long. ☽ 50° 11′ 9″; lat. ☽ 31′ 16″ R. aſc. ☉ 48° 36′ 44″; decl. ☉ 18° 3′ 13″; R. aſc. ☽ 47° 35′ 10″; declin. ☽ 18° 19′ 28″: diff. between R. aſc. ☉ and ☽ = 1° 1′ 34″; Correction of the beginning.

34″; the interval of time between the moment of beginning and noon = 3^h 32′ 32″; which being converted into arches of the equator gives 53° 8′ 0″. Now becauſe R. aſc. ☽ is leſs than R. aſc. ☉, the difference of the R. aſc. of ☉ and ☽ to be ſubtracted from this arc, remains 52° 6′ 26″, namely the angle of the circle of declination paſſing thro' the centre of ☽ with the meridian of the place. The altitude of ☽ = 38° 20′. The angle of the circle of declination with the vertical = 41° 28′. Parallax of altitude = 47′ 58″. Parall. R. aſc. in parallel circ. = 31′ 45″. Parall. declin. = 35′ 56″.

16. The diſpoſition and reduction of the R. aſcenſions according to prec. 12. now becomes thus:

		R. Aſc. ° ′ ″	Comp. Declin. ° ′ ″
At 20^h 27′ 28″	☽	47 35 10	18 19 28
1 before ♂	☽	47 53 35	18 26 0
20 27 28	☉	48 36 44	18 3 13
1 before ♂	☉	48 37 57	18 3 32
Diff. of R. aſc.	☉	1 13	Diff. of Dec. ☉ 19
	☽	47 36 23	18 19 47
	☽	47 53 35	18 26 0
Of the unmoved	☉	48 37 57	18 3 32
Diff. *a*		17 12	Parall. of Declin. 35 56
Diff. *b*		1 1 34	34 43
Diff. *a* reduced		16 24	Declin. ſeen ☽ 17 43 51
Diff. *b* reduced		58 39	☽ 17 51 17
Parall. of R. Aſc. at 1 before ♂		28 26	☽ 18 3 32
at 20^h 27′ 28″		31 45	Diff. *c* 7 26
	☽	31 45	Diff. *d* 19 41
	☽	54 50	
	☉	58 39	
Diff. *e*		13 5	
Diff. *f*		26 54	

Fig. 4.

17. From the differences *e*, *f*, *c*, *d*, is conſtructed the type and correction after the following method; let the diff. *e* = 13′ 5″ be = *ac*; and diff. *f* = 26′ 54″ = *ad*; let the perpendicular *bc* be = diff. *c* or 7′ 26″; let the perpendicular *fd* be = 19′ 41″ = diff. *d*; and 20^h 27′ 28″ will be the centre of ☽ in *a*; but 4′ before ♂ in *b*; the centre of the ſun unmoved in *f*. The ſeen orbit of the moon is determined by the points *a* and *b*; becauſe it paſſes thro' them. But if *fm* is equal to the ſum of the

the apparent diameters = 32′ 35″, it cuts the part *ma* from the hypothenuſe *b a*, which being converted into time, gives the quantity of correction.

18. If the thing is to be performed by calculation, let *b a* be continued, and from *f* let fall the perpendicular *fg*. In the preſent caſe *ab* = 15′ 2″, *ae* = 30′ 55″, *ge* = 2′ 10″: therefore *ga* = 33′ 5″, *gf* = 3′ 50″, *fm* = 32′ 35″; therefore *gm* = 32′ 21″; and *ga* − *gm* = *ma* = 44″; which quantity being converted into time, is = 1′ 27″. But when ☽ is moved from *a* towards *b*, and the centre of the moon is placed in *a* 20ʰ 27′ 28″, it is plain that this time muſt be added to the time of the beginning found above, that the beginning of the eclipſe may be found true and correct; 20ʰ 28′ 55″.

19 To ſhew the exactneſs of this calculation, let us inveſtigate the diſtance of the centres of ☉ and ☽ to this corrected time of the beginning. For if theſe are equal to the ſum of the apparent ſemidiameters, it is neceſſarily the true point of the beginning; if otherwiſe, it is falſe. The time that paſſes between this point of the corrected beginning and the time of ☌ is = 1ʰ 48′ 36″. With this agrees the motion of ☽ in the ecliptick 54′ 46″ increment. lat. ☽ 4′ 48″; motion of ☉ in longitude 3′ 34″; therefore at the time of corrected beginning, long. ☽ 50° 12′ 12″, N. lat. ☽ 31′ 19″; long. ☉ 51° 3′ 14″; R. aſc. ☉ = 48° 36′ 47″; declin. ☉ = 18° 3′ 14″; R. aſc. ☽ 47° 36′ 4″; declin. ☽ 18° 19′ 46″; diff. between R. aſc. ☉ and ☽ 1° 0′ 43″; diff. between time of corrected beginning and noon 3ʰ 31′ 6″; arc of the equator agreeing with this time = 52° 46′ 30″ = ang. circ. declin. paſſing thro' the centre of ☉ with the meridian of the place. The difference between R. aſc. ☉ and ☽ being ſubtracted from this, there remains for ang. circ. declin. paſſing thro' the centre of ☽ with the meridian = 51° 45′ 47″. Correſponding alt. ☽ = 38′ 33″: ang. circ. declin. with the vertical = 41′ 11″; parall. alt. = 47′ 50″, parall. declin. = 36′ 0″; parall. R. aſc. in parall. circle = 31′ 29″; ſeen declin. ☽ = 17° 43′ 46″; diff. between ſeen declin. ☽ and declin. ☉ = 19′ 28″; diff. between R. aſc. ☉ and R. aſc. ☽, reduced to parts of a great circle, allowing the declin. of the parall. 17° 44′ = 57′ 34″; parallax R. aſc. = 31′ 29″: therefore the diſtance of the places of ☉ and ☽ in this parallel circle = 26′ 5″. If therefore from 26′ 5″ as a baſe, and from 19′ 28″ as a *cathetus*, a rectangular triangle be conſtructed, the hypothenuſe of this triangle will be the diſtance of the centres of ☉ and ☽; but 26′ 5″ = 1565″; the ſquare of which is 2449225: and 19′ 28″ = 1168″, the ſquare of which is 1364224; and the ſum of the ſquares = 3813449, the ſquare root of which is = 1953″, 2″ only leſs than the ſum of the apparent ſemidiameters.

Proof of the correction.

20. The point of this, as determined above, numb. 14, is 22ʰ 43′ 31″. Time of ☌ 21ʰ 57′ 31″; difference, 46′ 0″. To this difference, the motion of ☽ in longit. is 28′ 25″; increment of lat. = 2′ 19″; motion of ☉ in longit. = 1′ 51″; wherefore to 22ʰ 43′ 31″ longit. ☽ = 51° 35′ 13″; latit. ☽ = 38′ 36″; longit. ☉ = 51° 8′ 39″; R. aſc. ☉ =

Correction for the end.

⊙ = 48° 42′ 17″; declin. ⊙ = 18° 4′ 39″; R. aſc. ☽ = 48° 58′ 38″; declin. ☽ = 18° 48′ 49″.

21. The diff. of time between the end of the eclipſe and noon, is 1ʰ 16′ 29″; which being converted into an arc of the equator, is = 19° 7′ 15″. Diff. between R. aſc. ⊙ and ☽ = 16′ 21″; R. aſc. ☽ precedes R. aſc. ⊙; therefore this diff. is to be added, to make 19° 23′ 36″, the angle of the circle of declination paſſing thro' the centre of ☽ with the meridian. This angle with lat. of the Obſervatory of *Paris* and declin. ☽ produces alt. ☽ = 56° 8′; and the angle of the circle of declin. with the vertical = 23° 4′. Hence follows parallax of alt. = 34′ 12″; parall. declin. 31′ 27″; and parall. R. aſc. in a parallel circle = 13′ 24″.

22. The reduction therefore and diſpoſition of R. aſcenſions and declinations is as follows.

		R. Aſc. ° ′ ″		Comp. Decl. ° ′ ″
	In 6 ☽	48 30 21	— — — —	18 38 59
h ′ ″	In 6 ⊙	48 40 24	— — —	18 4 10
At 22 43 31	⊙	48 42 17	— — — —	18 4 39
At 22 43 31	☽	48 58 38	— — — —	18 48 49
Diff between R. aſc. ⊙		1 53	Diff. between decl. ⊙	29
	In 6 ☽	48 32 14	— — — —	18 39 28
	Unmoved ⊙	48 42 14	— — — —	18 4 39
h ′ ″				
At 22 42 31	☽	48 58 39	— — — —	18 48 39
	Diff. *a*	10 3	Parall.	32 38
	Diff *b*	26 24	declin.	31 27
	Diff. *a* reduced	9 33		18 6 50 ☽
	Diff. *b* reduced	25 5	Decl. ſeen.	18 4 39 ⊙
				18 17 12 ☽

23. Diff. *a* is the diſtance of ⊙ unmoved from the firſt place of ☽, and diff. *b* the diſtance of the 2d place of ☽ from the 1ſt in the parallel circle, the decl. of which is 18° 7′. By the parallaxes of R. aſc. the two places of ☽ are now changed into the following, and ſo by adding the parallaxes, the diſtances will be of the

	′ ″
⊙ unmoved =	9 33
☽ in 6 =	20 27
☽ in the end =	38 29

Now if from theſe numbers we ſubtract the leaſt, 9′ 33″, there is left for the diſtance of the place of ☽ in 6 from ⊙ unmoved, 10′ 54″; for the diſtance of ☽ in the end of the eclipſe from ⊙ 28′ 56″; the differences of the declinations ſeen, from the leaſt ſeen, are 2′ 11″, and 12′ 33″.

24. Let

24 Let $q f$ be the portion of a circle parallel to the declination $18^{\circ}\ 7'$; therein let f be the centre of ☉ unmoved; r the place of ☽ in ☌; q the place of ☽ in the end of the eclipſe: wherefore $r f = 10'\ 54''$; $q f = 28'\ 56''$. At the points r and q raiſe the perpendiculars $a r$ and $q v$; ſo that $a r$ may be $= 2'\ 11''$, and $q v = 12'\ 33''$. The right line $m v a g$ being drawn thro' the points v and a, will ſhew the orbit of ☽ ſeen. But if the aperture of the circle is equal to the ſum of the apparent ſemidiameters, in this caſe $= 32'\ 39''$, this will cut off from f a portion of the orbit $m v$, which being converted into time, and added to the time of the end found above, gives the end corrected. Fig. 5.

25. If this is to be done by numbers alone, firſt, let the perpendicular $a r$ be ſubtracted from the perpendicular $v a$, to obtain $v z$. Let the orbit $v a$ be produced, and from f let fall the perpendicular $f g$; hence ariſe 3 ſimilar triangles; $a z v$, $a r n$, and $f n g$. On making the calculation, there comes out for $v a$, $20'\ 48''$; for $a n$ $4'\ 22''$; for $n g$ $6'\ 10''$: conſequently $v g = 31'\ 20''$, and $g f = 3'\ 32''$. But as $m f$ is $= 32'\ 39''$, $m g$ will be $= 32'\ 27''$, therefore $m v = m g - v g = 1'\ 7''$: which quantity being changed into time, is $= 2'\ 28''$: this time being added to the time of the end found above $22^{h}\ 43'\ 31''$, gives at laſt the end of the eclipſe corrected $22^{h}\ 45'\ 59''$.

I made choice of this example, becauſe it is the ſame with that by which *de la Hire* illuſtrated the precepts of his calculation: it will therefore not be amiſs to ſhew it's agreement with the preſent. In *de la Hire*'s calculation, the point of conjunction is ſuppoſed to be according to true time $21^{h}\ 57'\ 15''$; which however is not exact: for according to the *Ludovician* tables, it happens at $21^{h}\ 57'\ 31''$; as we ſettled above. After this ſmall error is corrected, the point of greateſt obſcuration, according to *de la Hire*'s calculation, agrees with ours even in ſeconds, $21^{h}\ 35'\ 26''$; but there is ſome little difference in the beginning, end, and quantity, of the eclipſe *. In that calculation, the perpendicular $L T$ produces at the true time of conjunction 211; and ſo the quantity of the eclipſe is $= 10$ dig. $49'$. The beginning happens at $20^{h}\ 27'\ 29''$, the end, $22^{h}\ 43'\ 23''$. By *de la Hire*'s precept, that beginning needs no correction; which however is true, if an error of $1'$ or $1\frac{1}{2}$ may be ſlighted. But if not, as the thing requires, and the proof of my correction ſufficiently ſhews, the labour of correcting is alſo to be undertaken in *de la Hire*'s calculation. In mine, the beginning found at firſt would agree exactly enough; but becauſe of different altitudes of the moon in the end and beginning, I have aſſumed diverſe apparent ſemidiameters, which *de la Hire* did not do; and therefore to make all things equal, let the apparent ſemidiameter of ☽ be ſet at $16'\ 43''$ in the beginning and end; in which caſe the beginning of my calculation not corrected will be brought back to $20^{h}\ 27'\ 23''$, the end, to $22^{h}\ 43'\ 29''$; therefore my beginning is $6''$ before *de la Hire*'s; and the end follows it at the ſame diſtance; and the quantity of the eclipſe, as we have determined above, exceeds *de la Hire*'s $7'$. Note.

* *Vid.* Tab. *Ludovic.* Edit. *Pariſ.* 1727. p. 48. *in uſu Tabularum.*

When the apparent orbits of ☽, or rather the feigned ones in the preſent calculation, and in that of *de la Hire*, are not right but curves, in this difference that the convexity in *de la Hire*'s may be objected to the point *L* *, and in the preſent the concavity to the point *f* †, it is evident that the perpendicular *L T*, on the length of which the quantity of the eclipſe depends, is greater than it ought to be in *de la Hire*'s calculation; as in mine the ſame perpendicular which is deſigned by *f g* is leſs than it ought to be: therefore if the greateſt exactneſs was to be uſed, the quantity of the eclipſe ought to be reckoned between them both.

Apparent Time.

	d	h	′	″	
IV. 1. *July*	14.	9.	3.	50.	The beginning, which perhaps might be 2″ or 3″ ſooner.
	14.	9.	39.	42.	The firſt little ſpot in the weſtern cluſter, quite covered.
			52.	00.	The biggeſt of that cluſter quite cover'd, yet ſomewhat doubtful for flying clouds.
			10.	12. 08.	The middle one of three conſiderable ſpots towards the eaſtern limb half cover'd. The end could not be preciſely obſerved for flying clouds; at 12. 09. 15. it was not quite over; but at 12. 09. 35. the ſun was clear, and nothing of the eclipſe left.

The Sun's Eclipſe *of* July 14, 1748. *obſerved at* Marlborough Houſe, *with the* 12 *foot refracting Teleſcope, fix'd as a finder to the tube of the great* 12 *foot reflector; by* John Bevis *M. D.* No. 489. p. 521. Oct. *&c.* 1748. *Read* Nov. 10. 1748.

N. B. *The wind was ſo boiſterous, that no phaſes could be meaſured with a Micrometer.*

—— *by Mr* Mark Day, *aated* Luffwick *near* Thrapſton, Northamptonſhire, Oct. 21. 1748. *Ibid.* p. 523.

2. The beginning $9^h\ 1'\ 0''$ *a. m.* The end 0. 5. 25. *p. m.* at $10^h\ 32'$; $10''$ *a. m.* $10°\ 18'$ were dark, which I take to be the greateſt with us. Theſe are apparent times, from a well adjuſted clock (by a meridian drawn *June* 10, on a plate of metal), and corrected to the time of obſervation. Our latitude is $52°\ 27'\ 30''$.

—— *At the Obſervatory Royal at* Berlin, *by* Auguſtine Nathanael Greſchow, *Memb. of the R. Acad. of Sc. at* Berlin, *&c. ibid.* p. 526.

3. 1748. *July* 25. *N. S.* The beginning of the eclipſe was not obſerved, the ſun having been covered with clouds.

	h	′	″	
The *annulus* was completed at	11	52	51	*ante merid.*
—— —— —— broken	11	54	13	
The end of the eclipſe	1	25	9	*poſt. merid.*

The diameter of the ſun was $31'\ 43''$.

* *Vide* Alleg. p. 48. in Tab. *Ludov.* † Fig. 3.

This

This eclipſe was likewiſe obſerved annular at *Francfort* upon the *Oder*, but not ſo exactly as at *Berlin*.

4. Theſe obſervations were made at *Aberdour* caſtle, belonging to the ſaid Earl, whoſe lat. is 56° 4′ N. Mr *le Monnier* having come over from *France* to go to *Scotland*, to obſerve the annular eclipſe of the ſun, *July* 14. 1748. I was deſirous to contribute all that lay in my power to aſſiſt him, and therefore reſolved to go to *Scotland* with the Earl of *Morton*, who was ſo good as to permit us the honour of accompanying him. We arrived at *Edinburgh July* 4. and immediately went to the College, to enquire what preparations were made there, in conſequence of letters we had writ before we left *London*; when Mr *Alex. Monro*, Prof. Anat. informed us, that, upon receipt of ours, he had writ circular letters to all his friends in different parts of the country, to prepare, in the beſt manner they could, for the moſt exact obſervation of this eclipſe.

—— *obſerved by the R. Hon.* James *E. of* Morton, *Mr* le Monnier, *R Aſtron. and Memb. of the* R. Acad. *of* Sc. *at* Paris, *and Mr* Ja. Short, *Fellows of the* R. Soc N°. 490 p 582. Dec. 1748. *Read* Dec. 8. 1748.

We found that the meridian mark, which had been ſettled from obſervations, by the late worthy Mr *Mac Laurin*, was loſt, by the taking down of a chimney, upon which it was fixed; and Mr *Matthew Stewart*, the preſent Profeſſor, having no proper inſtruments, had not as yet re-eſtabliſhed it; which we hoped to do by an inſtrument, which we every day expected from *London*; and Mr *Stewart* having promiſed to make the beſt obſervation he could, we reſolved to ſet out for *Aberdour*, a ſeat of the E. of *Morton*'s, which he readily offered to us, and did us the honour to accompany us thither himſelf, having the ſame deſire and curioſity to do whatever lay in his power to contribute to an exact obſervation.

Aberdour is about 8 miles almoſt N. W. of *Edinburgh*. We choſe this place, as being, by the computations of this eclipſe, at or very near the ſouthern limit of the *annulus*.

In the caſtle of *Aberdour*, 25″ of time weſt of the college of *Edinburgh*, we ſet up a clock, *July* 9. and the weather being cloudy, and our equal-altitude inſtrument and *tranſit* not being yet arrived, we on the 11th made uſe of an equatorial teleſcope of Lord *Morton*'s, to find correſponding altitudes of the ſun, and at the ſame time put up a *gnomon* of 15 feet high.

Being uneaſy that our inſtruments were not come to hand, and reſolving to have a communication with the college of *Edinburgh*, where they had a *tranſit* inſtrument; Lord *Morton* propoſed that two cannon ſhould be fired from the caſtle of *Edinburgh*, one preciſely at 12, and the other at 5′ after 12 on the day of the eclipſe; and the different obſervers in different parts of the country to be advertiſed of this, and to mark down the preciſe time of ſeeing the flaſh, or hearing the ſound of the cannon; ſo that, after having made a geographical map of theſe different parts of the country, and having found the exact meridian of one place, we ſhould be enabled to ſettle the times of all the reſt by the difference of meridians found by this map. This was ſettled

and

and agreed to on the 12th, and an expreſs ſent over to *Edinburgh* with a letter from Lord *Morton* to the Lord Juſtice Clerk, to deſire this favour of General *Bland*, who very readily granted it.

The 13th being a clear day, we took equal altitudes with the equatorial teleſcope, and found our clock gained $1'.46''$ in two days, and that the ſun paſſed the meridian at $12^h\ 7'\ 6''$ by the clock.

July 14th was an exceeding bad morning both for wind and rain; but about 8^h in the morning, the clouds diſperſed, and we had a very clear ſun.

In order to obſerve the eclipſe, Lord *Morton* made uſe of a reflecting teleſcope, 12 inches focal length, magnifying about 40 times. I made uſe of a reflecting teleſcope 4 feet *focus*, magnifying about 120 times; both belonging to Lord *Morton*. Mr *le Monnier* made uſe of a refracting teleſcope, about 9 feet *focus*, which he brought with him from *France*, armed with a micrometer, made after the method of Mr *G. Graham*, by the late Mr *Siſſon* at *London*. Mr *le Monnier* took his ſtation in the garden, under the window of the room where the clock was placed; Lord *Morton* was in the room next that where the clock ſtood; and I was at the window next the clock.

Clock. h	′	″	True Time. h	′	″	
8	55	0	8	47	5	The eclipſe not yet begun. Clouds come on.
8	59	13	8	51	18	Beginning of the eclipſe, found by the following chord.
9	0	42	8	52	47	Firſt view of the eclipſe, then conſiderably advanced.
9	2	30	8	54	35	Meaſured the chord of the part eclipſed; which was found equal to the field of the great reflector.
10	6	10	9	58	12	The illuminated part of the ſun, meaſured by the micrometer, and found $= 7'\ 37''\ \frac{1}{2}$.
10	45	0	10	37	0	Again meaſured, and found $= 7'\ 37''\ \frac{1}{2}$. L. *Morton* judged the middle of the eclipſe, or neareſt approach to an *annulus*, at $10^h\ 17'\ 54''$ apparent time.
11	52	43	11	44	40	The ſame phaſe or chord obſerved as at the beginning, and meaſured both in the teleſcope, as at firſt, and by the micrometer, and found $= 8'\ 25''$ of a great circle, as verified by a baſe after the eclipſe was over, which gives the end as exact as the beginning.
11	56	21	11	48	18	End of the eclipſe by the preceding chord.

Mr *le Monnier* meaſur'd with the micrometer the apparent equatorial diameter of the moon, when ſhe was upon the ſun; which he found $= 29'$

$= 29' 47'' \frac{1}{2}$. He meaſured alſo the apparent vertical diameter of the ſun at noon; which he found $= 31' 40''$. The micrometer, with which he meaſured theſe diameters, was afterwards verified, by a baſe of 2570 feet, and two marks, placed at right angles to it's extremity, at the diſtance of 22 feet from one another.

The flaſh of the firſt cannon fired from the caſtle was ſeen at $12^h 3' 4''$ by the clock; and the flaſh of the ſecond cannon alſo by the clock at $12^h 8' 4''$. The eclipſe was ſo nearly annular, that, at the neareſt approach, the cuſps ſeemed to want about $\frac{1}{7}$ of the moon's circumference to be joined; yet a brown light was plainly obſerved, both by L. *Morton* and myſelf, to proceed or ſtretch along the circumference of the moon, from each of the cuſps, about $\frac{1}{3}$ of the whole diſtance of the cuſps from each cuſp; and there remained about $\frac{1}{3}$ of the whole diſtance of the cuſps not enlightned by this brown light; ſo that we were for ſome time in ſuſpenſe whether or not we were to have the eclipſe annular with us. I obſerved, at the extremity of this brown light, which came from the weſtern cuſp, a larger quantity of light, than in any other place, which at firſt ſurprized me; but afterwards I imagined it muſt have proceeded from ſome cavity or valley made by two adjoining mountains on the edge or limb of the moon. I had often formerly obſerved mountains on the circumference of the moon, more or leſs every-where round it, but never ſaw them ſo plain as during the time of this eclipſe; for we had the air exceeding clear, and free of all agitation, notwithſtanding it blew a perfect hurricane of wind, which began about the middle of the eclipſe; and I remember, in the annular eclipſe of the ſun in the year 1737, it did the ſame. The mountainous inequalities on the ſouthern limb of the moon were particularly remarkable; in ſome parts mountains and valleys alternately; others extended a conſiderable way along the circumference, and ended almoſt perpendicularly like a precipice. L. *Morton* was able to ſee them very eaſily thro' his ſmall reflector.

A little after the middle of the eclipſe, ſome clouds, that ſeemed ſtationary below the ſun, appeared tinged on their upper extremities with all the colours of the rainbow.

During the greateſt darkneſs, ſome people, who were in the garden adjoining to the caſtle, ſaw a ſtar to the eaſt of the ſun; which, when they afterwards told us, and pointed to the place where they had ſeen it, we found muſt have been the planet *Venus*. This ſtar, we were afterwards told, was ſeen alſo at *Edinburgh*, and other places, by a great number of people; but I did not hear of any other ſtars being ſeen. The darkneſs was not great, but the ſky appeared of a faint languid colour. What is pretty remarkable, is, Mr *le Monnier* aſſured us, that when he looked at the ſun with his naked eyes during the middle of the eclipſe, he could obſerve nothing upon the ſun, but ſaw the ſun full, tho' faint in his light. This, I am apt to imagine, may be owing to his being ſhort-ſighted. I obſerved alſo, about the

the middle of the eclipſe, a remarkable large ſpot of light, of an irregular figure, and of a conſiderable brightneſs, about 7′ or 8′ within the limb of the moon next the weſtern cuſp. I thought I loſt this light ſeveral times; but whether this was owing to my ſhutting my eyes, in order to relieve them, or not, I cannot tell. I am told, that the rev. Mr *Irwin* at *Elgin* obſerved the ſame. When I firſt perceived it, I called to Lord *Moreton*, who was in the next room, but he could not ſee it.

Before the eclipſe began, and during the whole time of the eclipſe, the air, as I ſaid before, being exceeding clear, I ſaw thro' the 4 foot reflector the ſurface of the ſun cover'd with ſomething which I had never obſerved before; it ſeemed to be all irregularly overſpread with light, and a faint ſhade, eſpecially towards his equatorial diameter. This appearance was ſo odd, that it is difficult to deſcribe it, ſo as to give an adequate idea of what I ſaw; but if I may be allowed the expreſſion, it ſeemed as it were, curdled with a bright and more duſky light or colour. This appearance was permanent, and regularly the ſame; and if in any degree ſeen before, may have given riſe to *faculæ* having been ſeen in the ſun; but to me the whole ſun's body ſeemed to be more or leſs covered with it. I looked with all the attention poſſible, to ſee if I could obſerve the body or limb of the moon before ſhe touched the ſun, and alſo after ſhe left it, and was intirely off the ſun, but could ſee nothing at all of any ſuch appearance. I mention it to ſatisfy Mr *de Liſle*, who publickly deſired this might be attended to.

The barometer had been falling for ſeveral days before the eclipſe, and even that morning, when it was at 29.2 inches. But during the eclipſe it began to riſe.

				Diviſions.
July 11.	at 8^h		in the morning the thermometer ſtood at -	54
	at	12 0	or noon at - - - - - -	56
	at	4 0	*p. m.* at - - - - - -	60
July 12.	at	11 0	*a. m.* it ſtood at - - - - -	57
	at	12 0	or noon at - - - - -	58
July 13.	at	8 30	*a. m.* it ſtood at - - - - -	$55\frac{3}{4}$
	at	1 0	*p. m.* at - - - - - -	$57\frac{1}{2}$
July 14.	at	8 0	*a. m.* at - - - - - -	56
	at	8 53	at - - - - - - -	57
	at	9 7	at - - - - - - -	$57\frac{1}{2}$
	at	9 20	at - - - - - - -	$57\frac{1}{4}$
	at	10 8	at - - - - - - -	57
	at	10 26	at - - - - - - -	$56\frac{3}{4}$

All theſe obſervations of the thermometer were taken when it ſtood in the ſhade; and the times are by the clock. Immediately after the middle of the eclipſe, the thermometer, when expoſed to the ſun for the ſpace of 10′ of time, roſe only $\frac{1}{2}$ a diviſion.

Thermometer

	Diviſions
Thermometer ſtill expoſed to the Sun,	
at $10^h\ 46'\ 00''$, ſtood at	58½
at 10 51 30 at	62
at 10 57 30 at	63½
at 11 4 00 at	66
at 11 10 00 at	70
at 11 34 00 at	75¼
Thermometer replaced in the ſhade after this laſt obſervation,	
at $12^h\ 54'$ ſtood at	60½
at 1 28 at	61
at 5 50 at	59
at 7 30 at	58½
July 15. Thermometer at 8^h *a. m.* ſtood at	56
at 9 at	57
at 10 at	60

Theſe obſervations were made with a thermometer of *Fahrenheit*'s ſcale, the diviſions of which were very ſenſible. We did not at all perceive or feel any greater degree of cold, during the eclipſe, than we felt before it began.

The weather being very bad at *Edinburgh*, Mr *M. Stewart*, Prof. Math. could make no obſervations of the Eclipſe; he only ſaw the end at $11^h\ 50'\ 34''$ true time; and even then the ſun was ſomewhat cloudy: he took however the ſun's *tranſit* over the meridian (as then ſuppoſed) at $12^h\ 4'\ 42''$ by his clock, and heard the ſecond cannon fired from the caſtle at $12^h\ 7'\ 48''$ by the clock. We afterwards, in a few days, examined his meridian mark with a very exact equal altitude inſtrument by 3 ſeveral correſpondent obſervations; and found his mark $3'\ 22''$ of time to the W. of the true meridian. The college is about 2500 feet diſtant from the caſtle Eaſtward.

The Rev. Mr *Bryce*, at *Aldiſton*, about 6 miles to the W. of *Edinburgh*, lat. $55°\ 55'\frac{1}{2}$ N, obſerved with a reflecting teleſcope, 9 inches *focus*,

	h	*l.*	II
The beginning of the eclipſe at	8	52	30
Upper horn or cuſp vertical, at	9	5	0
Hitherto the weſtern cuſp lower than the eaſtern.			
The two cuſps horizontal at	10	13	10
The weſtern cuſp aſcends very faſt at	10	14	10
The weſtern cuſp vertical at	10	16	15
The cuſp which was juſt now vertical, now becomes Eaſt, and about 30° from the zenith to the Eaſt at	10	17	10
The middle of the eclipſe as near as he could judge at	10	17	40
The lower cuſp at the nadir, and very ragged and uneven at	10	24	45

	h	′	″
The ſame cuſp ſtill in the ſame poſition at - - - -	10	32	5
The ſame cuſp ſeems to begin to move towards the W. at	10	43	35
The motion of this cuſp ſcarce ſenſible at - - - - - -	10	55	45
The other cuſp middle between the *zenith* and the *nadir* towards the E. at - - - - - - - - - - - -	11	0	25
End of the eclipſe, the ſun being quite clear at - - -	11	48	40

I ſhall ſet down the following obſervations of this eclipſe juſt as they came to my hand when in *Scotland*, without making any other remark, than that, from the diſagreement among themſelves, they do not all of them ſeem to have been made with due accuracy and attention; for want, I ſuppoſe, of ſufficient practice in this kind of obſervations.

William Crow, Eſq; at his houſe of *Netherbyres* near *Haymouth*, lat. 55° 51′ N. ſays,

	h	′	″
The eclipſe began at - - - - - - - - - - - -	8	55	0
Half of the ſun eclipſed at - - - - - - - - - -	9	50	0
Middle of the eclipſe, $\frac{1}{6}$ of the ſun's limb cover'd by the moon at	10	25	0
End of the eclipſe at - - - - - - - - - - - -	11	55	0

Mr *John Mair*, at *Air*, lat. 55° 30′ N. ſays, the eclipſe began 8^h 45′; but that, by reaſon of clouds, he could make no other particular obſervation; only that, by a view he had of the ſun ſome little time before the end, he thinks the end of the eclipſe might be about 11^h 48′.

Mr *Mark*, teacher of Math. at *Dundee*, lat. 56° 25′ N. obſerved,

	h	′	″
The beginning of the annular appearance at - - - -	10	16	44
End of the annular appearance at - - - - - - -	10	23	8

He ſays, the beſt obſervations make the *annulus* a ſmall matter narrower on the upper than lower ſide; by which it appears the centre of the eclipſe was to the N. of *Dundee*.

Mr *John Stewart*, Prof. Math. at *Aberdeen*, writes, that by an obſervation made at *Monroſs*, lat. 56° 41′,

	h	′	″
The annular appearance began at - - -	10	20	0
Annulus ended at - - - - -	10	24	30
End of the eclipſe at - - - - -	11	52	45

And that, by an obſervation made at a place about 18 miles S. W. of *Aberdeen*.

The

	h	′	″
The eclipſe began at	8	52	0
Middle at	10	21	0
End at	11	52	0

And that at *Aberdeen*, lat. 57° 11′ N.

	h	′	″
The eclipſe began at	8	55	33
Middle of the eclipſe, and annular appearance, as near as he could judge, at	10	23	3
End of the annular appearance at	10	24	48

He writes alſo, that he received an account from Mr *Reid*, Miniſter at *New Macchar*, about 7 Miles N. W. of *Aberdeen*, who obſerved

	h	′	″
The beginning of the annular appearance at	10	18	28
And the end of the eclipſe at	11	49	3

Mr *Stewart* ſays, that, by comparing his obſervation at *Aberdeen* with this of Mr *Reid*'s, he apprehends he is in a miſtake as to his judging of the middle of the eclipſe, and annular appearance; and reckons, that the annular appearance began at *Aberdeen* at 10^h 19′, and ended as above. By which the total duration of the *annulus* was 5′ 48″: and the end of the eclipſe at *Aberdeen* was at 11^h 49′ 33″.

The Rev. Mr *Irwin*, at *Elgin*, lat. 57° 34′, ſays, the eaſtern limb of the moon touched or entered on the weſtern limb of the ſun at 8^h 57′; tho' he ſuſpects it began a little ſooner (another having taken the teleſcope out of his hand); for when he looked, the moon was a little advanced on the diſc of the ſun about 30° from the *zenith* of the ſun towards the W.

	h	′	″
The eaſtern cuſp in the *zenith* of the ſun at	9	6	10
Eaſtern limb of the moon reached the centre of the ſun at	9	39	0
The *annulus* began about 30° from the *zenith* of the ſun weſtward at	10	20	0
The *annulus* appeared moſt perfect at	10	22	45

Tho', as nearly as he could diſcern, he thought it a little narrower on the S. W. limb of the ſun, than it was on the oppoſite ſide. From hence it ſhould appear, that the centre of the eclipſe was to the ſouthward of *Elgin*.

The *annulus* was obſerved to break on the S. E. limb of the ſun, about 30° from the *nadir*, at 10^h 25′ 30″.

Before the joining of the cuſps of the ſun, as alſo at the breaking of the *annulus*, he ſays, he obſerved a quick tremulous motion, and ſeveral

irregular bright ſpots between the cuſps, which diſappeared in a few moments; and he thought the moon's body paſſed quicker about the time of the *annulus* (eſpecially as it was forming), than at any other time during the eclipſe.

Before the weſtern limb of the moon reached the centre of the ſun's diſc, the ſun was hid under a cloud, and continued ſo, till within ſome little time of the end of the eclipſe, which happened at $11^h\ 50'$.

There was no cloud all the time of the formation of the *annulus*, or the duration of it; and he thinks he is pretty right, as to the time of its continuance; for both the formation and breaking were very ſenſibly to be obſerved, and paſſed in a moment; affording a very pleaſant ſight, by the irregular tremulous ſpots of the ſun.

He ſays, the darkneſs, during the *annulus*, was not ſo great as a little before and after; and, when greateſt, was only ſomewhat duſkiſh, but obſervable. Some ſaw a ſtar to the eaſt of the ſun; but he ſaw it not, nor any preſent with him. He was told of it after his obſervation was over.

He ſays, that by an obſervation taken of the ſun that day at noon, he found that his clock was ſomewhat leſs than $1'$ faſter than the ſun. He ſays alſo, that he obſerved this eclipſe with a teleſcope 3 feet long, and that he had a very good burning-glaſs; but that it had little force, during the *annulus*, and ſome ſhort time before and after.

Mr *Duncan Frazer* writes to Mr *Monro*, Prof. Anat. *Edin.* that he went to the houſe of *Culloden*, lat. $57°\ 29'$ N. on purpoſe to obſerve the eclipſe; it having been ſaid, that the centre of the eclipſe would paſs there; and after having adjuſted his clock by the regulator-clock of a watch-maker at *Inverneſs*, he obſerved the eclipſe with a teleſcope 5 feet long, and found

	h	′	″
The beginning preciſely at	8	37	36
Beginning of the *annulus* at	10	0	10
End of the *annulus* at	10	5	10
End of the eclipſe at	11	29	30

By comparing his obſervation with that ſent him by Mr *Irwine* at *Elgin*, he imagines his clock was not ſet to true time, ſince there is ſo great a difference, and more than the difference of longitude between the two places will allow; it being no more than 26 computed miles, and nearly in the ſame parallel of latitude.

Mr *Murdock Mackenzie* (who has for ſome years paſt been making a ſurvey of the iſlands of *Orkney*, and whoſe abilities for ſuch an undertaking give us hopes he will for the future, free navigators of a great many melancholy diſaſters, which formerly happened in thoſe ſeas, thro' the want of true charts) made the following obſervation at *Kirkwall* in the iſland of *Pomona* in *Orkney* the latitude of which is $58°\ 58'$ N.

Beginning

	h	′
Beginning of the eclipſe about - - - -	8	40
End of the eclipſe about - - - -	11	37

He ſays, that by reaſon of clouds, he could not be perfectly exact, as to the preciſe time of beginning or ending; but adds, that the beginning cannot be more than 4′ wrong, nor the end more that 2′. He ſays, he is ſure he did not ſee it annular, but that there remained about $\frac{1}{4}$ or $\frac{1}{5}$ of the ſun's circumference intercepted at the middle of the eclipſe.

P. S. It having been an opinion pretty generally received, that the darker parts of the moon's ſurface are water, I take this opportunity to remark, that though thoſe leſs lucid ſpaces are for the moſt part, to appearance, evenly extended ſurfaces, when teleſcopes of ſmall magnifying powers are made uſe of, yet, when they are examined with larger magnifiers, it is eaſy to diſcern on them many protuberances in a longitudinal direction; and that theſe riſings are really elevated above the common plane ſurface, is paſt all queſtion, from their projecting ſhadows, always oppoſite to the ſun: Moreover, they are of the very ſame colour as the plane they ariſe from, of the like ſmooth ſurfaces, without any ſenſible aſperities; and invariably the ſame, under the like poſitions of the ſun to the moon, at leaſt as far as I have been able to diſcover in 12 or 15 years frequent obſervations of them.

5 Being prepared for the obſervation of this eclipſe with a reflecting teleſcope about 2 feet long, and being ſufficiently acquainted with the motion of an aſtronomical pendulum, which I had uſed in my voyage to *Peru*, in making ſeveral obſervations. I obſerved the beginning of the eclipſe to have happened in true time about - -$8^h\ 49'\ 6''$

The ſpot *a b* in the diſk of the ſun between it's eaſtern and ſouthern parts, which could then be eaſily diſcerned, becauſe there was no other near it, began to be immerged - - - - } 10 23 $44\frac{1}{2}$

The total immerſion of this ſpot - - - 24 $46\frac{1}{2}$

I was not able to obſerve either the emerſion of this ſpot, or the end of the whole eclipſe; for the ſun, being in it's greater altitude above the horizon, made the uſe of the teleſcope inconvenient. Nor could the particular number of the digits be conveniently determined for ſeveral reaſons.

About the beginning of the eclipſe, part of the lunar diſk had a colour inclining to red, which afterwards increaſed with the increaſe of the eclipſe. The day was clear, and the atmoſphere free from clouds, and ſo it continued till evening. When the eclipſe was in the middle, there was ſome diminution of light; and it's reflexion was obſerved to be ſomething weaker; and the air was obſerved to have loſt ſomething of it's uſual heat; which alteration continued from $\frac{1}{2}$ an hour after the beginning of the eclipſe to the end.

Fig. 6. *S* the ſun, *L* the moon, *a* the limb of the ſun at which the immerſion began, *b* the limb of the moon perceived before the beginning of the eclipſe.

——*at* Madrid, *by* Don Antonius de Ulloa, S. S. R. N° 491, p. 10, Jan. &c. 1749. *Preſented* Jan. 26, 1748-9. Fig. 7.

Fig. 7.

Fig. 7. *N O S L* the ſolar diſk; *L* the eaſt, *O* the weſt, *N* the northern part of the diſk, *S* the ſouthern part, *a b* the ſpot obſerved in the diſk of the ſun; *c* another between the N, and the W. to which the eclipſe did not reach; *d* other numerous ſpots in the middle of the ſolar diſk.

Solar Eclipſe, Jan 8. 1750. N. S. *at* Rome, *by Mr* Chriſtopher Maire. N° 494. p. 322. Jan. &c. 1750. *Read* Feb. 1. 1749.

V. 1. Beginning by a reflector of Mr *Short*, *Jan.* 7.

	h	′	″
The firſt ſpot covered	20	34	35
The reſt could not be obſerved for the clouds	20	49	50
Quantity of the eclipſe 7 dig. 48 min.	21	49	4
Again more exactly - 7 - 43	21	51	28
The ſun appears for a moment; horns nearly horizontal	21	56	15
Two digits remain eclipſed	22	55	37
One digit exactly	23	3	42
End of the eclipſe	23	11	32

The obſervation was made with a 7 foot tube, 2610 parts of the micrometer juſt claſping the ſun's diameter. The place of obſervation was in lat. 41° 54′ 0″, and 4″ of time E. of *St Peter*'s.

——*at the Obſervatory at* Berlin, *by M.* Griſchow, *jun. and M.* Kies. *Tranſlated from the* French. ibid. p. 339. *Read* Feb. 15. 1749-50.

2. The beginning was at 8 59 19½ true time.
The end of the eclipſe at 11 20 5½
The whole duration 2 20 46

The obſervations were made with the greateſt exactneſs, the weather being as favourable as could be wiſhed, the whole time.

M. *Euler* obſerved in his own houſe, which ſtands a little to the W. of the S. W. of the Obſervatory, at the diſtance of 190 *Rhinland* yards (*verges*) in a ſtrait line, that

	h	′	″
The beginning was at	8	58	30 true time.
And the end at -	11	19	50
The whole duration	2	21	0

That is, 34″ more than at the Obſervatory.

The diameter of the umbra was 6½ *Rhinland* inches.

Letter from Mr Richard Dunthorne, *to the Rev. Mr* Charles Maſon, *F. R. S. and* Woodwardian *Profeſſor of* Nat. Hiſt. *at* Cambridge, *concerning the* Moon's *Motion.* N°482. p. 412. Jan. &c. 1747. *dated* Cambridge, Nov. 4. 1746. *Read* Feb. 5. 1746/7.

VI. In the preface to my lunar tables, I hinted, that one uſe of publiſhing thoſe tables would be, the aſſiſting of perſons deſirous farther to rectify the lunar Aſtronomy, by enabling them more readily to compare the *Newtonian* theory with obſervations. Since the publiſhing thoſe tables, I have ſpent ſome time myſelf in that compariſon; and here ſend you the reſult, that you may communicate it to the *R. Soc.* if you think it deſerves to be made publick.

As the motion of every ſecondary planet muſt partake of the errors in the theory of its primary, I thought proper, before I undertook the examination of the lunar numbers, to compare thoſe of the ſun with obſervations. I compared ſeveral ſets of Mr *Flamſtead*'s obſervations, after the method he himſelf teaches *, which, for many reaſons, I think the beſt method hitherto uſed; and, with the concurrence of a gentleman well ſkilled in theſe matters, determined the mean motion of the ſun at *Greenwich*,

* Prolegom. Hiſt. Cœleſt. p 133, *& ſeq.*

wich the laſt day of *Dec.* at noon, 1700, *O. S.* ♑ 20° 43′ 40″ of its apogee, ♋ 7° 30′ 0″, and the greateſt equation of the ſun's centre 1° 55′ 40″; which, I am fully perſwaded, are very near the truth.

The theory of the ſun being thus ſettled, I proceeded to examine the elements of the lunar Aſtronomy. I began with obſervations of lunar eclipſes about the equinoxes, when the apogee of the moon was in the ſun's quadratures; becauſe at thoſe times I could conceive the moon's motion affected with no inequality: but the annual one, called by *Newton* the firſt equation, and the elliptic one, called *proſthaphæreſis:* From a compariſon of ſuch obſervations I obtained the moon's mean longitude, which came out 1′, at leaſt, greater than in the tables, and very nearly as *Newton* has it in the laſt edition of his *Principia.*

I went on to examine the place and motion of the apogee, and theory of the increaſe and decreaſe of the eccentricity, as well as the greateſt and leaſt eccentricities themſelves (from the beſt obſervations, and beſt ſituated that I could procure) all which agreed ſo well with the tables, about the ſun's mean diſtances, that I dare venture to make no alteration therein: indeed I think the 6th equation does not ſo well account for the variation of the motion of the apogee, and change of the eccentricity, according to the greater or leſſer diſtance of the ſun from the earth; and therefore I ſet myſelf to compute what change this difference of the ſun's action upon the lunar orbit would introduce in the moon's place in every ſituation of the ſun and lunar orbit; and found, after many tedious computations, that the ſun being in apogee, this change, where greateſt, would amount to about 4′, and to 4′ 16″, when the ſun is in perigee. In other diſtances of the ſun from the earth, this greateſt change is proportional to the difference of the cubes of the mean and preſent diſtances; and in every Situation of the moon, and of her orbit, the preſent is to the greateſt equation nearly as the ſine of the exceſs of the moon's mean anomaly above twice the annual argument to *radius.* It increaſes the moon's longitude, when the ſun is in his

{ *Apogeon* / *Perigeon* } Semicircle, and that exceſs { leſs / greater } than 180°; and diminiſhes it when otherwiſe *.

In fine, I compared the theory of the moon, as to her longitude, with ſeveral obſervations, as well in the octants and ſemi-octants, as in the ſyzigies and quadratures, and found ſuch an agreement when the above corrections were made, as ſeemed rather to be wiſhed than hoped for, conſidering the many inequalities wherewith the ſun's action diſturbs the motion of the moon, and the defects to which the beſt obſervations I have hitherto met withal are liable.

I have compared 100 obſerved longitudes of the moon with the tables; *viz.* 25 eclipſes of the moon, all, except the firſt, taken from *Flamſtead's* *Hiſtoria*

* If this equation be increaſed and diminiſhed in a direct *ratio* of the moon's horizontal parallax, it will become more exact. And I think, if it were always diminiſhed by $\frac{1}{4}$ or perhaps $\frac{1}{3}$ part, it would agree better with obſervations.

Hiſtoria Cœleſtis, the *Philoſ. Tranſ.* and the *Mem.* of the *R. Acad.* of *Sc.* the 2 great eclipſes of the ſun in 1706 and 1715: 25 ſelect places of the moon from *Flamſtead's Hiſtoria Cœleſtis*, and 48 of thoſe longitudes of the moon computed from *Flamſtead's* obſervations (by Dr *Halley* as I ſuppoſe) printed in the firſt edition of the *Hiſtoria Cœleſtis*. They are as follow:

25 Eclipſes of the Moon, and 2 of the Sun, compared with the Tables corrected as above.

	Place of Obſervation.	A.D.	Apparent Time at Greenwich.				☽'s true Place obſerved.				M. Anom. ☉		M. Anom. ☽		Ann. Arg.		☽'s Place computed.				Diff. from Obſervat.		
				h	′	″	s	°	′	″	s	°	s	°	s	°	s	°	′	″		′	″
a	Dantzick	1652	Septem. 7.	6	21	35	11	25	25	51	2	20	6	2	0	3	1	25	26	10	+	0	19
		1670	Septem. 18.	14	36	48	0	6	13	29	3	1	6	0	0	$0\frac{1}{2}$	0	6	16	37	+	3	8
	Greenwich	1678	Octob. 19.	8	21	54	1	6	47	9	4	1	7	$16\frac{1}{2}$	2	2	1	6	46	38	−	0	31
		1682	Febr. 11.	10	58	52	5	3	47	3	7	25	6	$28\frac{1}{2}$	1	14	5	3	46	14	+	0	49
	Paris	1684	Decem. 11.	10	47	6	3	1	11	20	5	24	1	8	7	16	3	1	13	7	+	1	47
	Dantzick	1685	Novem. 30	10	35	30	2	19	40	0	5	13	11	$26\frac{1}{2}$	5	25	2	19	39	46	−	0	14
	Dublin	1686	Novem. 19.	11	13	11	2	8	11	57	5	2	10	$12\frac{1}{2}$	4	$4\frac{1}{2}$	2	8	11	28	−	0	29
	Greenwich	1689	Septem. 18.	14	30	37	0	6	32	8	3	1	4	24	10	8	0	6	34	16	+	2	8
		1690	March 14.	9	56	30	6	4	37	11	8	25	9	18	3	$15\frac{1}{2}$	6	4	37	28	+	0	17
		1696	May 6.	12	2	0	7	26	53	14	10	18	3	$1\frac{1}{2}$	8	28	7	26	53	40	+	0	26
	Paris	1699	March 5.	7	13	44	5	25	32	27	8	16	8	27	3	1	5	25	32	14	−	0	13
⊙	New England	1703	Decem. 11.	18	29	8	3	0	49	56	5	23	11	$24\frac{1}{2}$	5	23	3	0	48	51	−	1	5
	Marſeilles	1706	May 10.	21	13	57	1	20	46	44	10	12	6	22	7	6	1	20	46	32	−	0	12
	Greenwich		Octob. 10.	7	10	0	0	28	0	20	3	22	5	27	11	25	0	27	58	47	−	1	33
		1707	April 5.	13	39	0	6	26	18	33	9	17	11	9	5	3	6	26	19	0	+	0	36
		1708	Septem. 18.	9	10	25	0	6	39	23	3	1	2	12	8	7	0	6	39	19	−	0	4
		1712	January 12.	7	34	0	4	2	57	47	6	24	1	$17\frac{1}{2}$	7	$25\frac{1}{2}$	4	2	58	37	+	0	50
	Paris	1713	Novem. 20.	15	27	30	2	9	58	21	5	3	9	21	3	17	2	9	56	56	−	1	25
⊙	London	1715	April 22.	21	11	5	1	12	0	22	10	3	6	$12\frac{1}{2}$	6	22	1	11	59	36	−	0	46
	Paris	1717	March 15.	15	7	4	6	6	23	50	8	26	8	$22\frac{1}{2}$	2	27	6	6	23	31	+	0	19
		1719	Auguſt 18.	8	22	46	11	5	42	8	1	29	10	28	4	20	11	5	42	39	+	0	31
		1722	June 17.	13	46	10	9	6	47	43	11	29	5	$8\frac{1}{2}$	10	25	9	6	47	33	−	0	10
		1724	Octob. 20.	15	40	40	1	9	0	0	4	2	5	$25\frac{1}{2}$	11	22	1	8	58	50	−	1	10
		1729	February 2.	8	42	55	4	25	13	39	7	16	3	25	9	14	4	25	14	40	+	0	1
			July 28.	13	0	0	10	16	15	28	1	9	8	3	2	15	10	16	16	26	−	0	58
		1731	June 8.	13	47	51	8	28	9	58	11	19	4	27	10	11	8	28	8	45	−	1	13
		1732	Novem. 20.	9	40	25	2	10	3	54	5	3	7	8	1	24	2	10	0	56	−	2	58

25 Places of the Moon, computed by myself from *Flamstead*'s Observations, compared with the Tables.

	A D.	Apparent Time at Greenwich				☽'s true Place observed.				M. Anom. ☉		M. Anom. ☽		Ann. Argt.		☽'s Place computed.				Diff. from Observat.		
			h	′	″	s	°	′	″	s	°	s	°	s	°	s	°	′	″		′	″
	1684	March 13.	8	9	8	2	28	48	40	8	25	2	25	11	19	2	28	50	44	+	2	4
	1693	March 6.	7	22	48	3	16	43	12	8	17	3	11½	11	7	3	16	44	0	+	0	48
		Octob. 11.	18	12	34	3	28	34	2	3	24	2	27	5	15	3	28	38	21	+	4	19
	1694	February 27.	10	29	16	4	27	27	31	8	10	3	12	9	20	4	27	26	48	−	0	43
		August 23.	11	13	54	11	0	19	11	2	5	8	2	2	21	11	0	21	41	+	2	30
	1694	Septem. 15.	5	34	13	8	27	1	34	2	27	6	10	3	11	8	27	1	41	+	0	7
		Septem. 21.	10	50	31	11	22	47	41	3	3	9	3½	3	17	11	22	49	16	+	1	35
		Decem. 13.	6	2	53	0	6	28	43	5	25	8	26½	6	0	0	6	29	58	+	1	15
	1695	February 8.	3	55	22	1	5	14	3	7	21	9	7½	7	22	1	5	12	14	−	1	49
		July 9.	5	56	14	7	2	2	42	0	20	3	14	0	2	7	2	3	4	+	0	22
		Septem. 8.	8	30	26	10	3	0	12	2	20	5	21	1	24	10	2	59	50	−	0	22
	1696	January 16.	17	29	2	7	5	4	20	6	29	2	29	5	20½	7	5	6	11	+	1	51
		March 4.	9	8	13	4	12	2	24	8	16	11	8	7	3	4	12	0	59	−	1	25
	1697	February 18.	6	29	47	2	20	52	29	8	2	8	19	5	10	2	20	51	7	−	1	22
		19.	7	22	57	3	4	18	16	8	3	9	2½	5	11	3	4	17	54	−	0	22
		Septem. 15.	7	54	40	10	1	7	8	2	28	3	21½	11	9	10	1	5	38	−	1	30
	1698	Septem. 8.	11	2	35	11	12	15	6	2	21	3	24	9	23	11	12	15	27	+	0	21
		Novem. 27.	3	49	54	10	11	33	39	5	9	2	3½	0	3	10	11	31	13	−	2	26
	1699	March 5.	12	8	23	5	28	15	59	8	16	8	29½	3	1	5	28	16	11	+	0	12
	1701	Septem. 28.	6	55	23	9	27	13	17	3	10	9	9	6	8	9	27	12	49	−	0	28
	1702	Octob. 16.	6	16	49	10	3	25	41	3	28	8	11½	5	13	10	3	26	27	+	0	46
	1703	Septem. 13.	11	58	50	11	28	50	30	2	25	8	28	3	3	11	28	53	30	+	3	0
		Octob. 6.	6	28	39	9	27	1	44	3	18	7	4	3	23	9	27	0	38	−	1	6
	1706	Octob. 10.	11	11	10	1	1	11	15	3	22	6	0	11	25	1	1	10	17	−	0	58
	1714	Septem. 6.	6	34	7	9	3	44	13	2	18	3	17	0	0	9	3	45	24	+	1	11

48 Places of the Moon, computed by Dr *Halley*, from *Flamstead*'s Obſervations, compared with the Tables.

A. D.	Apparent Time at Greenwich				☽'s true Place obſerved.				M. Anom. ☉.		M. Anom. ☽.		Ann. Arg.		☽'s Place computed.				Diff. from Obſervat.		
			h	′	″	s	°	′	″	s	°	s	°	s	°	s	°	′	″		′	″
1689	Novem.	16.	11	59	0	2	6	12	43	4	29	6	1	0	0	2	6	14	40	+	1	57
	Decem.	9.	6	1	0	11	28	48	54	5	21½	3	21	0	20½	11	28	48	40	—	0	14
		10.	6	46	35	0	12	46	49	5	22½	4	4	0	21½	0	12	47	14	+	0	25
		12.	8	26	33	1	12	13	8	5	24½	5	0½	0	23	1	12	10	39	—	2	29
		13.	9	24	30	1	27	38	36	5	25½	5	14	0	24	1	27	36	16	—	2	20
		16.	12	42	0	3	15	14	54	5	28½	6	24	0	27	3	15	15	48	+	0	54
1690	January	4.	3	3	46	11	11	12	59	6	17	2	24	1	14	11	11	13	8	+	0	9
		6.	4	30	15	0	8	13	31	6	19	3	20½	1	16	0	8	14	36	+	1	5
		10.	7	59	22	2	6	11	20	6	23	5	14½	1	19½	2	6	10	37	—	0	43
		12.	10	8	49	3	7	5	21	6	25	6	12½	1	21	3	7	3	59	—	1	22
b		13.	11	14	0	3	22	36	28	6	26	6	25½	1	22	3	22	35	19	—	1	9
	February	2.	2	25	39	0	3	57	24	7	15½	3	5	2	10	0	3	56	9	—	1	15
		5.	4	51	10	1	16	31	33	7	18½	4	26	2	12½	1	16	34	15	+	2	42
		7.	6	48	17	2	15	58	14	7	20½	5	24	2	14½	2	15	59	13	+	0	59
		8.	7	51	54	3	0	56	20	7	21½	6	8	2	15½	3	0	56	31	+	0	11
		10.	9	56	26	4	0	55	24	7	24	7	6	2	17	4	0	55	1	—	0	23
		11.	10	52	31	4	15	42	16	7	25	7	20	2	18	4	15	41	49	—	0	27
		14.	13	19	31	5	28	12	40	7	28	9	2	2	21	5	28	11	44	—	0	56
		19.	17	3	55	8	2	23	26	8	3	11	12	2	25½	8	2	21	17	—	2	9
		21.	18	45	37	8	27	1	25	8	5	0	10	2	27½	8	26	59	6	—	2	19

	1690	February	22.	19	37	40	9	9	21	32	8	6	0	24	2	28	9	9	23	26	+	1	54
		March	7.	5	50	38	2	26	22	50	8	18	6	12	3	9	2	26	25	20	+	2	30
			11.	9	43	23	4	24	31	45	8	22	8	8	3	13	4	24	33	15	+	1	30
			12.	10	33	9	5	8	31	54	8	23	8	22	3	14	5	8	33	11	+	1	17
			13.	11	18	30	5	22	16	37	8	24	9	$5\frac{1}{2}$	3	15	5	22	17	26	+	0	49
			14.	12	2	18	6	5	44	39	8	25	9	$19\frac{1}{2}$	3	$15\frac{1}{2}$	6	5	45	18	+	0	39
			25.	20	54	54	10	24	54	20	9	$6\frac{1}{2}$	2	21	3	$25\frac{1}{2}$	10	24	53	20	−	1	0
b		April	7.	7	48	46	4	20	8	0	9	19	8	5	4	6	4	20	9	18	+	1	18
			8.	8	36	30	5	3	59	40	9	20	8	19	4	7	5	4	0	34	+	0	54
			9.	9	22	0	5	17	34	42	9	21	9	2	4	8	5	17	35	40	+	0	58
			10.	10	5	19	6	0	54	50	9	22	9	16	4	9	6	0	55	35	+	0	45
			11.	10	47	56	6	14	0	8	9	23	9	29	4	10	6	14	2	13	+	2	5
			12.	11	30	56	6	26	55	56	9	24	10	$12\frac{1}{2}$	4	11	6	26	56	55	+	0	59
			13.	12	17	31	7	9	41	19	9	25	10	26	4	12	7	9	42	20	+	1	1
			14.	13	4	0	7	22	16	0	9	26	11	$9\frac{1}{2}$	4	$12\frac{1}{2}$	7	22	17	34	+	1	34
			15.	13	52	44	8	4	45	10	9	27	11	23	4	$13\frac{1}{2}$	8	4	44	47	−	0	23
			18.	16	26	47	9	11	43	52	10	0	1	$3\frac{1}{2}$	4	16	9	11	44	53	+	1	1
			20.	18	5	47	10	6	30	55	10	2	2	0	4	18	10	6	33	2	+	2	7
			22.	19	37	30	11	2	8	6	10	4	2	27	4	20	11	2	8	45	+	0	39
			24.	21	6	38	11	29	14	33	10	6	3	$23\frac{1}{2}$	4	$21\frac{1}{2}$	11	29	14	30	−	0	3
		May	14.	13	30	30	8	25	6	53	10	$25\frac{1}{2}$	0	7	5	8	8	25	8	48	+	1	55
b			22.	19	41	40	0	6	50	26	11	$3\frac{1}{2}$	3	23	5	15	0	6	49	43	−	0	43
		June	3.	6	2	13	5	22	31	45	11	15	8	19	5	25	5	22	32	6	+	0	21
		Septem.	1.	6	48	53	9	2	27	42	2	$13\frac{1}{2}$	11	15	8	12	9	2	31	20	+	3	38
			8.	12	16	42	0	1	44	51	2	21	2	22	8	18	0	1	44	18	−	0	33
	1691	February	23.	3	55	10	1	19	39	56	8	6	3	16	1	18	1	19	40	45	+	0	49
		March	11.	18	27	51	9	7	30	5	8	$22\frac{1}{2}$	10	$23\frac{1}{2}$	2	2	9	7	30	23	+	0	18
		Septem.	22.	8	51	3	10	19	26	24	3	4	11	16	7	19	10	19	28	32	+	2	8

a, the time of the middle of this eclipſe here ſet down is from the beginning and end; but *Hevelius* ſays he could not obſerve the beginning exactly. Several intermediate *phaſes* compared together ſhew the middle to have been about 4′ ſooner; to which the moon's place computed is $0^s\ 6^\circ\ 14'\ 3''$ and diff. $+\ 34''$.

b, *b*, *b*, the moon's places, obſerved on *Feb.* 2. *April* 7. and *May* 22. are computed by myſelf, from the obſervations; there being manifeſtly errors, either of the computation or preſs, in thoſe printed in the *Hiſt. Cæleſtis.*

Several obſerved latitudes of the moon, which I have compared with the tables, ſhew them to be very near the truth, both in the motion of the nodes, and alſo in the quantity and variation of the inclination.

Of the Acceleration *of the* Moon, *by the ſame.* N° 492. p. 162. April, &c. 1749. *dated* Cambridge, Feb. 28, 1748-9. *Read* June 1, 1749.

VII. After I had compared a good number of modern obſervations made in different ſituations of the moon and of her orbit in reſpect of the ſun, with the *Newtonian* theory, I proceeded to examine the mean motion of the moon, of her apogee, and nodes, to ſee whether they were well repreſented by the tables for any conſiderable number of years, and whether I ſhould be able to make out that acceleration of the moon's motion which Dr *Halley* ſuſpected.

To this end I compared ſeveral eclipſes of the moon obſerved by *Tycho Brahe*, as they are ſet down in his *Progymnaſmata*, *p.* 114, with the tables *, and found them agree full as well as could be expected; conſidering the imperfection of his clocks, and the difficulty there muſt commonly have been in determining the middle of the eclipſe from the facts obſerved, as publiſhed in his *Hiſt. Cæleſt.* Indeed the ſmall diſtance of time between *Tycho Brahe* and *Flamſtead*, rendered *Tycho*'s obſervations but of little uſe in this enquiry.

The next obſervations that occurred to me were thoſe of *Bernard Walther* and *Regiomontanus*, which being at double the diſtance of time from *Flamſtead* that *Tycho*'s were, ſeemed to promiſe ſome aſſiſtance in this matter: upon comparing ſuch of their eclipſes of the moon whoſe circumſtances are beſt related with the tables, I found the computed places of the moon were moſtly 5′ too forward, and in ſome conſiderably more, which I could hardly perſwade myſelf to throw upon the errors of obſervation; but concluded, that the moon's mean motion ſince that time, muſt have been ſomething ſwifter than the tables repreſent it; though the diſagreement of the obſervations between themſelves is too great to infer any thing from them with certainty in ſo nice an affair.

Then I compared the four well-known eclipſes obſerved by *Albategnius* with the tables, and found the computed places of the moon in three of them conſiderably too forward: this, if I could have depended upon the longitude

* My tables corrected as in my former letter; which is always to be underſtood of the tables mentioned in this.

longitude of *Aracta*, would very much have confirmed me in the opinion, that the moon's mean motion muſt have been ſwifter in ſome of the laſt centuries than the tables make it; though the differences between theſe obſervations, and the tables, are not uniform enough to be taken for a certain proof thereof.

I could meet with no obſervations of eclipſes to be at all depended upon between thoſe of *Regiomontanus* and *Albategnius*, except two of the ſun and one of the moon made at *Cairo* in *Egypt*, related in the *Prolegomena* to *Tycho Brahe*'s *Hiſt. Cæleſt. p.* 34; nor any between thoſe of *Albategnius* and *Ptolemy*, beſides the eclipſe of the ſun obſerved by *Theon* at *Alexandria*; notwithſtanding I carefully ſearched all the remains of antiquity I could find with that view. Theſe eclipſes of the ſun are the more valuable, becauſe they were obſerved in places the longitudes and latitudes whereof are determined by Monſieur *Chazelles* of the *R. Acad. Sc.* who was ſent by the *French* King in the year 1693, with proper inſtruments for that purpoſe. *

The ſolar eclipſe obſerved by *Theon* was in the 1112th year of *Nabonaſſar* the 24th day of *Thoth*, according to the *Egyptians*, but the 22d day of *Pauni*, according to the *Alexandrians:* he carefully obſerved the beginning of 2 temporal hours and 50′ afternoon, and the end at $4\frac{1}{2}$ hours nearly afternoon at *Alexandria*. *Theonis Comment. in Ptol. mag. Conſtruct. p.* 322. This eclipſe was *June* 16, in the year of Chriſt 364: and the temporal hour at *Alexandria* being at that time to the equinoctial hour as 7 to 6, makes the beginning at 3 equinoctial hours and 18′ afternoon, and the end at 5 equinoctial hours 15′ nearly.

The eclipſes obſerved at *Grand Cairo* were as follow.

"Anno Hegiræ 367, die *Jovis*, qui erat 28, rabie poſterioris (is eſt "ordine menſis quartus, & incipit ille annus *Saracenicus* die 19 *Auguſti*, "anno *Chriſtiano* 977) obſervatum fuit *Cahiræ* in *Ægypti* metropoli ini- "tium eclipſis ſolaris, cum altitudo ſolis eſſet 15° 43′. quantitas obſcu- "rationis 8 digit. Ea finita ſol elevabatur $33\frac{1}{2}$ *gr. Ex Schickardo in* "*MS.*"——This eclipſe was *Decem.* 13, in the year of Chriſt 977, the beginning at 8^h 25′, and the end at 10^h 45′, apparent time in the morning.

"Anno eodem die Sabbathi, videlicit 29 menſis *Sywal* (numero decimi, "qui Paſchalis eſt eorum) eclipſis ſolis occupavit digitos $7\frac{1}{2}$. In prin- "cipio ſol altus fere 56°. In fine ſol occiduus elevabatur gradibus 26. "*Ex Schickardo in MS.*"——This eclipſe was *June* 8, in the year of Chriſt 978. The beginning at 2^h 31′, and the end at 4^h 50′ apparent time afternoon.

"Anno Hegiræ 368 (qui incœpit die 9 *Auguſti*, anno *Chriſtiano* 978) "die *Jovis*, 14 *Sywal*, luna fuit orta cum defectu, qui at $5\frac{1}{2}$ digitos ac- "crevit; cum extaret ſupra horizontem gradibus etiam 26 ſubaudio fi- "nem tunc accidiſſe). *Schickardus.*"——This eclipſe was *May* 14, in the

* *Du Hamel, Hiſt. Acad.* p. 309, 395.

year

year of Chriſt 979; but as the middle cannot be known from what was obſerved of it, I made no uſe thereof in this enquiry. The account concludes with the following paragraph:

" Hæ tres obſervationes habitæ ſunt ab *Ibn-Junis*, qui juſſu regis *Abu* " *Haly Almanzor*, ſapientis, *Ægypto* tunc imperantis, rebus vacabat cœ- " leſtibus. Hujus authoris tabulas habet *Jac. Golius* profeſſor *Lugdun.* " (qui mihi inde communicavit iſta) in quibus plures aliæ, ſui & ſuperioris " ævi obſervationes extant. Locus obſervationis propinquus urbi *Cahiro.* " *Schickardus.*"

That the before-mentioned ſolar eclipſes might be applied to the examination of the lunar motions, I contrived the following method; which I think renders eclipſes of the ſun as uſeful at leaſt as thoſe of the moon are in that buſineſs.

Fig. 8. Let ABC repreſent half the earth's enlightned diſk, AEC a portion of the ecliptick projected thereon, FGH the path of the moon's ſhadow over the diſk, EI the univerſal meridian, α the ſituation of the place at the beginning of the eclipſe, β it's ſituation at the end thereof, δ the centre of the ſhade at the beginning, and ε its centre at the end of the eclipſe. Draw EG, $\alpha\zeta$, and $\beta\eta$, perpendicular to the path of the ſhadow, $\beta\gamma$ parallel thereto; join $\alpha\delta$ and $\beta\varepsilon$, and through α draw $\theta\alpha\iota$ perpendicular to AC.

Then (computing the true places of the ſun and moon at the obſerved times of the beginning and end of the eclipſe) we ſhall have given $\delta\varepsilon$ the motion of the moon from the ſun in her orbit during the time of the eclipſe, and $\alpha\delta = \beta\varepsilon$ the ſemidiameter of the *penumbra*; which are to be reduced into ſuch parts as the ſemidiameter of the diſk contains 10000: The angles BEI and BEG, being found by methods commonly known, GEI their ſum or difference will be likewiſe given. Alſo $E\alpha$ and $E\beta$ will be ſines of the ſun's altitude at the beginning and end of the eclipſe reſpectively; $IE\alpha$ and $IE\beta$, are the angles at the ſun betweeen the vertex of the place and the pole of thoſe times; which being found, the angle $\alpha E\beta$, their difference will be known, from whence the line $\alpha\beta$ and the angle $E\alpha\beta$ may be computed.

The angle $GE\alpha$ is the ſum or difference of the known angles GEI and $IE\alpha$: In the figure before us, the complement of this to a ſemicircle is $E\alpha\gamma$; which being ſubtracted from $E\alpha\beta$ leaves the angle $\gamma\alpha\beta$, from whence and the line $\alpha\beta$, $\alpha\gamma$, and $\gamma\beta = \zeta\eta$ may be found.

Let $a = \delta\varepsilon - \zeta\eta$, $b = \alpha\delta = \beta\varepsilon$, $c = \alpha\gamma$, and $x = \beta\eta = \gamma\zeta$.

Then $\sqrt{bb - xx} = \eta\varepsilon$, and $\sqrt{bb - cc - 2cx - xx} = \delta\zeta$, by *Eucl.* 1. 47.

Conſequently $a - \sqrt{bb - xx} = \sqrt{bb - cc - 2cx = xx}$ which being reduced, gives us the quadratic equation $xx + cx = \frac{4a^2b^2 - a^4 - 2a^2c^2}{4aa + 4cc}$.

This equation ſolved, gives us the value of x, from which $\delta\zeta$ and $\eta\varepsilon$ will be likewiſe had. In the triangle $\alpha\zeta\theta$ we have $\alpha\zeta$ and the angle $\zeta\alpha\theta =$

GEB

GEB given, whence $\alpha\theta$ and $\zeta\theta$ may be found: conſequently $\delta\theta$ will be known; and from the obſerved time of the beginning of the eclipſe, and hourly motion of the moon from the ſun, the time when the centre of the ſhade is at θ will be had. Laſtly, in the triangle $E\iota\alpha$, we have given the ſide $E\alpha$, and the angle $E\alpha\iota = BE\alpha$ (the ſum or difference of the angles BEI and $IE\alpha$); therefore the ſides $E\iota$ and $\alpha\iota$ may be found. But $E\iota$ is the diſtance of the moon from the ſun in the ecliptic, and $\alpha\iota \mp \delta\alpha\theta$ the moon's latitude at the time when the centre of the ſhade is at θ; which may be compared with the computation from the tables for that time.

By this means I compared the aforeſaid ſolar eclipſes with the tables, and found the difference in longitude and latitude, as follows.

A.D.	Apparent time at Greenwich	Diſt. ☽ a ☉ from *E*	Lat. ☽ from ☿	☽ a ☉ by Tab.	Lat. ☽ by Tab.	Diff. from Obſer. in Long.	Diff. from Obſer. in Lat.	Diff. in Lat from Digits obſerved.
	h ′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″
364	June 16. 2 4 20	39 41 in conſeq.	34 47 N.	35 25	37 26 N.	−4 16	+2 49	
977	Dec. 12. 19 12 30	43 39 in antec.	30 23 N.	36 3	31 50 N.	+7 36	+1 27	— 2 36
978	June 8. 1 16 10	29 3 in conſeq.	8 24 S.	37 48	3 21 S.	+8 45	−5 3	+ 3 38

The agreement there is between the two laſt of theſe differences in longitude, ſhews that the tables repreſent the mean motion of the moon's apogee very well for above 700 years, the moon being very near her perigee at the time of one of thoſe eclipſes, and near her apogee at the time of the other.

By the ſame method I alſo compared the ſun's eclipſe, *July* 29, 1478. (which appears, from what is related of it, to have been carefully obſerved by *Bernard Walther* at *Nuremberg*), with the tables, and found the difference in long. to be + 10′ 29″, and in lat. + 9′ 12″. This wide difference in lat. from the tables, that agree ſo well with the former ancient obſervations, confirmed me in the opinion, that the *Nuremberg* obſervations are too inaccurate to determine any thing from them in this affair.

The eclipſes recorded by *Ptolemy* in his *Almageſt*, are moſt of them ſo looſely deſcribed, that, if they ſhew us the moon's mean motion has been accelerated in the long interval of time ſince they happened, they are wholly incapable of ſhewing us, how much that acceleration has been. There are indeed two or three of them attended with ſuch lucky circumſtances as not only plainly prove, that there has been ſuch an acceleration, but alſo help us to gueſs at its quantity. One of theſe is the eclipſe, ſaid by *Hipparchus* to have been obſerved at *Babylon*, in the 366th year of *Nabonaſſar*, the night between the 26th and 27th days of *Thoth*, when a ſmall part of the moon's diſk was eclipſed from the N. E. half an hour before the end of the night, and the moon ſet eclipſed. This was in the year before Chriſt 383, *Decemb.* 22. The middle of this eclipſe at *Babylon* (ſuppoſing with *Ptolemy* the meridian of that place to be 50′ in time E. of the meridian of *Alexandria*), by my tables was *Dec.* 22, $21^h\ 4'$ apparent time; the duration was $1^h\ 37'$, *Ptolemy* makes it $1^h\ 30'$ nearly; whence

whence the beginning ſhould have been about 8^h 15′ after midnight: according to *Ptolemy*, the night at *Babylon* was at that time 14^h 24′ long, and therefore ſun riſe at 7^h 12′ after midnight; and as the moon had then S. lat. and was not quite come to the ſun's oppoſition, her apparent ſetting muſt have been ſomething ſooner, *i. e.* more than an hour before the beginning of the eclipſe, according to the tables; whereas the moon was ſeen eclipſed ſome time before her ſetting; which, I think, demonſtrates, that the moon's place muſt have been forwarder, and conſequently her motion ſince that time leſs than the tables make it by about 40′ or 50′. But the computed place of the moon in each of the before-mentioned ſolar eclipſes obſerved at *Grand Cairo*, being about 8′ before her place, from obſervation ſhews us, that the mean motion of this luminary has been ſomething greater in the laſt 700 years than the tables ſuppoſe it, and therefore muſt have been accelerated.

This acceleration is further confirmed by the eclipſe, which *Hipparchus* ſays was obſerved at *Alexandria*, in the 54th year of the ſecond *Calippic* period, the 16th day of *Meſſori*, when (he ſays) the moon began to be eclipſed half an hour before her riſing, and was wholly clear again in the middle of the third hour of the night. This was in the year before Chriſt 201. *Sep.* 22. The middle of this eclipſe at *Alexandria* by the tables was *Sept.* 22. 7^h 44′ apparent time; and the duration 3^h 4′, which makes the beginning at 6^h 12′ apparent time, that is, about 10′ after the riſing of the moon at *Alexandria*, or 40′ later than the beginning from obſervation. This difference in time makes a difference of near 20′ in the moon's place.

The moſt antient eclipſe of which we have any account remaining, namely that related by *Ptolemy*, to have been obſerved at *Babylon* the firſt year of *Mardokempad*, in the night between the 29th and 30th days of *Thoth*, in which the moon began to be eclipſed when one hour after her riſing was fully paſt; if, by reaſon of the lat. of the expreſſion, it be not a direct proof of the acceleration, it may nevertheleſs help to limit it's quantity. This eclipſe was in the year before Chriſt 721. *March* 19. The middle whereof at *Babylon*, by the tables, was *March* 19. 10^h 26′ apparent time; and the beginning at 8^h 32′ the apparent riſing of the moon at that place was about 5^h 46′ afternoon; ſo that the obſerved beginning of the eclipſe was at leaſt 6^h 46′ afternoon, *i. e.* not above $1\frac{3}{4}^h$ before the beginning, by the tables: wherefore the moon's true place could precede her place by computation but little more than 50′ at that time.

If we take this acceleration to be uniform, as the obſervations whereupon it is grounded are not ſufficient to prove the contrary, the aggregate of it will be as the ſquare of the time: and if we ſuppoſe it to be 10″ in 100 years, and that the tables truly repreſent the moon's place about *A. D.* 700. it will beſt agree with the before-mentioned obſerations;

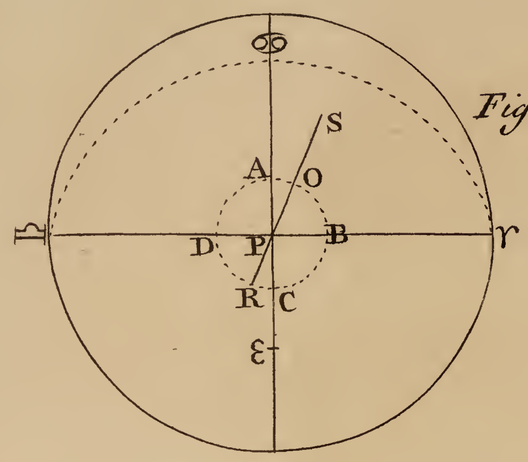
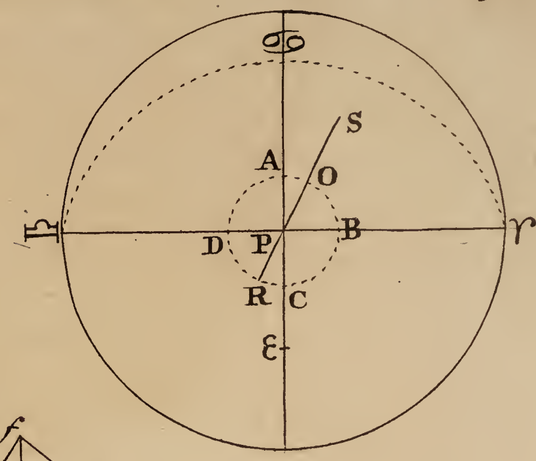


Fig. 1.



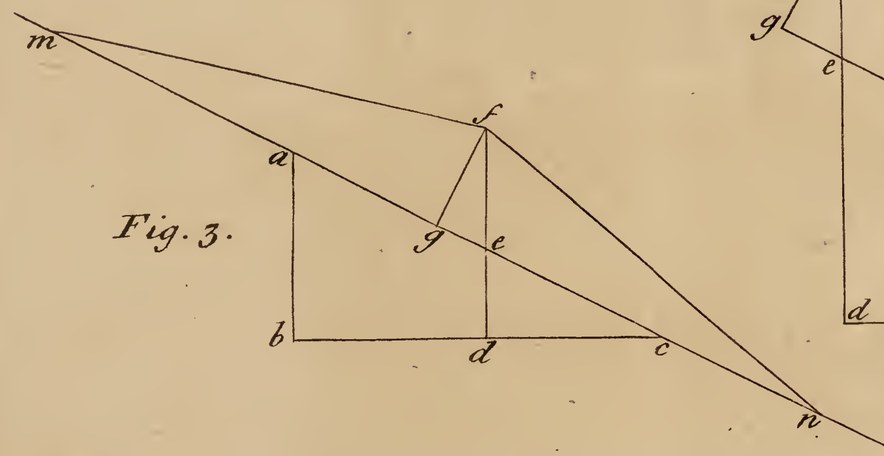


Fig. 3.

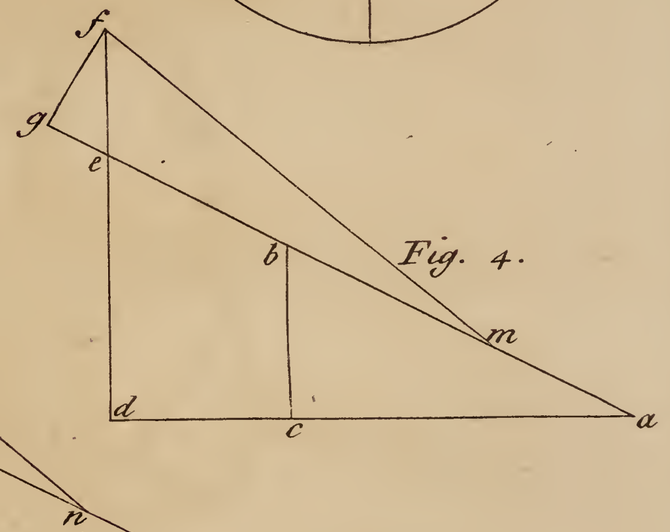


Fig. 4.

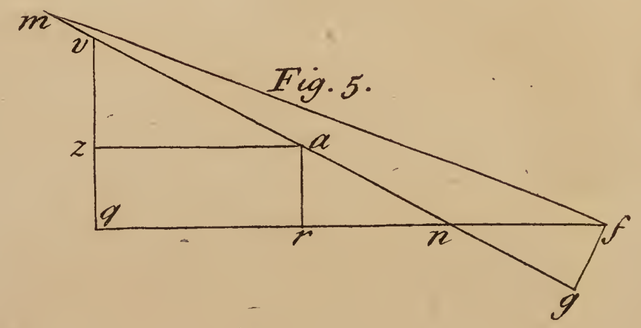


Fig. 5.

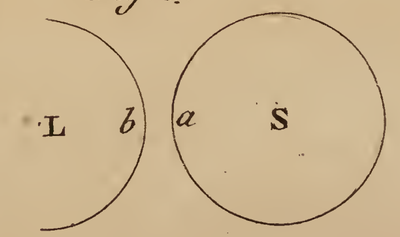


Fig. 6.

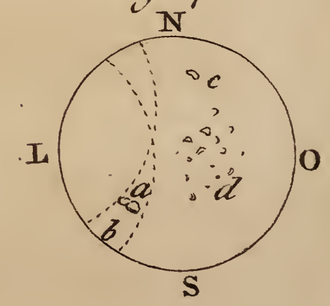


Fig. 7.

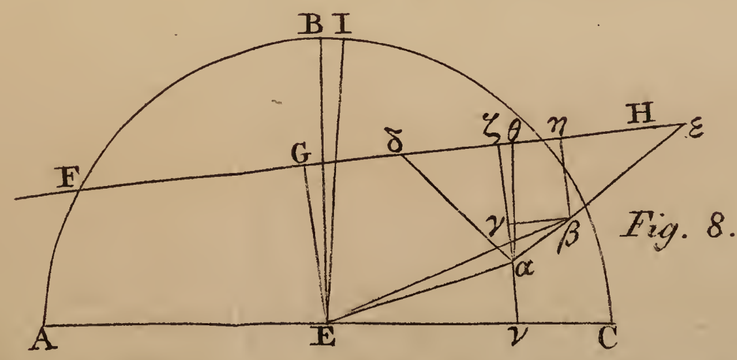


Fig. 8.

vations; and the difference between the moon's place by the tables and her place in the heavens, will be as follows.

Years before Christ	Error of Tab.	Years before Christ.	Error of Tab.	Years of Christ.	Error of Tab.	Years of Christ.	Error of Tab.	Years of Christ.	Error of Tab.
	/ //		/ //		/ //		/ //		/ //
700	— 56 0	200	— 28 30	300	— 9 20	800	+ 1 30	1300	+ 4 0
600	— 49 50	100	— 24 0	400	— 6 30	900	+ 2 40	1400	+ 3 30
500	— 44 0	A.D.0	— 19 50	500	— 4 0	1000	+ 3 30	1500	+ 2 40
400	— 38 30	100	— 16 0	600	— 1 50	1100	+ 4 0	1600	+ 1 30
300	— 33 20	200	— 12 30	700	0 0	1200	+ 4 10	1700	0 0

	d	h	′	″	
VIII. 1. *July*	28.	10.	13.	28.	The *penumbra* discernible.
			06.	30.	The beginning, as most of the company judged.
			18.	38.	*Mare humorum* just touch'd.
			26.	24.	Began to touch *Tycho*.
			27.	51.	*Tycho* bisected.
			24.	09.	*Tycho* cover'd.
			29.	53.	Touch'd *Grimaldi*.
			30.	25.	*Mare humorum* cover'd.
			34.	14.	*Grimaldi* cover'd.
	12.	24.	30.		The End.
			27.	40.	The *penumbra* quite gone.

The Moon's Eclipse *of* July 28, 1748. *observ'd at* Marlborough house, *by* J. Bevis, M. D. No. 489. p. 522. Oct. *&c.* 1748. *Read* Nov. 10, 1748.

About the middle of the eclipse, the moon's diameter, perpendicular to the equator, measur'd in a 5 foot telescope was 33′ 50″; perhaps 15″ or 20″ greater than it would have been found to be with a 12 foot tube.

2. I made use of the same telescope, with which I observed the eclipse of the sun mentioned above. The *phases* in this eclipse being reduced to true time are as follows.

—— *at* Madrid, *by* D. Ant. de Ulloa. No. 491. p. 12. Jan. *&c.* 1749. *Presented* Jan. 26, 1748-9.

	h	′	″
The *penumbra* began to be perceived at	9	45	42
The beginning of the eclipse, not without some doubt		50	0
Immersion of *Capuanus*	10	0	13
Beginning of the immersion of *Mare humorum*		4	10
Tycho begins to enter the shadow		11	54
Total immersion of *Tycho*		14	14
Beginning of the immersion of *Grimoaldus*		15	15
Total immersion of *Mare humorum*		15	18
Total immersion of *Grimoaldus*		20	51
Reinoldus enters the shadow		28	40
Snellius and *Furnerius* touch it		44	40

	h	′	″
Snellius and *Furnerius* under the ſhadow		47	40
Fracaſtorius begins to be immerged		49	0
Grimoaldus begins to emerge		51	16
Mare nectaris begins to be immerged		52	20
Grimoaldus totally emerged		56	32
Beginning of the immerſion of *Mare fœcunditatis*	11	13	58
Mare humorum begins to emerge		19	11
totally emerged		30	18
Total emerſion of *Mare nubium*		40	24
Total emerſion of *Mare nectaris*		45	16
Tycho begins to emerge		47	35
Total emerſion of *Tycho*		49	54
End of the ſhadow on the lunar diſk	12	10	22
End of the ſtronger *penumbra*		17	25
End of any *penumbra* whatſoever		22	12

The beginning of the eclipſe was doubtful, becauſe the ſhadow and the *penumbra* were not well diſcerned; and therefore they could not well have been determined, tho' the atmoſphere had remained clear, and free from all impediments.

An Eclipſe *of the* Moon, Dec. 12. 1749. *obſerved at Mr* Graham's *in* Fleet-ſtreet, *by* John Bevis, *M. D. and Mr* James Short, *F. R. S.* N° 493. p. 247. Oct. &c. 1749. *Read* Dec. 14. 1749.

	1749.	By the Clock.		App. Time.			
IX. 1.	*Dec.* 11.	23	56 15½	h	′	″	The ſun paſſed the meridian.
	12.	6	43 0	6	46	36	A ſenſible *penumbra*.
		6	47 20	6	50	56	Eclipſe begins.
		7	1 26	7	5	1	Shadow touches *Tycho*.
		7	3 12	7	6	47	*Tycho* half covered.
		7	4 38	7	8	13	*Tycho* covered.
		8	33 37	8	37	11	*Tycho* begins to be uncovered.
		8	34 50	8	38	24	*Tycho* half uncovered.
		8	36 9	8	39	43	*Tycho* quite uncovered.
		9	9 5	9	12	38	Eclipſe ends.
		9	13 30	9	17	3	*Penumbra* gone.
		12	5 53½				Moon's centre paſſed the meridian.
		12	20 2				*Sirius* paſſed, his mean right aſcenſion being 98° 31′ 38″.
	13.	23	56 46				The ſun paſſed the meridian.

The appulſes of the ſhadow to the ſpot *Tycho* were obſerved with a reflecting teleſcope, which magnified about 40 times, and may be ſerviceable for geographical purpoſes. The beginning and end of the eclipſe were eſtimated by the bare eye, and a refracting teleſcope of a ſmall magnifying power; larger powers being apt to dilate the ſhadow too much, and thereby render theſe phaſes more uncertain.

A com-

			Apparent Time. h	′	″
A computation by Dr *Halley*'s tables gave the beginning		-	6	52	0
	end	- - -	9	14	58

2.

h	′	
7	0	The umbra came on the lower limb of the moon, almoſt directly under the ſpot called *Tycho*, in *Keil*'s map of the moon.
	2¾	The penumbra overſpread *Tycho*.
	6	The umbra approached the lower part of *Mare humorum*, and *Tycho* immerged into the umbra.
	21	*Mare humorum* totally immerged into the umbra.
	41	The lower part of *Mare nectaris* immerged into the umbra.
	57	The N. E. limb began to evolve itſelf; and that part of the limb below the ſpot called *Grimaldus*, began to appear brighter than when the penumbra covered it.
8	9	The upper part of *Mare humorum* emerged from the umbra.
	21	*Mare humorum* totally emerged.
	45½	*Tycho* emerged from the umbra.
	51¼	The penumbra left *Tycho*.
	54½	*Mare nectaris* emerged from the umbra.
9	0	The penumbra left *Mare nectaris*.
	4¾	*Mare fœcunditatis* emerged from the umbra.
	16	The umbra left the moon a little below *Mare fœcunditatis*.
	18	The penumbra went off, and the eclipſe ended.

At the time of the greateſt obſcuration, the edge of the umbra paſſed below *Grimaldus*; approached the lower part of *Peninſula fulgurum*; paſſed over the upper part of *Mare nectaris*, and croſſed about the middle of *Mare fœcunditatis*. The edge of the umbra did not ſeem to make one regular curve, but looked like two curves, meeting in a very obtuſe angle near *Peninſula fulgurum*. And that part of the moon, immerſed in the umbra, was not viſible.

——*At* Earith, *near* St Ives, *in* Huntingdonſhire, *by Mr* Wm. Elſtobb, *jun. in a letter to* Martin Folkes, *Eſq*; *Pr. R. S.* ibid. p. 280. *Read* Dec. 21. 1749.

3. It was ſo boiſterous a day, that I deſpair'd of being able to ſee this eclipſe, and for that reaſon neglected to put my micrometer in order. My clock had likewiſe been altered without my knowledge, on which account I betook myſelf too late to the obſervation, as will appear by the following detail. The place of obſervation is in lat. 41° 54′ 0″. and 4″ of time eaſtward of *St Peter*'s. For I take it for granted that the *Thermæ Dioclefianæ* are, according to *Bianchini*'s determination, in lat. of 41° 54′ 27″.

——*At* Rome, *by Mr* Chriſtopher Maire. N° 494. p. 321. *Read* Feb. 1. 1748 9.

	h	′	″
Chord of the part eclipſed 13′ as was deduc'd from the map of the moon - - - - - - - - - - - -	7	47	18
Hence beginning of the eclipſe - - - - - - - - -	7	40	53

	h	′	″
The ſhade to *Tycho* and *Capuanus*	7	54	3
Tycho entirely covered	7	55	56
Shade to *Fracaſtorius*	8	28	43
Fracaſtorius quite hid	8	30	24
Tycho entirely diſengaged	9	30	24
End of the eclipſe, as far as could be perceiv'd thro' a thin cloud	10	0	16

I judg'd the eclipſe to be ſomewhat leſs than 5 digits.

Eclipſe *of the* Moon, June 8, 1750. *obſerved in* Sury-ſtreet *in the* Strand; *by Mr* John Catlin *and Mr* James Short, *F. R. S.* No. 496. p. 523. Nov. &c. 1750. *Read* Nov. 1. 1750.

X. 1. We expected to have ſeen the moon riſe eclipſed before the ſetting of the ſun; but were prevented by clouds. About half an hour after 9, we ſaw the moon then totally eclipſed; tho' conſiderably brighter on the E. than on the W. ſide; by which we found that ſhe was then paſt the middle of the eclipſe.

	h	′	″
Emerſion, or end of total darkneſs, at	9	45	0
End of the eclipſe at	10	51	30

Here follows a computation of the ſame eclipſe by Mr *John Catlin* from Dr *Halley*'s tables, which he ſays was done in a hurry; however he knows of no error in the calculation.

	h	′	″
Beginning at	7	14	25
Immerſion at	8	21	20
True oppoſition at	9	0	24
Emerſion at	9	45	52
End at	10	52	53

—— *at* Wittemberg, *by* G. M. Boſe, *Prof. of* Phyſicks. No. 496. p. 570. Nov. &c. 1750. *Read* Nov. 22, 1750.

2.

h	′	″	alt. ☉ °	′	h	′	″	h	′	″	h	′	″		h	′	″
8	11	4	38	34	3	47	40	23	58	44	11	59	22	hence noon	11	59	26
	12	30	38	44		46	9		58	39		59	19½	corr. for dec. ☉		—	3
	18	17	39	40		40	31		58	48		59	24	noon correct.	11	59	23
	20	23	40	0		38	39		59	2		59	31	and correction			
	23	30	40	25		35	17		58	47		59	23½	of the clock.		+	37
	27	36	41	0		31	23		58	59		59	29½				
	29	4	41	15		30	2		59	6		59	33				

The eaſtern part of the ſky continued covered with clouds, at

length

Length time by the clock h ′ ″	true time h ′ ″	Moon viſible. Emerſion a good while paſt. Ariſtarchus already diſcovered. lucid parts	rev. ″	that is ′ ″
10 50 0	10 50 37		3 220	9 49
54 0	54 37		4 75	11 26
58 13	58 50		4 192	12 19
11 0 22	11 0 59		5 37	13 52
3 36	4 13		5 245	15 26
6 3	6 40		5 349	16 13
9 53	10 30		6 152	17 27
12 1	12 38		6 321	18 43
14 8	14 45		7 122	19 56
16 9	16 46		7 265	21 1
18 32	19 9		7 319	21 25
20 40	21 17		8 55	22 9
23 7	23 44			
25 16	25 53			

I can hardly believe that the difference of meridians can be ſafely determined by lunar eclipſes.

Time by the clock h ′ ″	h ′ ″	
11 40 0	11 40 37	end of the ſhadow according to me.
40 40	45 17	end according to a friend.
	40 30	end by the projection of a friend.
	39 38	end by the corrected Kalendar of Leipſick.
	39 11	end by the *connoiſſance des temps*.
	39 46	end by the *ephemerides* of *Manfredi*.

Making uſe of the difference of meridians which I determined in 1743, by a tranſit of ☿ over ☉, which is alſo uſed by the Acad. in their *Connoiſſance*.

	rev.	° ′ ″	
Diam. of ☽ by a microm. =	11 142	that is 30 57	
Hence ſemidiam. of ☽ =	- - - -	15 28½	
The ſame according to Nicaſ. Grammat. =		15 25	See Roſt's Aſtron. Tab. 12.
J. Gauppius		15 2½	See Roſt's Aſtron. Tab. 13.
Kalend. Leipſ.		15 32	

Total Eclipse *of the* Moon, *observed* Dec. 2, 1750. *in the morn. in the* Strand, London, *about* 5^{II} *of time W. of* St Paul's, *and* 27^{II} *W. of the* R. Obſerv. *at* Greenwich; *by Dr* Bevis *and Mr* James Short, *F. R. S.* ibid. p. 575. *Read* Dec. 13, 1750.

	h	′	″
XI. A ſenſible *penumbra* (*Dec.* 1,) at	16	32	0
The eclipſe judged to begin at		36	50
Grimaldi covered		40	20
Shadow touches *Mare humorum*		45	26
at the middle of *Kepler*		48	40
at the middle of *Ariſtarchus*		50	7
touches *Copernicus*		55	24
Copernicus half-cover'd		56	56
——— quite covered		58	5
Timocharis half-covered		59	0
Shadow touches *Tycho*		59	20
at the middle of *Tycho*	17	0	0
covers *Tycho*		1	3
at the middle of *Menelaus*		14	42
touches *Goclenius*		24	29
covers *Goclenius*		25	17
at the middle of *Proclus*		27	20
touches *Mare Criſium*		28	44
at the middle of *Mare Criſium*		31	15
covers *Mare Criſium*		33	30
Total immerſion at		36	5
The moon begins to emerge	19	14	33
Grimaldi begins to emerge		16	4
quite uncovered		18	10

The moon was now got ſo low, and day-light ſo far advanced, that no more phaſes could be obſerved with any degree of certainty.

Theſe obſervations were made with a reflecting teleſcope, that magnified 40 times, and a refracting teleſcope, which magnified 12 times; and the times were the ſame through theſe two teleſcopes; for the air was exceeding clear, and the ſhadow well defined, the *penumbra* being ſcarce ſenſible.

Here follows a computation, made from Dr *Halley*'s tables, by Mr *John Catlin*, of *Guy*'s hoſpital; and ſent to Mr *Short* the day before the eclipſe.

Dec. 1. in the morning 1750.

	h	′	″
Beginning of the moon's eclipſe	16	44	31
Immerſion at	17	42	45
Emerſion at	19	20	37
End at	20	18	51

From hence it appears, that the eclipſe began about 8′ ſooner than the computation from Dr *Halley*'s tables gave it; but the computation which Mr *Brent* made and publiſhed ſome time before the eclipſe happened,

ed, was within 1′ of the time obſerved; and this exactneſs he imputes to his leaving out 3 of the 7 equations of the moon, publiſhed by Sir *I. Newton* in his theory of the moon.

XII. *Occultation of* Jupiter *by the* Moon, *obſerved at* London, *by* J. Bevis, *M. D.* No. 473, p. 65. May *&c.* 1744. *Read* June 7, 1744.

d	h	′	″	
1744 *June* 6	11	13	58	By the clock Immerſion of the centre of Jupiter certainly enough.
		35	14	*α Serpentaria* culminates.
		43	15	Emerſion of the centre. With a tube of 12 feet.

N. B. The clock was too ſlow 1′ 25″

XIII. In all the Orreries that I have ſeen, *Venus* is repreſented as having her *axis* perpendicular to the plane of the ecliptic, and her diurnal motion thereon equal to 23 hours of our terreſtrial time. Hence, as her annual motion is performed in about 225 of our days, it will contain 234 of her's; conſequently, to an eye placed in *Venus*, the ſun will always appear to go through a ſign of the zodiac in 19½ of her days; and as her *axis* has no inclination, ſhe muſt have a continual equality of her days and nights, without any variation of ſeaſons, and ſo her annual motion can be of no other uſe than to keep her from falling down to the ſun.

The Phænomena of Venus, *repreſented in an Orrery made by Mr* James Ferguſon, *agreeable to the obſervations of S.* Bianchini. No. 479, p. 127. March, *&c.* 1746. *Read* March 20, 1745-6. *here printed with alterations.*

But *Bianchini* gives a very different account of her; which is, that her *axis* inclines 75° from a line ſuppoſed to be drawn perpendicular to the plane of the ecliptic (by which I ſuppoſe he means her own ecliptic, and not the earth's); and that her diurnal motion is performed in 24 days and 8 hours of our time; and this will cauſe her year, which is almoſt equal to 225 of our days, to contain only 9¼ of her days; and this odd quarter of a day in *Venus* will make every fourth year a leap year to her, as happens to us on earth, by the 6 hours that our year contains above 365 days: and to her the ſun will appear always to go thro' a ſign of the zodiac in little more than ¾ of her day, which is equal to 18 ¼ of our days; and in going round the ſun, her N. pole conſtantly leans towards the 20th degree of *aquarius*.

Thus, with regard to the abſolute length of *Venus*'s year, *Bianchini* agrees with *Caſſini* and other Aſtronomers: but differs widely in other very remarkable particulars, from which ariſe ſo many advantages, as to make that planet incomparably more fit for it's inhabitants, than we could poſſibly conceive it to be by a quick rotation on an *axis* perpendicular to it's annual path. For *Venus* is ſo much nearer the ſun than our earth is, that it is well known ſhe muſt have twice as much light and heat as our earth has; and then, was the ſun always perpendicularly above her equator, we cannot imagine but that her equatorial parts muſt be burnt up with heat, and her polar parts uninhabitable, by reaſon of the greatneſs of cold, occaſioned by the ſun-beams being parallel to, or making ſo very acute angles with, the horizon.

But,

But, by ſuch a motion as *Bianchini* deſcribes, and which I have exactly repreſented in my Orrery, theſe inconveniences are avoided; for there is no place in *Venus* but what will have the four ſeaſons every year, and the heated places will have time to cool; becauſe, to any place over which the ſun paſſes vertically on any given day, he will, on the next day, be 26° from the *vertex* thereof, even tho' the place be on the tropic; and if it be on the equator, one day's declination will remove him $37\frac{1}{3}$° from it.

I having conſidered in general what the effects of the ſun's quick and great declination would be in *Venus*, as occaſioned by the great inclination of her *axis*, with her ſlow diurnal and quick annual motion; and finding that her globe in the Orrery, by being not quite an inch in diameter, was inſufficient for ſolving her *phænomena* to any degree of exactneſs; I took the following method, by which I could do it mechanically, to ſerve my purpoſe.

Along the middle of a ſtrait narrow ſlip of parchment I drew a black line, and then meaſuring my parchment round a common globe of 9 inches diameter, cutting it ſo as when the ends were a little overlapp'd, it would become a girdle, and ſtick faſt on any great circle of the globe. Having thus fitted it, I took it off; and laying it flat on a table, I divided one ſide of the black line into $9\frac{1}{4}$ equal parts for the $9\frac{1}{4}$ days in *Venus*'s year, and then I ſubdivided each day into 24^h or equal parts, of which the odd quarter contained 6, and ſet the proper figures to them. The other ſide of the line I divided into 12 equal parts or ſigns, and each ſign into 30°: by this means I could eaſily ſee, at every day and hour in *Venus*, in what place of the ecliptic the ſun was: and putting this girdle round the globe, at an angle of 75° to the equator, croſſing it in two oppoſite points, it would, by repreſenting *Venus*'s ecliptic drawn on her globe, ſerve for the ſolution of problems concerning her, as the ecliptic on our terreſtrial globe does for thoſe relating to our earth: for, by bringing the ſun's place, at any day or hour, to the braſen meridian, I had thereby his declination for that time; which gave me both an eaſy and ſure way for drawing the ſpiral of the ſun's motion over the body of *Venus* on this globe; and then, by elevating it to different latitudes, I could immediately ſee where the ſpirals cut the horizon in any lat. and at what height or declination they croſs'd the meridian; as by the hour-circle I could eaſily perceive the times of the ſun's riſing and ſetting, and his amplitudes on the horizon; and I called that the firſt meridian, which paſſed thro' the northern tropic, in the place where the ſun touch'd it at his greateſt N. declination; reckoning the E. or W. longitudes on the equator from that meridian. But this meridian will only ſerve for one year; becauſe, as the odd quarter of a day in *Venus*, cauſes the ſun to croſs her equator 90° weſtward of the former place every year, the place of the ſun's greateſt declination at the N. tropic will be in a meridian 90° weſtward of the former alſo. Things being thus premiſed in general: I now proceed to

give

give as good a deſcription as I can of the particular *phænomena* in *Venus*, confining myſelf chiefly to what happens in her northern hemiſphere; knowing that the ſame muſt happen, *mutatis mutandis*, in the ſouthern.

1. Her *axis* is inclined $51\frac{1}{2}^{\circ}$ more than the *axis* of our earth, and therefore the variation of her ſeaſons will be much greater than of ours.

2. Becauſe her N. pole inclines toward *aquarius*, and ours to *cancer*; her northern parts will have ſummer in the ſigns where thoſe of our earth have winter; and *vice verſa*.

3. The artificial day at each of her poles (containing $4\frac{5}{8}$ apparent diurnal revolutions of the ſun) will be equal to $112\frac{1}{2}$ natural days on our earth.

4. The ſun's greateſt declination, on each ſide of her equator, amounts to 75°: therefore her tropics are only 15° from her poles, and her polar circles at the ſame diſtance from her equator. Conſequently, her tropics are between her polar circles and poles, contrary to what thoſe on our earth are.

5. The ſun, in one apparent diurnal revolution from the equator, and any meridian where he croſſes it, to the ſame meridian again, changes his declination at leaſt 14° more on *Venus*, than on our earth from the equinox to the ſolſtice.

6. Let us now ſuppoſe an inhabitant ſtanding on her N. pole, where the ſun's declination is always the ſame with his altitude, and looking toward that point of the horizon where the firſt meridian (above mentioned) cuts it; and let him call that point the S. ſo ſhall he have a meridian fixt, which will determine the other cardinal points on the horizon; tho' ſtrictly ſpeaking, every point of the horizon to him is S.: yet, for once, let us ſuppoſe him to have an horizontal plane, fixed with it's S. point in this meridian, and thence divided and numbered like the horizon of a globe: put a moveable ruler with ſights to turn round the centre of this plane, for obſerving the ſun's amplitude at riſing and ſetting; and a graduated quadrant to be fixed in the N. and S. line, with a moveable index, for taking the ſun's altitude, in paſſing over the meridian. The ſame degree or part of a degree, that gives him the ſun's altitude, will alſo give him it's declination, and he will have the following *phænomena*.

The ſun will riſe $22\frac{1}{2}^{\circ}$ N. of the E. and going on $112\frac{1}{2}^{\circ}$, as meaſured on the horizontal plane, he will croſs the meridian at an altitude of $12\frac{1}{2}^{\circ}$; then, making an entire revolution without ſetting, he will croſs it again at an altitude of $48\frac{1}{2}^{\circ}$, at the next revolution he will croſs it as he culminates, at the height of 75°, being only 15° from the *zenith*; and thence he will deſcend in the like ſpiral manner, croſſing the meridian firſt at an altitude of $48\frac{1}{2}^{\circ}$; then, at an altitude of $12\frac{1}{2}^{\circ}$, and going on thence $112\frac{1}{2}^{\circ}$ he will ſet $22\frac{1}{2}^{\circ}$ N. of the W. having been 4 revolutions and $\frac{5}{8}$ parts of one above the horizon.

7. If the ſpectator turns his inſtrument $22\frac{1}{2}^{\circ}$ toward the E. and then ſuppoſes his quadrant in the plane a new meridian to him; the ſun will then riſe due E. and ſet in the N. W.; and his declination in the meridian will not be the ſame as before; for he will firſt croſs it at an altitude of 10°: next of 46; then, of $74\frac{6}{8}$; and, at $1\frac{1}{2}^{h}$ after, he will come to his greateſt declination; from which, in his deſcent, he will not croſs the meridian in the ſame degrees of altitude, as in aſcending he did.

8. Now, let the ſpectator turn his inſtrument 90° ſtill more toward the E. and the ſun will riſe due S.; and from thence making a complete revolution, he will croſs the meridian at an altitude of $37\frac{1}{2}^{\circ}$; making another revolution, he will croſs it at an altitude of $70\frac{3}{4}^{\circ}$; and, going on $7\frac{1}{2}^{h}$ (or 112°) he comes to his greateſt declination in the W. N. W.: thence deſcending, at the end of the third revolution he croſſes the meridian $58\frac{1}{3}^{\circ}$ high; at the end of the 4th he croſſes it in $23\frac{3}{4}^{\circ}$ of alt. and, going on thence 225°, or $\frac{5}{8}$ of a revolution, he ſets in the N. E.

9. If the ſpectator will now turn his inſtrument juſt half round, ſhifting his meridian 180°, the ſun will riſe in the N.; and, going on 180°, or $\frac{1}{2}$ a revolution, he will croſs the meridian at an alt. of 19°; then, making a complete revolution, he will croſs it at an alt. of 55°; and going on thence $292\frac{1}{2}^{\circ}$ he comes to his greateſt declination in the E. S. E. from which place he deſcends, croſſing the meridian in $73\frac{1}{2}^{\circ}$ of alt.; and, in the next rev. he croſſes the meridian at an alt. of $41\frac{1}{2}^{\circ}$: at the fourth rev. he croſſes it at an alt. of 5°; and going on thence 45°, or $\frac{1}{8}$ of a rev. he ſets in the S. W.

10. The ſun being thus for half a year together above each pole of *Venus* in it's turn, will cauſe the whole year at her poles, as well as at the poles of our earth, to contain only one day and one night: but there, the difference between the heat in ſummer and cold in winter (or of mid-day and mid-night) is greater than betwixt the ſame on any two places of our earth; becauſe, in *Venus*, the ſun is for half a year together above the horizon of one or other of the poles; and for at leaſt $\frac{2}{3}$ of a rev. (or about 16 of our days) within 20° of the *zenith*; and during the other half of the year, always below the horizon; and for a conſiderable part of that time, at leaſt 70° from it: whereas at the poles of our earth, tho' the ſun is for half a year together above the horizon, yet his alt. is never more than $23\frac{1}{2}^{\circ}$ above it in ſummer, nor his depreſſion greater than that quantity below it in winter. When the ſun is in the equator, he is ſeen in the horizon of both poles; $\frac{1}{2}$ of his diſc above, and the other below: and deſcending quite below the horizon of one pole, he aſcends in a viſible ſpiral above that of the other, until he comes within 16° of the *zenith*, where he keeps the ſame alt. nearly for ſome time; then deſcends in the like ſpiral manner, till he gets below the horizon, where he continues inviſible for the other half of the year. This will occaſion to each pole one ſpring, one harveſt, a ſummer as long

long as them both, and one winter, equal in length to the other 3 ſeaſons.

The ſun's great diſtance below the horizon of *Venus*'s poles, will make her winters much more uncomfortable than at the poles of our earth, where they have twilight more than half the winter-time; unleſs ſhe be ſurrounded with an atmoſphere capable of occaſioning a twilight, at leaſt as long in proportion to her winter, as our twilight is to ours. But this can hardly be ſuppoſed; becauſe always, when we ſee *Venus*, ſhe appears with the ſame conſtant ſerenity; and therefore I am apt to believe ſhe has a ſatellite, to ſupply, in ſome meaſure, the abſence of the ſun; as our moon does to our earth's poles, for one half of the winter conſtantly, without ſetting, from the firſt to the third quarter. 'Tis true, that we are inconveniently poſited, with regard to *Venus* for ſeeing her ſatellite (if ſhe has one); becauſe, when her moon or ſatellite has it's enlighten'd ſide toward us, it may be too far diſtant to be ſeen, becauſe *Venus* is then beyond the ſun, and, conſequently, furtheſt from us; and when ſhe is betwixt us and the ſun, or there abouts, her full moon would have it's dark ſide to us: and tho' *Venus* be then neareſt the earth, yet her ſatellite could no more be ſeen by us, than we can ſee our own moon at her conjunction. When *Venus* is at her greateſt elongation, we ſhould have only one half of the enlighten'd ſide of her full moon turn'd towards us; and even then, perhaps, on account of it's ſmallneſs, it may be too far diſtant to be ſeen by our teleſcopes. But of this only by-the-bye.

11. At the tropics, the ſun in ſummer will continue for about 15 of our weeks together above the horizon without ſetting, and as long below it in winter without riſing. While he is more than 15° from the equator, he neither ſets to the inhabitants of the neareſt tropic, nor ſets to thoſe of the other; whereas, at our terreſtrial tropics, he riſes and ſets every day in the year. But to let us know more particularly the *phænomena* of *Venus*'s tropics, we will ſuppoſe the inhabitant, who has ſeen the abovemetion'd appearances at the N. pole to have travell'd thence along the firſt meridian 15° to the northern tropic, carrying his engine or inſtrument along with him; and to have ſet it due N. and S. in the place where the ſaid meridian interſects the tropic; and as the meridian of every place is in a great circle paſſing thro' the *zenith* of the place and both poles, he can now be at no loſs how to ſettle his meridian, and obſerve as well the amplitude and azimuth, as the alt. of the ſun; who will riſe to him 10° N. of the E. with about 1° of N. declination: and going on 100° (to be meaſured on the horizontal plane) he will croſs the meridian with $12\frac{1}{2}$° of N. declination, and $27\frac{1}{2}$ of alt.; then, making an intire rev. without ſetting, he will croſs the meridian at $48\frac{1}{2}$° of decl. and $63\frac{1}{2}$ of alt.: at the end of the next rev. he will croſs the meridian in the *zenith* at the greateſt decl.; namely, 75°; and thence he deſcends in the like ſpiral, croſſing the meridian at the ſame alt. as above, till, in his fifth rev. he ſets 10° N. of the W.

12. Let our traveller now remove weſtward on the ſame tropic, to a meridian $97\frac{1}{2}^{\circ}$ diſtant from the firſt; and there he will have very great differences of the riſing, ſetting, and meridian alt. of the ſun; which will now riſe to him the firſt time, in the S. point of his horizon, at 12^{h}; at 1^{h} he will be about half a degree above the horizon, and will ſet at 2^{h}: ſo this ſhort artificial day in *Venus* (which is ſomewhat longer than two natural days on our earth) will have no forenoon at all. The ſun, after continuing almoſt 14 of *Venus*'s hours below the horizon, ſuppoſing each diurnal rotation to be divided into 24^{h}, will riſe a little before 4^{h} next morning, near the N. E.; and, going on 130°, he will then croſs the meridian with 22° of N. decl. and 37 of alt.: then going on without ſetting, he again croſſes the meridian at 57° of decl. and 72 of alt.; and advancing forward thence $17\frac{1}{2}^{h}$, or $262\frac{1}{2}^{\circ}$, he comes to his greateſt decl. $7\frac{1}{2}^{\circ}$ to the N. of the E.: from thence, completing his rev. to the meridian, he now croſſes it in $71\frac{1}{2}^{\circ}$ of decl. being only $3\frac{1}{2}^{\circ}$ from the *zenith*: at the next rev. he croſſes the meridian with $38\frac{1}{2}^{\circ}$ decl. and $53\frac{1}{2}$ of alt.: at the next, which is the fourth rev. he croſſes the meridian with $1\frac{1}{2}^{\circ}$ of decl. and $16\frac{1}{2}^{\circ}$ of alt.; and then goes on 65°, and ſets near the W. S. W.

13. Suppoſe now that our traveller removes ſtill further weſtward, on the ſame tropic, to a meridian 105° diſtant from this his ſecond ſtation; and then the ſun will firſt riſe to him in the S. E. about 9^{h}; and going on thence 45° he will croſs the meridian with 6° of S. decl. and 9 of alt. at 12^{h}: about 2^{h} he will be 1° higher; and, thence deſcending, he will ſet near the N. W. a little before 9^{h}: ſo the afternoon of this day is almoſt 6^{h} (about 6 natural days with us) longer than the forenoon; and it's night is but little more than 3^{h} long: for the ſun, after going a little below the horizon, riſes in the N. point thereof; and, making half a rev. he croſſes the meridian with 33° of decl. and 48 of alt. thence, making a whole rev. he croſſes the meridian at 66° of decl. and 81 of alt.: at the next rev. his decl. is 63° (having paſſed the greateſt 14^{h} before): at the next, it is 28° of decl.; and, going on thence about 146°, he ſets N. W. by N. about half an hour after 9; and continues inviſible till 3 quarters paſt 5 in the next morning, when he riſes about 4° N. of the E.; and, going thence forward 94°, he croſſes the meridian about 5° alt. and 10 of S. decl. having kept the ſame alt. very nearly for 3^{h}; then deſcending, he ſets in the S. S. W. about half an hour paſt 1; which makes the afternoon 5^{h} and about $12'$ ſhorter than the forenoon of the ſame day. The ſun now ſets for about 15 of our weeks to *Venus*'s northern tropic, and riſes to the ſouthern; in which the *phænomena* are the ſame: each tropic having the 4 ſeaſons once every year; the winters being longer than the ſummers, tho' not quite ſo long, in proportion, as at the poles.

14. Having ſaid ſo much concerning the N. pole and tropic, proceed we now to ſtation our inhabitant in a place of 45° of N. lat. where the firſt meridian cuts the parallel, and he will have the following *phænomena*.

The ſun will riſe 43° E. of the S. a little before 9^{h}; and, aſcending very quickly, he will, in little more than 3^{h}, croſs the meridian at an

alt.

alt. of 19°, with 26° of S. decl.; then going on 62°, he will ſet near the W. S. W. about 5^h in the afternoon; by which means it is almoſt 2^h longer than the forenoon; each hour in *Venus* being equal in length to 24^h 20′ of our terreſtrial time. The next day the ſun will riſe 3° N. of the E. about half an hour paſt 5 in the morning, and will croſs the meridian with $12\frac{1}{2}$° of N. decl. and $57\frac{1}{2}$ of alt.; and will ſet in the N. W. by W. about half an hour paſt 7: ſo that the afternoon will be 2^h longer than the forenoon. The next day the ſun riſes 53° N. of the E. about 3^h; and will croſs the meridian $3\frac{1}{2}$° N. of the *zenith*; or with $86\frac{1}{2}$° of N. alt. and $48\frac{1}{2}$ of decl.: then he goes round without ſetting; and croſſes the meridian 30° N. of the *zenith*, where he comes to his greateſt decl.; from which he returns in the like ſpiral toward the equator, and beyond it; but will not riſe and ſet at the ſame hours as before: for, having made a rev. without ſetting, in the next he ſets 53° N. of the W. about 9^h: next morning he riſes in the N. E. by E. about half an hour paſt 4; croſſes the meridian with $12\frac{1}{2}$° of decl. and ſets 3° N. of the W. about half an hour paſt 6; and now the forenoon is 2^h longer than the afternoon. The next day the ſun riſes about 7^h 62° E. of the S.; paſſes over the meridian at an alt. of 19°, with 26° of S. decl.; and ſets a little after 3; which makes the forenoon to be about 2^h at leaſt longer than the afternoon: and now the ſun will continue below the horizon at leaſt 12 of our weeks without riſing to this inhabitant of *Venus*.

15. In this place of *Venus* the hour and amplitude of the ſun's riſing, for one half of the year, are the ſame with thoſe of his ſetting in the other half; which will alſo happen in all places under the firſt meridian, where he riſes and ſets: but, if our Spectator pleaſes to remove along the parallel of 45° lat. eaſtward 142°, the *phænomena* of things will then be very different to him; for the ſun from once riſing in the N. E. by E. will paſs over the meridian with $3\frac{1}{2}$° of N. decl. and ſet due N.; which will make the afternoon ſomewhat above 4^h longer than the forenoon; and the next morning the ſun will riſe at 2^h $21\frac{1}{2}$° E. of the N. or about the N. N. E. As to what would happen on the other days concerning the ſun's riſing, and ſetting, I ſhall not take any further notice of it; but, if the inhabitant will travel eaſtward $37\frac{1}{2}$°, ſtill upon the ſame parallel of lat. he will ſee the ſun, at making his firſt appearance from the ſouthern tropic, riſe due S. at 12^h; and, getting about half a degree above the horizon, when he has gone forward about 9°, he will then deſcend, and ſet about a quarter after 1: ſo there is only 1^h and a quarter in the firſt day of the ſun's appearance; and the 2d day will be 11^h long; but the 3d day will be about 87^h long; for the ſun will make 3 rev. and ſomewhat more than an half without ſetting: the fourth day will be 11^h long; and the fifth will only contain 1^h and a quarter; for the ſun will riſe about 18° E. of the S. and ſet in the S. point of the horizon.

16. We

16. We will now ſuppoſe that the ſpectator has travelled from 45° of N. Lat. to the equator, and has a mind to take a tour round the ſame, becauſe the *phænomena* will be very different in different parts thereof; though the ſun will riſe and ſet to every part of it, in every apparent revolution; but we ſhall only conſider in general what happens at two places thereof: the firſt place ſhall be that, where the firſt meridian croſſes the equator; and the ſecond, a place 112½° W. of the firſt. To each of theſe places the ſun will always riſe at 6, and ſet at 6, though ſometimes his meridian alt. may be 11° more or leſs than his midnight depreſſion; and in other places the difference will amount to 15 or 16°; ſo that, if the diurnal and nocturnal ſpirals of the ſun's motion on the body of this planet were meaſured, the one would very much exceed the other. To the firſt of theſe two places the ſun will riſe 74° S. of the E. in coming from the ſouthern tropic, and ſet 61½ S. of the W. having been 22° high at mid-day, and will be 32½ depreſſed below the horizon at midnight. The next day he will riſe 44° S. of the E. and ſet 26° S. of the W. having been 55° high at noon, and will be 74½ depreſſed at midnight. The third day he will riſe 7½° S. of the E. and croſſing the equator at half an hour after 10, he will, in 7½ʰ after, ſet 12° N. of the W. and ſo proceed, changing his riſing and ſetting amplitude every day, in advancing toward the northern tropic, till he reaches it; and then his ſetting amplitude, in going from it, will be the ſame as his riſing amplitude in coming toward it. In the ſecond place, all I ſhall take notice of, is, that the ſun, in coming from the ſouthern to the northern tropic, will croſs the equator at 9 at night; and, in going from the northern to the ſouthern tropic, he will croſs the equator at mid-day.

17. At the equator the ſun's rays will be as oblique, when his declination is greateſt, as they are at *London*, when he touches the tropic of *Capricorn* in *December*; becauſe the tropics of *Venus* are as far from each ſide of her equator, as the tropic of *Capricorn* is from the parallel of *London* on our earth, therefore, at *Venus*'s equator, there will be two winters, two ſprings, two ſummers, and two autumns, every year: and becauſe the ſun ſtays for ſome time near the tropics, and paſſes ſo quickly over the equator, every winter there will be about twice as long as ſummer: but, becauſe of the quick return of ſummers, and the general heat on the body of *Venus*, the winters there will be very mild; and ſo will make the equator, and all places thereabouts, very temperate, and fit for habitation.

18. Thoſe parts of *Venus* which lie between the poles and tropics, and between the tropics and polar circles, and alſo between the polar circles and equator, will more or leſs participate of the *phænomena* of theſe circles, as they are more or leſs diſtant from them.

19. The places of the equinoxes and ſolſtices on the body of *Venus* go backward, or from E. toward the W. 90° every year. This is not occaſioned by any mutation of her *axis* from its parallelism; but by the ſun's being ¼ of a day later in croſſing the equator every year, than on the year before; and therefore he will croſs it in a place 90° weſtward

of

of the former every year: ſo that to any place where he croſſes the equator at noon, he will, on the return of that day at noon in the next year be almoſt 10° S. of the equator, and will croſs it at 6 in the evening; ſuppoſing the year to begin when the ſun is on the equator, in paſſing from the ſouthern tropic to the northern. Hence, though the ſpiral, in which the ſun's apparent motion is performed, be of the ſame ſort every year, yet it will not be the very ſame; becauſe the ſun will paſs vertically over all the ſame places but once in every 4 years: and, in the above deſcription, I have only ſhewn what will happen in general, for 1 year, having only drawn the ſpiral of the ſun's motion for that time: and if a ſpectator, on any parallel of latitude, ſhould want to ſee the ſame appearances of the ſun's riſing and ſetting every year, and, conſequently to have the particular days thereof to be ſtill of the ſame length with thoſe of the year, he muſt travel weſtward every year 90° on the ſame parallel.

20. The inhabitants of *Venus* will be very careful in adding a day to ſome particular part of every 4th year, to keep ſtill the ſame ſeaſons to the ſame times; becauſe, as the great annual change of the equinoxes and ſolſtices will ſhift the ſeaſons forward $\frac{1}{4}$ of a day every year, they would, in 36 years, ſhift the ſeaſons forward through all the days of the year: But, by this intercalary day, every 4th year will be a leap-year; which will bring her time to an even reckoning, and keep her Kalendar right.

21. The great change of the ſun's declination every day, which cauſes his altitude, at noon, or any other hour, and his amplitude at riſing and ſetting, to be ſo very different in places lying under the ſame parallels of lat. will be of one ſingular uſe in *Venus*, the like whereof we ſhall never enjoy on the earth; and that is no leſs than the giving a ſure and eaſy method of finding the longitude. For, ſuppoſe to one place, at noon, the ſun's declination is 30°, and to another place, it is only 20° 35′ at noon, in the ſame revolutional ſpiral, going from the equator toward the northern tropic; the difference of theſe two declinations is 9° 25′: in the ſame ſpiral from the equator, where any meridian croſſes it, to the ſame meridian again, the declination changes from 0 to 37° 21′; and the ſun has gone 38° 55′ in the ecliptic. Theſe things being known, the proportion will be thus; As 75°, the greateſt declination, is to to the ſun's motion in that time, which is 3 ſigns, equal to $2\frac{5}{16}$ revolutions round *Venus*; ſo is 9° 25′ (the difference of declination at two given places) to 9° 44′, which is $\frac{1}{4}$ part of a revolution; and therefore the one place is $\frac{1}{4}$ part of a circle, or 90° of long. diſtant from the other: and, as the decl. was advancing from the equator toward the northern tropic, the place, in whoſe meridian it was 20° 35′ is eaſtward from the place in whoſe meridian it was 30°, ſuppoſing them both to be in the northern hemiſphere.

I ſhould be very glad to ſee this deſcription examined into, and put in a better form, by ſome whoſe abilities are much greater than mine: And although it ſeems ſtrange, at the firſt view, that the great inclination of

Venus's

Venus's axis, with her ſlow diurnal, and quick annual motion, ſhould make ſuch mighty differences of her *phænomena* from the earth's; yet I verily believe, that, was the ſpiral of the ſun's motion for four years, which would contain 37 revolutions, nicely drawn on a large globe, and the times mentioned in which the ſun would riſe and ſet, with his different amplitudes, altitudes, and declinations, where the effects thereof would differ conſiderably in many particular parts of each ſpiral; and ſo occaſion remarkable differences of the lengths of day and night, in the ſame revolutions, to places under the ſame parallels of latitude; a whole volume might be written in the deſcription, if the author would deſcend to particulars.

A letter from John Bevis, *M. D. to* Martin Folkes, *Eſq; P. R. S. &c. containing ſome* Obſervations *concerning* Mercury. No. 473. p. 48. May *&c.* 1744. *Preſented* May 4. 1744.

XIV. The obſervations to which the incloſed computations are made by Mr *Morriſs* from ſomewhat more correct elements than thoſe in Dr *Halley*'s tables, were carefully taken by myſelf, with an excellent aſtronomical ſector of 5 feet *radius*. You will perceive how far I am limited, by my friend's requeſt; ſo muſt intreat you, if you think it worth while, to inform the *R. Soc.* that *Mercury*'s motion has not been at all diſturb'd [by the late *Comet*].

		°	′	″
1744, *May* 17. The apparent right aſcenſion of ε *Geminorum*, according to Dr *Bradley*'s obſervations		97	2	10
Declination		25	21	30

May.	Appt. time. h ′ ″		Long. ☿. ° ′ ″	Latitude. ° ′ ″		Comp. Rt. Aſc. ° ′ ″
15	8 31 15	♊	28 56 51	1 57 58	N.	88 50 7
17	8 26 6	♋	1 1 16	1 44 45		91 7 41
18	9 4 40		1 59 30	1 36 40		92 11 53
19	8 41 00		2 51 38	1 28 7		93 9 12

May.	Comp. Declin. ° ′ ″	Obſd. Rt. Aſc. ° ′ ″	Obſd. Declin. ° ′ ″	Error Comput. Rt. Aſc. ″	Declin. ″
15	25 26 12	88 49 20	25 26 20	+ 47	— 8
17	25 13 00	91 7 4	25 12 56	+ 37	+ 4
18	25 4 15	92 11 10	25 4 27	+ 43	— 12
19	24 54 43	93 8 20	24 54 56	+ 52	— 13

Tranſit of Mercury *over the* Sun, Nov. 5, 1743. *at the Obſervatory at* Gieſen; *by* Chriſtian Lewis Gerſten, Math. *Prof. and F. R. S.* No. 482. p. 376. Jan. *&c.* 1747. *Read* Jan. 22, 1746-7.

XV. In the obſervation of this tranſit, I chiefly made uſe of 3 inſtruments; the firſt of which was a good teleſcope of 10 feet, to which I fitted an excellent micrometer made at *London*. The ſecond was an oſcillatory aſtronomical clock, made by Mr *Ellicot*, which the experience of many years gave me reaſon to depend upon. The third was of my own invention, and ſerved to diſcover the true time of noon, inſtead of an aſtronomical quadrant, with which I was not provided.

A little

A little after 8^h in the morning, the clouds, which had totally obſcured the heavens, began to break unexpectedly; and in a ſhort ſpace of time the ſun began to ſhine clear through the opening. I applied my teleſcope immediately; but not being able to ſee any thing of *Mercury* or of any ſpot whatſoever, I endeavoured to take the horizontal diameter of the ſun by repeated obſervations, and though the rapidity of the motion made it not eaſy to do this, yet I thought I had pretty juſtly found the ſemidiameter to be $21\frac{22}{72}$ rev. of the micrometer. I afterwards found the vertical ſemidiameter at about $11^h\ 20'$ exactly $21\frac{18}{72}$ rev. How much theſe numbers make in the parts of a great circle will be ſhewn below.

When I had taken the horizontal ſemidiameter of the ſun, it was hid again by very thick clouds; but at $9^h\ 6'\ 25''$ on a ſudden I ſaw *Mercury* on it's diſk, being wholly entered, if I rightly remember, but yet adhering to the edge. But going to look at the clock, in the abſence of my aſſiſtant, on my return, I found the ſun covered with clouds; ſo that I dare not affirm what was the exact time of the contact.

The following obſervations were made in the intervals of the clouds. I was favoured by the calm ſtate of the air, and by the abſence of many ſpectators. The body of ☿ appeared round and black with a determinate edge, and without any ſigns of an atmoſphere, but ſo minute, as to appear to the naked eye not above twice as thick as the hair of a micrometer. About $1^h\ 10'$ *p. m.* till the egreſſion, the clouds were very diſtinct; but by that time the ſun cauſed ſuch an undulation of the limb as I could not by any means remove.

The 1ſt col. of the following table ſhews the time by the clock. The 2d, the true corrected time. The 3d, the ſpaces of time from the appulſe of the limbs of the ſun to the appulſe of ☿ to the horary thread, reduced into ſeconds of a great circle for the declination of the ſun $15^\circ\ 39'\ 18''$. The 4th, the obſervations. The 5th, the diſtances of ☿ from the lower limb of ☉ in parts of the micrometer. The 6th, the parts of the micrometer reduced to ſeconds of a great circle. The baſis of the reduction is; 23 entire rev. give $17'\ 33''\frac{1}{2}$ which I found to be ſuch by the tranſit of ☉ and ſome of the fixed ſtars.

Time by the clock before noon.			True time corrected.			Dift. ☿ in R.A.	Obſervations.	Rev. Micr.	Dift. ☿ in Decl.
h	′	″	h	′	″				
9	6	25	9	5	55	00	1. I ſaw ☿ wholly entered or certainly the greateſt part.		
							2.		″
9	44	7	9	43	37	694	☿ to the horary.		
0	0	55	0	44	25		Following limb of ☉ to the horary.	$5\frac{13}{72}$	237
							3.		
9	49	5	9	48	35	722	☿ to the horary.		
0	0	55	0	49	25		Following limb of ☉ to the horary.	$5\frac{35}{72}$	251
							4.		
9	56	30	9	56	0	766	☿ to the horary.		
0	57	23	0	0	53		Following limb of ☉ to the horary obſerved thro' a thin cloud.	$5\frac{66}{72}$	271
							5.		
10	2	43	10	2	13	794	☿ to the horary.		
0	3	38	0	3	8		Following limb of ☉ to the horary.	$6\frac{21}{72}$	288
							6.		
10	44	35	10	44	5	996	☿ to the horary.		
0	45	44	0	45	14		Following limb of ☉ to the horary.	$8\frac{42}{72}$	393
							7.		
10	48	14	10	47	44	1028	☿ to the horary.		
0	49	25	0	48	55		Following limb of ☉ to the horary.	$8\frac{60}{72}$	404
							8.		
10	53	26	10	52	55	1054	☿ to the horary.		
0	54	39	0	54	9		Following limb of ☉ to the horary.	$9\frac{9}{72}$	418
							9.		
11	33	52	11	33	22	1272	☿ to the horary.		
0	35	20	0	34	50		Following limb of ☉ to the horary.	$11\frac{30}{72}$	522
							10.		
11	40	54	11	40	24	1310	☿ to the horary.		
0	42	25	0	41	55		Following limb of ☉ to the horary.	$11\frac{54}{72}$	538
							11.		
11	43	52	11	43	22	621	Preceding limb of ☉ to the horary.		
0	44	35	0	44	5		☿ to the horary.	$12\frac{6}{72}$	553
0	46	7	0	45	37	1329	Following limb of ☉ to the horary.		
A. M.			P. M.						
11	57	42	11	57	12	549	12. Preceding limb of ☉ to the horary.		
0	58	20	0	57	50		☿ to the horary.	$12\frac{42}{72}$	580
0	59	57	0	59	27	1401	Following limb of ☉ to the horary.		
P. M.									
							13.		
12	3	27	12	2	57	520	Preceding limb of ☉ to the horary.		
0	4	3	0	3	33		☿ to the horary.	$13\frac{8}{72}$	600
0	5	42	0	5	12	1430	Following limb of ☉ to the horary.		
							14.		
12	46	56	12	46	26	289	Preceding limb of ☉ to the horary.		
0	47	16	0	46	46		☿ to the horary.	$15\frac{44}{72}$	715
0	49	11	0	48	41	1661	Following limb of ☉ to the horary.		

Time

Time by the clock after noon.	True time corrected.	Diſt. ☿ in R.A.	Obſervations.	Rev. Micr.	Diſt. ☿ in Decl.
h / //	h / //		15.		
1 16 56	1 16 28	144	Preceding limb of ⊙ to the horary.		//
0 17 8	0 0 38		☿ to the horary.	$17\frac{16}{72}$	788
			The limb of ⊙ began to tremble.		
			16.		
1 19 27	1 18 57		Preceding limb of ⊙ to the horary.		
0 0 35	0 19 5		☿ to the horary.	$17\frac{24}{72}$	793
0 21 41	0 21 11		Following limb of ⊙ to the horary.		
			17.		
1 36 15	1 35 45		It ſeemed to touch the inner edge,		
0 0 20	0 35 50		it certainly either touched it or went		
0 38 19	0 37 49		a little beyond it, the great undu-		
			lation and trembling of the limb		
			quite went off.		
			Obſ. 16.		
			On account of the trepidation of		
			the ſolar limb, I think it ſhould be		
			thus corrected according to the ana-		
			logy of the reſt.		
1 19 $26\frac{1}{2}$	1 18 $56\frac{1}{2}$	122	Preceding limb of ⊙ to the horary.		
0 0 35	0 19 5	1828	☿ to the horary.	$17\frac{24}{72}$	793
0 21 $41\frac{1}{2}$	0 21 $11\frac{1}{2}$		Following limb of ⊙ to the horary.		

I now proceed to the corollaries to be drawn from theſe obſervations. In the firſt place, the diameter of ⊙ is to be determined: in order to this, we muſt have it's decl. and alt. at the time when the vertical diameter was meaſured. The decl. of ⊙ is eaſily computed from it's longitude. I found it's long. by the *Ludovician* tables at $11^h\ 20'\ 39''$ true time (near the middle of it's tranſit) for merid. $25'\ 10''$ of time E. of *Paris* to be ♏ $12^\circ\ 37'\ 37''$. To this long. anſwers S. decl. $15^\circ\ 39'\ 18''$. The diſtance of time from the appulſe of the preceding limb of ⊙ to the appulſe of the following, by obſ. 11, 12, 13 and 14, is $2'\ 15''$, which time being converted into arcs of the equator, gives $33'\ 45''$. Therefore, if this arc for the decl. be reduced according to the rules of the ſpherical doctrine into parts of a great circle, the diameter of the ſun will be $32'\ 30''$.

By the aſtron. obſ. of *Philip* of *Butiſbach*, *Landgrave* of *Heſſe*, the latitude of the city of *Butiſbach*, which is not above 4 hours journey diſtant from *Gieſen*, is $50^\circ\ 28'$. Wherefore I take $50^\circ\ 30'$ for the latitude of *Gieſen*. Hence the alt. of ⊙ when it's vertical diameter was meaſured, is between the 23d and 24th degree more or leſs. The ſemidiameter of ⊙ in parts of the micrometer was 21 $\frac{18}{72}$ rev. which according to my ta-

ble, is = 16′ 13″ of a great circle. Hence, the vertical diameter at that time 32′ 26″. But becauſe of the refraction it ought to appear leſs than the truth, and by *de la Hire*, tab. v. that defect is 4″. This being added, we have again 32′ 30″. But if we make uſe of a newer table of refractions conſtructed from *Taylor*'s *Hypotheſes*, which *Halley* * publiſhed, and preferred before the reſt, the defect will be leſs only by ſome thirds.

I found the horizontal ſemidiameter, as I ſaid above, to be in parts of the microm. 21 $\frac{2}{7}\frac{2}{2}$ rev. the duple of this quantity gives according to my table, 32′ 31″ of a great circle. Therefore theſe 3 obſervations agree very well together, and make the diam. of ☉ 32′ 30″.

I proceed now to the angle ſeen of the apparent path of ☿ with the ecliptick. I followed the method of *Manfredi* in the tranſit of 1736 †, which alſo I have made uſe of. I drew a ſcale with great exactneſs, and found, that if the mean place is ſought between the places of obſ. 15 and 16 arithmetically corrected, and then through this, and alſo through that, which obſ. 5 determines, a right line be drawn; that it ſhews it's true apparent path in the diſk of ☉ as near as poſſible. This principle being laid down, I applied the numbers. The mean diſt. between obſ. 15 and 16 corr. from the following limb of ☉ is 1817″ of a great circle. The mean diſt of ☿ between the ſame obſervations from the inferior limb of ☉ 790 $\frac{1}{2}$″. The diſt. of the place of obſ. 5, from the following limb, is 794″. Diſt. from the inferior limb, 288″. Theſe differences therefore form a rectangular triangle, the firſt of which is the baſe, and the other the *cathetus*. The calculus being made, the angle at the baſe is 26° 9′, to which the angle of the path, with the circle parallel to the equator, is equal. After the ſame manner I ſought the angles of the ſeveral places; beginning with obſ. 7, with the place of obſ. 5, and they came out as follows.

				°	′
By obſ.	5 and	7	the angle is	26	33
	5 and	8	- - -	26	33
	5 and	9	- - -	26	5
	5 and	10	- - -	26	51
	5 and	11	- - -	26	21
	5 and	12	- - -	25	41
	5 and	13	- - -	26	7
	5 and	14	- - -	26	13
	5 and	15	- - -	26	17
			Mean	26	11

Wherefore, when in the former caſe, the angle at the baſe is 26° 9′, and in this, 26° 11′, I take the mean 26° 10′ for the angle ſeen of the

* See Vol. VI. p. 167. where Mr *Eames* has by miſtake omitted in the margin the name of Dr *Halley*, who was author of the paper.

The table here mentioned was not Dr *Brook Taylor*'s; but Sir *I. Newton*'s.

† See Vol. VI. p. 195.

path

path with the parallel circle. Hence, the angle of the apparent path with the horary, 116° 10′. But to the place of ☉, 12° 37′. ♏ anſwers by *de la Hire*'s tab. to the angle of the ecliptick with the merid. 107° 43′. Therefore the angle of the apparent path of ☿ with the ecliptick, is 8° 26′.

For the leſt diſtance of the centres, I choſe two obſervations, the middle way between which was ſhewn by the type to be taken, nor were they much diſtant from the very path, namely the 7th and 10th. From the diſtances of ☿ from the inferior limb, I ſubtracted 8″ allowing $5\frac{1}{2}$″ for the ſemidiameter of ☿ *, and the reſt for $\frac{1}{2}$ the thickneſs of the parallel thread: for the diſtances are to be taken from the centre of the tube, not from the edge of the thread. And then from the diſtance of ☿ from the following limb in obſ. 7. Semid. ☉, the angle of the path with the parallel circle being found, I diſcovered, by the analyſis of the triangles, the laſt diſtance of the path, or of the centres of ☉ and ☿ to be 9′ 2″. By obſ. 10 the diſtance is 9′ 7″; therefore I take the mean 9′ $4\frac{1}{2}$″ for the true diſt. of the path from the centre of ☉. From theſe premiſes, I drew the following concluſions by a trigonometrical calculation.

	′	″
Long. of path ſeen in the diſk of ☉	26	57
Lat. ſeen of ☿ in conjunction	9	10
Lat. of ☿ in the ingreſs	10	57
Lat. of ☿ in the egreſs	6	59
Diff. between lat. in egreſs and ingreſs	3	58
Portion of the path between ☌ and middle of the tranſit	1	20

The time of the conjunction, the poſition of the node, and the inclination of the orbit, cannot hence be immediately diſcovered; for there is ſtill required an exact determination of the ſtay of the centre of ☿ on the diſk of ☉, which I cannot ſafely determine from my obſervations. But by comparing the intervals of the times with the diſtances of many places in the path, I found the horary motion to be about 5′ 56″, and therefore that the whole ſtay of the centre of ☿ on the diſk, is pretty near 4^h 33′. And as an error of 1′ or 2′ of time in this caſe, makes but a ſmall difference in the lower node, in the inclination of the orbit, I ſhall briefly ſet down what is produced by this hypothetical calculus. And becauſe, by probable reaſoning, the trepidation of the limb anticipated the contact of ☿ with the inner edge, and conſequently the egreſs, let us ſet down

		h	′	″
The true time of the egreſs of the centre of ☿ on the diſk of ☉ at *Gieſen*		1	37	0
Half the ſtay on the diſk		2	16	30
The middle of the tranſit will be	*Nov.* 4.	23	20	30

* Dr *Bradley* determined the diam. of ☿ to be 10″ 45‴, by a microm. applied to the *Hugenian* teleſc. of above 120 foot long. See Vol. VIII. p. 254.

By

	h	′	″
By the horary motion and portion of the path, between ♁ and middle, the time of the tranſit through that portion will be	0	13	28
Therefore true time of ♁ at *Gieſen* - - - - *Nov.* 4.	23	7	2
Let us therefore lay down the diſt. of merid. between *Gieſen* and the Obſ. at *Paris*, rejecting ſeconds	0	25	0
The true time of ♁ at the Obſervatory at *Paris* will be	22	42	2
Equation of time by the *Ludovician* tables	0	20	24
Mean time of ♁ at the Obſervatory at *Paris*	22	21	38
To this time the place of the ſun by the *Ludovician* tables	♏ 12	37	0
By the diff. between the lat. in the ingreſs, egreſs, and ♁, and by the ſtay of the centre on the diſk $4^h\ 33'$ the time reſults which ☿ finiſhes from ♁ to ☌	h 10	′ 31	″ 25
By tab. *Ludovic.* from which the *Caroline* here ſcarcely differ, in this ſpace of time ☿ proceeds heliocentrically in the ecliptick	2	39	13
Therefore place of the node by theſe hypotheſes	☌ 15	16	13
But if the ſtay of the centre of ☿ be ſuppoſed $4^h\ 32'$ then ☌ will be	☌ 15	15	38
But if the ſtay of the centre of ☿ be ſuppoſed to be $4^h\ 34'$, then ☌ will be	☌ 15	16	47
But if we ſuppoſe the diſt. of ☿ from the earth, to be as 676 to 313, as the Great *Halley* defines it *, then the inclination of ☿ in ♁ will be	h 0	′ 19	″ 47
From this arc and the diſt of ☿ in the ecliptick from ☌ follows at length the inclination of the orbit, and in the firſt caſe. where the ſtay of the centre is ſuppoſed to be $4^h\ 33'$	0	7	5
But if the ſtay of the centre of ☿ on the diſk is ſuppoſed to be $4^h\ 32'$, the inclination of the orbit will be	0	7	6
But if $4^h\ 34'$	0	7	5

Occultation of Cor Leonis *by the* Moon, *on* Thurſday, March 12. 1747, *in* Surrey ſtreet *in the* Strand, London, *with a reflecting teleſcope, made by Mr* Short, *F. R. S. which magni-*

XVI. Apparent Time.

	d	h	′	″	
1747, *Mar.*	12	8	24	19	The ſtar immerg'd into the dark limb.
		9	27	4	It emerg'd from the enlighten'd limb a ſmall matter to the W. of the moon's zenith.
			44	$4\frac{1}{2}$	The moon's preceding limb paſs'd the meridian in the tranſitory.
			44	21	The ſtar paſſed the meridian.

Mr *Short*, another gentleman, and myſelf, agreed to a ſingle ſecond in the immerſion, with different teleſcopes; but I ſaw and pronounced

* See Vol VI. p. 253.

the

the emerſion 2 or 3″ before them. —— There had been an exact obſervation of the ſun's tranſit at noon; and the clock gain'd about ½ a ſecond a day.

We reckon *Surrey-Street* 27″ in time W. of the R. Obſ. at *Greenwich*.

Mr *John Catlin* had a few days before deliver'd me a computation of this eclipſe, corrected from two places of the moon, obſerv'd the 28th of *February* and the 2d of *March* 1729, correſponding pretty nearly with her preſent ſituation; as likewiſe from the ſtar's poſition, as I had rectify'd it from ſeveral late obſervations; and this gave the

fied about 100 *times; communicated to the* Royal Society *by* J. Bevis, *M. D.* N° 483. p. 455. March &c. 1747. *Read* March 19. 1746-7.

	h	′
Immerſion at - - - - - - - - - - - - - - -	8	26½
Emerſion - - - - - - - - - - - - - - - -	9	30

XVII. The comet which appeared towards the end of laſt *Dec.* and in the following months *Jan.* and *Feb.* 1744. was firſt ſeen in *England*, at the Obſervatory of the Earl of *Macclesfield*, *Dec.* 23. between 5 and 6 in the evening. It formed, at that time, an obtuſe-angled triangle, with α of *andromeda*, and γ *pegaſi*, the comet being at the obtuſe angle; and it's paſſage over the meridian was obſerved at 5^h 32′, mean *Oxford* time. His Lordſhip's obſerver could not then take it's diſtance from the *vertex* accurately, the comet's *tranſit* being unexpected; however, by an obſervation made at *Paris* the ſame evening by Mr *Monnier* we have the diſtance very nearly.

A letter from the Rev. Mr Joſ. Betts, *M. A. Fellow of* Univerſ. Coll. Oxon. *to* M. Folkes, *Eſq*; *Pr. R. S. containing obſervations on the late* Comet, *made at* Sherborn *and* Oxford; *with the* Elements *for computing its motions.* N° 474. p. 91. June &c. 1744. *Read* June 14. 1744.

His Lordſhip the next day acquainting the Rev. Mr Profeſſor *Bliſs* with this diſcovery, gave us an opportunity of looking after it at *Oxford*; but, unfortunately, bad weather, and a continued ſucceſſion of cloudy evenings prevented our obſerving it, till *Dec.* 31. but the weather proving more favourable at *Sherborn*, it's R. aſc. and decl. were taken by his Lordſhip, the reſult of which obſervations is as follows:

Note, That the equal time is made uſe of in the following obſervations, and that the comet's *tranſits* (reduced to the meridian of *Oxford*) are only given to the neareſt half minute, as being ſufficient for computing it's places.

Dec. 23^d 5^h 32′) the R. aſc. of the comet by the *tranſit* inſtrument at *Sherborn* was found to be 5° 48′ 1″; and it's polar diſtance by Mr *Monnier* 68° 18′ 35″.

Dec. 27^d 5^h 7′½) the R. aſc. of the comet, obſerved at *Sherborn*, was 3° 41′ 7″; and it's decl. 21° 7′ 13″ N.

Dec. 28^d 5^h 1′½) the obſerved R. aſc. of the comet was 3° 11′ 8″; and it's diſtance from the pole 69° 0′ 38″.

Dec. 31^d 4^h 44′) the R. aſc. of the comet, by the *tranſit* inſtrument, was found to be 1° 44′ 40″; and it's decl. 20° 36′ 37″ N.

The ſame evening, at 5^h 53′ the ſky favouring us at *Oxford*, the diſtance of the comet from *aldebaran*, taken with *Hadley*'s quadrant, was 60° 10′

60° 10′, corrected for refraction, 60° 11′; from *γ pegaſi* 7° 2′½; corrected, 7° 2′ 40″.

Jan. 12ᵈ 9ʰ 10′ the comet followed *φ pegaſi*, in a 5 foot glaſs, 1° 43′ 32″ of R. aſc.; and was more northerly than the ſtar 1° 36′ 00″: the R. aſc. of the ſtar, by the *Greenwich* obſervations at that time, was 354° 52′ 12″, it's decl. 17° 41′ 55″: therefore the comet's R. aſc. was 356° 35′ 44″, and it's decl. 19° 17′ 55″.

Jan. 13ᵈ 6ʰ 30′) the diſtance of the comet from *aldebaran*, at a *medium* of ſeveral trials by the quadrant, was 65° 26′ 50″; corrected for refraction 65° 28′ 10″; it's diſtance from *γ pegaſi* 6° 31′½; corrected, 6° 31′ 45″.

At 8ʰ 20′ the comet followed *φ pegaſi* 1° 21′ 13″ of R. aſc.; and was more northerly than the ſtar 1° 30′ 33″. Hence the comet's R. aſc. was 356° 13′ 25″; and it's decl. 19° 12′ 28″ N.

Jan. 16ᵈ at 6ʰ 33″ the comet's diſtance was obſerved by the quadrant from *aldebaran* 66° 36¾′; corrected for refraction 66° 38′ 10″; from *γ pegaſi* 7° 0′¾; corrected 7° 1′.

At 8ʰ the ſame evening) the comet followed *φ pegaſi* in the 5 foot glaſs 10′ 24″ of R. aſc.; and was more northerly than the ſtar 1° 13′ 24″. Hence the comet's R. aſc. was 355° 2′ 36″; and it's decl. 18° 55′ 19″ N.

Jan. 23ᵈ 6ʰ 11′ the comet's diſtance was obſerved by the quadrant from *aldebaran* 69° 26⅔′; corrected for refraction 69° 28′ 5″; from *γ pegaſi* 8° 42¼′; corrected 8° 42′ 35″.

January 23ᵈ 7ʰ 29′ the comet preceded *φ pegaſi* 2° 43′ 27″ in R. aſc.; and was N. of the ſtar, in the 8 foot glaſs, 26′ 32″. Hence the comet's R. aſc. was 352° 8′ 46″; and it's decl. 18° 8′ 27″.

The comet this evening appeared exceedingly bright and diſtinct, and the diameter of it's *nucleus* nearly equal to that of *Jupiter*'s; it's tail, extending above 16° from it's body, pointed towards *ζ* of *andromeda*; and was in length, ſuppoſing the ſun's parallax 10″ above 23 millions of miles; but cloudy weather ſucceeding, we loſt this agreeable ſight till *Feb.* 5ᵗʰ.

Feb. 5ᵈ 7ʰ 31½′ a ſmall ſtar of *pegaſus*, marked *α* by *Bayer*, preceded the comet in R. aſc. 1° 40′ 20″; and was S. of the ſtar 54′ 23″: the R. aſc. of the ſtar, by the *Greenwich* obſervations at that time, was 343° 0′ 4″; it's decl. 13° 49′ 56″: wherefore the comet's R. aſc. was 344° 40′ 24″; and it's decl. 14° 44′ 19″ N.

Feb. 11ᵈ 6ʰ 37½′ the comet followed *ξ pegaſi*; the correction for refraction being allowed 43′ 1″ in R. aſc.; and was S. of the ſtar 50′ 3″: the R. aſc. of *ξ*, by the *Greenwich* obſervations at that time, was 338° 28′ 24″; it's decl. 10° 51′ 3″: therefore the comet's R. aſc. was 339° 11′ 25″; and it's decl. 10° 1′ N.

Feb. 12ᵈ 6ʰ 33′ the comet followed *ζ pegaſi* 56′ 45″ of R. aſc.; and was more ſoutherly than the ſtar 44′ 42″. The R. aſc. of *ζ*, by the *Greenwich* obſervations at that time, was 337° 10′ 15″; it's polar diſtance

distance 80° 29′ 53″. Hence the comet's R. asc. was 338° 7′ 00″; and it's decl. 8° 45′ 25″ N.

Feb. 13^{d} 6^{h} 25′ the comet preceded ρ *pegasi* 7° 41′ 31″ in R. asc.; and was more southerly than the star 1′ 13″: the R. asc. of the star, at that time, was 344° 41′ 55″; it's polar distance 82° 40′: whence the R. asc. of the comet was 337° 0′ 24″; and it's decl. 7° 18′ 47″ N.

This was the last observation made at *Oxford*; the comet being now so near the sun, and withal so low in the evening, that the great difficulty of finding any star to compare it with, made us desist from attempting it again; however, the prodigious brightness it acquired, by it's near approach to the sun, made it visible in the day-time. And at *Sherborn*,

Feb. 16^{d} 23^{h} 42½′ it's R. asc. by the *transit* instrument, was found to be 333° 13′ 53″; and it's decl. 0° 2′ 40″ S.

Feb. 17^{d} 23^{h} 36′ the R. asc. was observed 332° 33′ 20″; and it's decl. 2° 29′ 00″.

By the help of these observations, which were made by the Rev. Mr Professor *Bliss* (the *transits* excepted taken at *Sherborn*), I was enabled, by the method delivered in the third book of the *Principia*, to determine the comet's parabolic trajectory; and found the place of the ascending node to be in ♉. 15° 45′ 20″; the logarithm of the *perihelion* distance 9,346472: the logarithm of the diurnal motion 0,940420: the place of the *perihelion* ♎. 17° 12′ 55″; the distance of the *perihelion* from the node 151° 27′ 35″: the logarithm, sine, and co-sine of the inclination of the orbit to the ecliptic 9,865138, 9,832616: and thence the time the comet was in the *vertex* of the *parabola*, or the time of the *perihelion*, *Feb.* 19^{d} 8^{h} 12′: the motion of the comet, in it's orbit thus situated, was direct, or according to the order of the signs.

From these elements, by the help of Dr *Halley*'s general table (to which they are adapted), I computed the comet's places for the times of observation, exhibited in the following table: to which are added the comet's longitudes and lat. deduced from the observed R. ascensions and declinations together with the errors between the observed and computed places; the observations being all reduced to *Oxford* mean time.

Equal time at *Oxford.*				Longit. Comet observed.			North Latit. observed.			Longit. Comet computed.			North Latit. computed.			Diff. in Long.	Diff. in Latit.
	D	H	′		°	′	″	°	′	″		°	′	″	°	′	″	″	″
1743. *Dec.*	23	5	32	♈	14	10	2	17	33	11	♈	14	10	3	17	33	37	1—	26—
	27	5	7½	♈	12	2	25	17	51	29	♈	12	2	26	17	51	47	1—	18—
	28	5	1½	♈	11	32	11	17	55	54	♈	11	32	14	17	56	8	3—	14—
	31	4	44	♈	10	4	57	18	9	3	♈	10	5	16	18	8	53	19—	10+
		5	53	♈	10	4	11	18	9	37	♈	10	3	55	18	9	6	16+	31+
1743. *Jan.*	12	9	10	♈	4	52	5	18	59	37	♈	4	52	24	18	59	13	19—	24+
	13	6	20	♈	4	31	40	19	2	31	♈	4	31	13	19	2	49	27+	18—
		8	20	♈	4	29	27	19	3	32	♈	4	26	6	19	3	12	21+	20+
	16	6	33	♈	3	18	43	19	15	47	♈	3	18	27	19	15	13	16+	34+
		8	00	♈	3	17	31	19	16	7	♈	3	17	00	19	15	30	31+	37+
	23	6	11	♈	0	19	45	19	42	30	♈	0	19	16	19	42	1	29+	29+
	23	7	29	♈	0	17	58	19	42	47	♈	0	17	45	19	42	12	13+	35+
1744. *Feb.*	5	7	31½	♓	21	52	37	19	35	00	♓	21	52	56	19	34	42	19—	18+
	11	6	37½	♓	14	42	45	17	23	30	♓	14	42	58	17	24	5	13—	35—
	12	6	33	♓	13	10	36	16	38	40	♓	13	10	52	16	39	17	16—	37—
	13	6	25	♓	11	32	50	15	43	45	♓	11	33	16	15	44	16	26—	31—
	16	23	41½	♓	5	9	14	10	17	40	♓	5	9	1	10	18	8	13+	28—
	17	23	35	♓	3	37	37	8	15	39	♓	3	37	11	8	16	3	26+	24—

Perhaps

Perhaps it may not be thought foreign to my purpoſe to remark, that the nodes of the comet, and the planet *Mercury*, are ſituated within leſs than half a degree of each other; which, I ſuppoſe, gave riſe to a report, that the comet had carried *Mercury* from it's orbit. In order therefore to find how nearly they approached each other, I had the curioſity to bring the matter to calculation; and preſently found, there was above a week's difference in the times of their coming to the nodes; the comet paſſing it's deſcending node, *Feb.* 22. about 2^h in the morning; and *Mercury* not coming to his till *Feb.* 29. the comet moving all that time ſouthwards with a prodigious velocity. Again, computing their heliocentric conjunction, which happened *Feb.* 18. about 1^h in the afternoon, I found the comet was, at that time, diſtant from *Mercury* nearly $\frac{1}{5}$ part of the ſemidiameter of the *orbis magnus*; being almoſt twice as near to the ſun as the planet ☿; and having then 31° 30′ of N. Lat. *Mercury*'s not exceeding 3° 58′ to an eye in the ſun: whence it is eaſily collected, that the comet could have no ſenſible influence upon ☿'s motion.

I ſhall now only beg leave to obſerve, that the elements above-given cannot poſſibly differ much from the true. For, after an interval of two months (in which time the comet had gone through almoſt $\frac{1}{3}$ part of it's orbit), it is ſurpriſing to find the obſerved and computed places agree ſo accurately, that the difference no-where amounts to a minute. In ſome parts of the orbit, the agreement is ſtill greater; particularly, in the obſervations made at *Sherborn*, which come within half that quantity; and would have correſponded ſtill nearer, but that I was ambitious to confine the whole ſeries of obſervations within the narrow limit above-mentioned; which I have at laſt compaſſed, not without a long and tedious calculation.

It may, perhaps, be expected, conſidering the great part of it's orbit the comet deſcribed during it's appearance, that I ſhould have ſettled it's period, and foretold it's return. This, I confeſs, would have given me great pleaſure; neither would I have ſpared any pains in the inquiry, had I met with any proſpect of ſucceſs; but the period, upon my attempting it at firſt, came out ſo prodigiouſly long, the tranſverſe ax of the ellipſe being nearly equal to infinity, that I was ſtopped ſhort in my inquiry; neither could I prevail upon myſelf to reſume the ſubject again, when, upon turning over *Hevelius*, I found the account of comets, which had appeared at long intervals of time from us, as it might reaſonably be expect'd, ſo ſhort and uncertain: but, could I procure *Celſius*'s obſervations, or any made after the *Perihelion*, I might be induced to fall to work again; and would not fail communicating the reſult, did I meet with ſucceſs; and, at the ſame time, the elements of the comet, which appeared in 1742, which I have had by me ſome time; not ſo perfect as I could wiſh, but as perfect as may be obtained from the few obſervations I met with.

The comet was in conjunction with the ſun, *Feb.* 15. about midnight; and it's perigee, *Feb.* 16. about 1^h in the afternoon; at which time it was ſomewhat nearer the earth than the ſun is at it's perigee; the comet's

The Path of the Comet, which appeared from the beginning of March 1742, to the beginning of April, from the observations made at the Observatory and College of the Jesuits at Pekin in China, and computed according to the equator and ecliptick, and also according to it's proper orbit; communicated by Mr James Hodgson, F. R. S. & Schol. Reg. Math. Præc. in Ed. Christi, Lond. No. 481, p. 264. Oct. &c. 1746.

True time of observation.	Right Ascension.	Decl. from the Equator.	Force in it's proper orbit.	Longitude in the ecliptick.	N. Lat. in the ecliptick.	Digression from the node in the ecliptick.	Constellations to which the Comet passed.
d. h. min.	° ′	° ′	° ′	° ′	° ′	° ′	
Mart. 2 4 30. m.	281 55	6 0 A	0 0	♑ 12 24	16 58	17 14	To the foot of Antinous.
4 4 0. m.	283 30	5 15 B	11 18	14 44	28 4	28 33	Near the tail of the Serpent.
5 4 45. m.	283 33	10 50	16 55	16 2	33 33	34 9	Below the tail of Aquila.
7 4 0. m.	284 48	22 40	28 48	19 32	45 9	46 3	Between Anser and Cerberus.
11 2 30. m.	288 1	44 57	51 15	♒ 3 6	66 22	68 29	Between the wing of Cyg. and Lyra.
12 4 30. m.	289 6	50 3	56 24	9 56	70 53	73 38	In the Northern wing of Cygnus.
13 3 15. m.	290 11	54 15	60 39	18 19	74 20	77 53	
14 4 0. m.	291 40	58 50	65 18	♓ 2 20	77 33	82 32	Between Cygnus and the belly of Draco.
15 3 15. m.	293 12	62 36	69 9	19 20	79 22	86 23	
16 4 0. m.	295 0	66 0	72 38	♈ 8 35	79 59	89 32	In the belly of Draco.
17 4 30. m.	297 10	69 11	75 55	26 57	79 31	93 11	
18 4 0. m.	299 34	71 50	78 41	♉ 10 11	78 23	95 56	Between Draco and Cepheus.
19 4 0. m.	302 39	74 23	8 123	20 27	76 49	98 38	At the knee of Cepheus.
8 20. v.	304 38	75 40	82 47	24 46	75 53	100 1	
22 9 0. v.	319 56	81 0	88 54	♊ 8 19	71 5	106 8	Between the feet of Cepheus, and then in the same place in the neighbourhood of the N. pole.
23 9 45. v.	327 25	82 14	90 32	10 52	69 41	107 47	
24 10 15. v.	336 22	83 12	92 1	12 53	68 23	109 16	
27 9 0. v.	21 24	84 26	96 44	18 7	64 0	114 7	
28 8 40. v.	26 28	84 20	97 26	18 34	63 32	114 38	
29 1 30. m.	30 34	84 13	97 52	18 56	63 8	115 4	
30 2 0. m.	38 13	83 54	98 42	19 38	62 21	115 55	
31 2 50. m.	45 3	83 29	99 33	20 19	61 33	116 46	
Apr. 1 2 50. m.	50 51	83 0	100 23	20 56	60 47	117 36	
2 3 12. m.	55 55	82 27	101 13	21 32	59 59	118 37	

distance being then (,83) and the sun's (,98) such parts, as the semidiameter of the *magnus orbis* is (,100); from which we may have some idea of the comet's magnitude; and therefore may suppose it, at least, equal to the earth.

XVIII.

But

But from the obſervations made *March* 2 and 4, it is manifeſt, that the comet came to the equator *March* 3. about 6^h *a. m.* and that it paſſed in R. aſc. 282° 30′, with inclination of it's path to the equator 84° 30′ very nearly; and therefore that it's long. was 13° 35′ in ♑, with N. lat. 22° 54′. Hence we may collect, that the path of the comet, which did not ſeem to deviate from a great circle, met the ecliptick in ♑ and ♋ 9° 19′ with incl. of 80°: and the colure of the equinoxes in the diſtance of 50° 37′½ from the poles of the world toward the equinoctial points with the angle of incl. 77° 33′⅓: and the colure of the ſolſtices in the diſt. of 23° 57′⅓ from the poles of the world, toward the ſolſticial points with ang. of incl. 13° 38′ equal to the greateſt elongation of it's orbit from the ſame colure in the averſe part, and to the diſt. of the poles of the orbit from the equinoctial points.

The Paths of Comets *according to the Hypotheſis which makes them deſcribe a Parabola about the* Sun; *by* Nicholas Struyck, *F. R. S.* N° 492. p. 89. April, &c. 1749. *Preſented* April 6. 1749.

XIX. That the tracing of the courſes of comets belongs to the principal parts of the ſublimer Aſtronomy, has been paſt all doubt, ever ſince the great *Newton* 63 years ago publiſhed a problem of finding the path of comets by 3 accurate obſervations, from this hypotheſis, that they deſcribe a parabola about the ſun in their courſe. Dr *Halley* by this method determined the paths of 24 comets, by calculation, in a table publiſhed in the *Phil. Tranſ.* N. 297. p. 1886. and in the *Acta Erud.* 1707. p. 216. There are in reality, 21 different comets. The difficulty and neceſſity of this work has been ſufficiently ſhewn by the laſt mentioned Aſtronomer.

Following the ſteps of ſo great a man, I have noted by the ſame method, 18 other comets, which are not found in that table, in hopes that the periodical time of each may at length be found. But leſt thoſe obſervations of the paths of comets, ſhould, by any accident be loſt, I determined now to publiſh them, at the ſame time, thinking it my duty to mention thoſe who have accommodated them to an arithmetical calculus. The path of the comets of 1723 and 1737 was determined by Dr *Bradley*; of 1744, by Mr *Betts*; of 1699, 1702, and 1739, by the Abbot *de la Caille*. The path of the 2d comet of 1743 by Mr *Klinkenberg*; that of the 2d comet of 1746 by *M. des Chezeaux*; of the 1ſt comet of 1748, by *Maraldi*. I gave the obſervations of the comets ſeen in 1533, 1678, 1718, and 1729, to Mr *C. Downes* to be calculated. But the comets of 1706, 1707, 1742, the 1ſt of 1743, and the 2d of 1748, I calculated myſelf. I am alſo induced by various reaſons to be of opinion, that in *May* 1748, both here at *Amſterdam*, and in other places of *Europe*, on the very ſame night, 3 *Comets* were viſible; of which there is no other certain inſtance in Hiſtory. I have alſo added the comet ſeen at the end of 1680, and beginning of 1681; becauſe, in the laſt edition of Sir *I. Newton*, there are emendations, by which the ellipſis, that it deſcribed about the ſun, is determined. I ſhall only add, that of the 31 obſervations which I have of the comet ſeen in 1742, there are 22, the longi-

tudes

tudes of which ſcarce differ 1′; and 23 of which the latitudes do not differ ſo much as 1′.

Here follow the paths of the 19 comets mentioned above.

Time of the equations of the Perihelion at London.						Node of aſcenſion.			Inclination of the Orbit.			Perihelion in the Orbit.			Dif. Perih. from the Sun.	Motion.
An.		d	h	′	″	☊	°	′	″	☊	′	″	☊	°	′	″	Part.	
1533	June	16	19	30	0	☊	5	44	0	35	49	0	☊	27	16	0	20280	Retro.
1678	Aug.	16	14	3	0	♍	11	40	0	3	4	20	♒	27	46	0	123802	Dir.
1699	Jan.	2	8	22	19	♒	21	45	35	69	20	0	♏	2	31	6	74400	Retro.
1702	March	2	14	12	19	♎	9	25	15	4	30	0	☊	18	41	3	64590	Dir.
1706	Jan.	19	4	56	0	♈	13	11	23	55	14	5	♊	12	36	25	42686½	Dir.
1707	Nov.	30	23	43	6	♉	22	50	29	88	37	40	♊	19	58	9	85904	Dir.
1718	Jan.	4	1	14	55	☊	7	55	20	31	12	53	☊	1	26	36	102565	Retro.
1723	Sept.	16	16	10	0	♈	14	14	16	49	59	0	♉	12	15	20	96942	Retro.
1729	June	12	6	35	41	♒	10	35	15	77	1	58	♒	22	16	53	406980	Dir.
1737	Jan.	19	8	20	0	♏	16	22	0	18	20	45	♒	25	55	0	22282⅓	Dir.
1739	June	6	9	59	49	♈	27	25	14	55	42	44	♋	12	38	40	67358	Retro.
1742	Jan.	28	4	20	50	♎	5	34	45	67	4	11	♏	7	33	44	76555½	Retro.
1742	Dec.	30	21	15	16	♊	8	10	48	2	15	50	♋	2	58	4	83811½	Dir.
1743	Sept.	9	21	16	18	♈	5	16	25	45	48	21	♐	6	33	52	52157	Retro.
1744	Feb.	19	8	17	0	♉	15	45	20	47	8	36	♎	17	12	55	22206 1/10	Dir.
1747	Feb.	17	11	44	38	☊	26	58	27	77	56	55	♑	10	5	41	229388	Retro.
1748	April	17	19	25	4	♏	22	52	16	85	26	57	♏	5	0	50	84066⅔	Retro.
1748	June	7	1	24	15	♉	4	39	43	56	59	3	♑	6	9	24	65525½	Dir.
1680	Dec.	7	23	9	0	♑	2	2	0	6	6	48	♐	22	44	25	617	Dir.

But the *perihelian* diſtances are eſtimated in ſuch parts, as the mean diſtance of the earth from the ſun has 100,000.

Various aſtronomical obſervations made in Paragua *in* S. America; *communicated by* Jacob de Caſtro Sarmento, *M. D. F. R. S.* No. 490. p. 667. Dec. 1748. *Preſented* Jan. 28, 1747-8.

XX. 1. *Eclipſes of the ſun and moon obſerved in the miſſions of the Jeſuits to* Paragua, *by* F. Bonaventura Suarez, *Miſſionary, with a 5 foot teleſcope, and a pendulum, vibrating ſeconds, with an equal motion, and rectified to true time, by the altitude of the fixt ſtars.*

Eclipſe *of the* Sun, Nov. 5. 1706.

This eclipſe was obſerved at the town of *S. Ignatius* in *Paragua*, where the altitude of the S. pole is 26° 52′, and it's merid. diſt. from the R. Obſerv. at *Paris* 3ʰ 57′ 50″.

		h	′	*St. civ.*
Beginning of the eclipſe		8	52	*a. m.*
Digits obſcured - -	2	9	15	
	3½	9	40	
	4	10	0	
	1	11	5	
End		11	15	

The greateſt quantity at 9ʰ 50′ dig. 4. 0′

Beginning

Beginning below the horizon: ſun riſe 5^h 53′.

——March 11. 1709. *St. civ.*

	h	′	*a. m.*
Digits obſcured 9	20	6	15
6	30	6	50
6	0	6	54
5	0	7	3
3	30	7	13
3	0	7	17
3	30	7	21
1	30	7	28
End of the eclipſe		7	37 15″

The greateſt obſcuration was $9^{dig.}$ 20″

——Moon, April 16. 1707. *p. m.*

	h	′
Beginning	7	55
Total obſcuration	8	58
Beginning of emerſion	10	45

The end was not obſerved becauſe of clouds.

——April 4. 1708. *p. m.*

Immerſion of ☽	h	′	″	*Emerſion of* ☽	h	′	″
Into a ſenſible penumbra.	12	18	0	*Ariſtarchus*	14	13	15
Into the ſhadow	12	30	29	*Plato*	14	45	0
Ariſtarchus obſcured	12	37	11	Out of the ſhad.	15	3	0
Plato obſcured	12	46	0	Out of the pen.	15	12	0

——Sun, Jan. 18. 1730. *p. m.*

	h	′	″	*Digits obſcured.*
Beginning	2	52	30	0 0
	2	58	10	1 0
	3	5	0	2 0
	3	19	45	4 15
	3	29	20	5 45
	3	21	22	6 0
	3	39	17	7 0
	3	41	55	7 20
	3	45	0	7 40
	Clouds			
	4	7	33	8 0
	4	9	36	7 45
	4	11	34	7 30
	Clouds			
	4	51	0	4 0
	4	42	0	2 0
	4	50	0	0 30

The end was not obſerved becauſe of clouds; it ſeems to have been at 4^h 52′;

4^h $52'$: at about 4^h $55'$ the diſk of ☉ was ſeen entire: ☽ did not appear on his limb.

The greateſt obſcuration ſeems to have been dig. $8\frac{1}{3}$.

——*Total of the* Moon, Aug. 8. 1729.

	h	′	″
Clouds			
Beginning of emerſion	10	1	0
Digits obſcured 11	10	6	28
Digits 6	10	33	2

Occultation of Jupiter *by the* Moon, Dec. 9. 1729. *p. m.*

h	′	″	
11	3	5	☽ eclipſed a ſatellite of ♃
11	13	25	☽ touched the limb of ♃
11	15	0	☽ totally eclipſed ♃.

Eclipſe *of the* Moon, Dec. 1. 1713. *p. m.*

This eclipſe was obſerved in the town of *S. Joſeph.* Diff. of merid. from R. Obſ. *Par.* 3^h $52'$ $30''$.

	h	′	″
Beginning	10	33	31
End	12	56	57

The greateſt quantity obſcured was dig. 5. at about 11^h $45'$.

——March 26. 1717. *p. m.*

This eclipſe was obſerved on the very meridian of *S. Coſma.* Diff. of merid. from *Paris* 3^h $52'$ $20''$. Sky clear and calm.

		h	′	″
Senſible penumbra		9	40	0
Beginning of the eclipſe		10	2	21
Digits obſcured	1	10	8	30
	2	10	15	2
	3	10	13	41
	4	10	31	32
	5	10	40	56
	6	10	52	8
	7	11	10	40

The greateſt quantity obſcured ſeemed to be $7^{dig.}$ $18'$

Emerſion of ☽ *from the ſhadow.*

		h	′	″
Digits obſcured	6	11	45	40
	5	12	6	25
	4	12	16	35
	3	12	24	10
	2	12	32	46
	1	12	39	25
End of the eclipſe		12	45	40
Emerſ. from penumb.		13	1	0

This

This eclipſe was obſerved in the town of *S. Michael the Archangel* with a tube of 10 foot. — Feb. 24. 1728. *p. m.*

Diff of time between *S. Mich.* and R. Obſ. *Par.* $3^h\ 48'\ 50''$.

	h	′	″
Beginning of the eclipſe	14	3	35
End	17	0	37

Digits obſcured at the middle of the eclipſe $9^{dig.}\ 40'$.

This total eclipſe of ☽ was obſerved in the college *de las Corrientes*. Diff. merid. from *Paris* about $4^h\ 2'$. — Mar. 4. 1700. *p. m.*

	h	′
Beginning of the eclipſe	13	14
Total immerſion	14	34
Beginning of emerſion	16	15
End of the eclipſe	17	15

	h	′	″	Tubes	
Emerſion of the 1ſt ſatellite obſerved at *S. Ignat.*	10	52	49	13 foot	— Satellites *of* Jupiter, Dec. 21. 1729. *p. m.*
——— at *Peterſburg* by *M. Nic. de l'Iſle*	16	42	36	15	
Diff.	5	49	47		
Immerſion of the 4th ſatellite at *S. Ignatius*	7	23	0	18	— Mar. 27.
Peterſburg	13	12	31	13	1730.
Diff.	5	49	31		
Emerſion of the 2d ſatellite at *S. Ignatius*	6	36	45	13	— Apr. 8.
Peterſburg	12	26	15	13	1730. *p. m.*
Difference of meridians	5	49	30		

The following *phænomena* of the ſatellites of ♃ were obſerved at *S. Ignatius p. m.*

At $14^h\ 21'$ there was a conjunction of the 1ſt with the 2d both ſtars ſeemed to be but one. — Dec. 29, 1729.

At $9^h\ 10'$ there was a conjunction of the 1ſt and 2d. — Jan. 23.

At $15^h\ 21'\ 15''$ the 1ſt and 2d were conjoyned, ſo as to appear but one. At $15^h\ 27'$ one was yet viſible: but at $15^h\ 36'$ they were diſjoined. — 1730. Jan. 25.

At $11^h\ 36'$ there was a conjunction of the 2d and 4th. — Mar. 9.

At $10^h\ 9'$ there was an occultation of the 2d being retrograde in the margin of ♃. — 12.

At $6^h\ 38'$ there was a conjunction of the 2d and 3d. — 18.

At $9^h\ 7'\ 40''$ there was an occultation of the 3d direct in the margin of ♃. — 29.

Mar. 30. At $7^h\ 21'\ 30''$ there was an occultation of the 2d retrograde in the limb of ♃.

31. At $9^h\ 21'\ 15''$ there was an occultation of the 1ſt retrograde in the limb of ♃.

Apr. 1. At $6^h\ 36'\ 25''$ with a tube of 18 f. I obſerved an occultation of the 1ſt direct in the margin of ♃: but at $10^h\ 16'\ 57''$ it emerged from the ſhadow of ♃.

—— Jupiter. Dec. 9. 1729. *p. m.* At $11^h\ 3'\ 5''$ the margin of ☽ touched the 3d ſatellite of ♃. The beginning of the occultation of ♃ was $11^h\ 13'\ 15''$. The total occultation of ♃ in the margin of ☽ was $11^h\ 15'$.

—— Saturn. Apr. 27. 1730. The *anſulæ* of ſaturn appeared very thin; but *May* 8 17^h ſaturn was round and quite deprived of his *anſulæ*.

Aſtronomical obſervations of F. Bonaventura Suarez, *in the town of* S. Ignatius *in* Paragua. *There is another town of* S. Ignatius *more to the E. on the river* Zabebiri.

The town of S. Ignatius, more weſtward than the reſt, is 50 *Spaniſh* leagues to the S. from the city of *Aſſumption* in *Paragua*. Lat. of *Aſſumption* obſerved by me is 25° 14′ S. lat. of S. *Ignatius* 26° 52′.

		h	′	″
Diff. mer. S. *Ignat.* W. from	*Peterſburg*	5	59	40
	Paris	3	57	50
	London	3	48	40
	S. Coſma	0	5	30

Emerſions of the 1ſt *p. m.*

—— *Satellites of* Jupiter.

		d	h	′	″			foot
1729	*Jan.*	16	8	16	42	tube		13
	Feb.	18	8	28	13	-	-	13
	Mar.	6	6	50	18	-	-	13
		13	8	47	4	-	-	13

Immerſion of the firſt

	Nov.	3	16	20	36	-	-	13
	Dec.	21	10	52	49	-	-	13
1730	*Jan.*	6	9	0	40	-	-	18
		13	10	53	8	-	-	18

Emerſions of the firſt

1730.	*Feb.*	7	7	46	20	-	-	13
	Mar.	9	9	56	21	-	-	13
		16	11	53	21	-	-	18
		25	8	49	46	-	-	18
	Apr.	1	10	16	57	-	-	18
		17	8	39	55	-	-	18
	May.	3	7	1	47	-	-	18
		10	8	56	51	-	-	18

Emerſions of the ſecond *p. m.*

	Apr.	8	6	36	45	-	-	18
	May.	10	6	32	30	-	-	18

Emerſion

	d	h	′	″		
					Emerſion of the third foot	
Apr.	20	8	44	45	tube	18
					Emerſion of the fourth	
Mar.	10	9	22	0	doubtful	
					Immerſion of the fourth	
Mar.	27	7	23	0	- -	18

—— *communicated by* — Suarez, M. D. No. 491. p. 8. Jan. *&c.* 1749. *Preſented* Jan. 19. 1748-9.

2. The ſky was ſo cloudy, that I could make only the following obſervations, after the emerſion of the ſecond digit of the moon from the ſhadow of the earth, with a teleſcope of $10\frac{1}{2}$ feet.

	h	′	″	
Ariſtarchus emerges	14	31	47	Eclipſe *of the* moon *obſerved at* S. Angelus Cuſtos, Feb. 24. 1747.
Tycho emerges	14	37	30	
Calippus emerges	14	56	47	
Dionyſius emerges	15	0	4	
Mare criſium begins to emerge	15	13	17	
End of the eclipſe	15	16	4	

The town of *S. Angelo* in the miſſions of *Paragua* is more eaſtward than the reſt. It's longitude from the Iſland of *Ferro* is 323° 30′ and lat. 28° 17′ S.

The ſenſible penumbra was $14^h\ 44'$

Immerſion of the moon and ſpots into the ſhadow.

	h	′	″	
Beginning	14	55	44	— *at* S. Maria Major, *Aug.* 19. 1747. *p. m.*
Ariſtarchus	15	0	13	
Galilæus	15	0	41	
Mare humorum begins	15	4	14	
Lower angle of *terra pruinæ*	15	5	29	
Copernicus &c.	15	9	26	
Mare humorum entire	15	9	26	
Plato and *Tycho* were equally diſtant from the centre of the ſhadow	15	13	44	
The ſame	15	17	2	
Plato and *Tycho* at the ſame time in the edge of the ſhadow	15	20	25	
6 digits obſcured	15	24	6	
Menelaus	15	27	28	
Dionyſius	15	29	35	
Lacus ſomniorum	15	36	10	
Beginning of *mare criſium*	15	43	41	
Middle	15	46	26	
End	15	49	16	
Total obſcuration of ☽	15	53	16	

Emerſion of ☽ from the ſhadow

	h	′	″
Beginning of the emerſion	17	34	48
Grimaldus	17	36	52
Ariſtarchus	17	40	0
Plato	17	53	34
Tycho	18	0	23
6 digits obſcured	18	3	30

The moon near the W. trembling with the vapours of the horizon was no longer obſerved. The ſky was very clear during the whole time of the eclipſe.

Long. of *S. Maria Maj.* from *Ferro* 322° 40′ lat. 27° 51′ S.

— *by* F. Auguſtin Hallerſtein, *Preſ. of the imperial* College *of Aſtron. at* Pekin *in* China, *to Dr* Mortimer, No. 494. p. 305. Jan. &c. 1750. *Read* Jan. 18. 1749.

3. The comet ſeen by us this year was very diſmal, for beſides it's ſhining with a very obſcure and malignant light, it went in ſo deſert a path, Fig. 9. and in ſuch an unfavourble ſky, that it could be obſerved but very ſeldom, and compared but with a few ſmall ſtars not well known.

The Path *and* Ephemeris *of the* Comet *ſeen at* Pekin *in* 1748.

Apr. 26. about 3 in the morning it was firſt ſeen by thoſe whoſe office it is to watch in the Obſervatory of this palace; and the place of it was rudely obſerved to be in 18° ♓ with 27° N. lat. namely in the breaſt of *pegaſus* under the ſtars λ and μ, the head was equal to a ſtar of the 3d order, and the tail ſeemed to be about 1° long.

Fig. 9.

On the following days there was no poſſibility of comparing it accurately with any fixed ſtar, and therefore ſome places of it can only be groſsly determined by configurations with the neighbouring ſtars, which as they cannot bear the ſtrictneſs of a calculus, do but barely ſhew the path which the comet held.

Apr. 27. about 2 in the morning long. ♓ 21° 21′ with N. lat. 31° 35′.

Apr. 28. about the ſame time ♓ 25° 15′ N. lat. 36° 0′

Apr. 29. about the ſame time ♓ 29° 10′ N. lat. 40° 0′

Apr. 30. and *May* 1. nothing ſeen for the clouds.

May. 2. the comet could at laſt be compared with a little ſhining ſtar in the middle of 5 ſmall ſtars in the bend of the chain of *Andromeda*, and is obſerved by means of a micrometer and pendulum 2ʰ 31′ 49″ true time. The comet more eaſtward than the ſtar 1′ 50″ of the pendulum, and more northward 57′ 8″.

May 6. in the morning the comet was compared with a ſmall ſtar, the 6th in order in *Caſſiopea* in *Flamſted's Cat. Brit.* by the differences of declinations and diſtances, on account of the want of clearneſs in the ſky, the ſlow motion, and malignant light of the comet, it's tranſit could not be determined by ſeveral ſeconds; at 2ʰ 3′ 57″ true time the comet

was

was more northward than the ſtar 44′ 8″ and the diſtance being taken was 50′ 50″.

On the following days the comet was compared with ſeveral unknown ſmall ſtars, ſo that it's place could not be determined.

May 15. about 9^h *p. m.* for the moment of time was forgotten to be noted, the comet was ſeen between 2 ſmall ſtars, from the neareſt of which it was diſtant, not in an inverted but right ſituation 11′ 3″ S. and from the farther 59′ 58″ N. It was alſo more to the E. than the neareſt 1′ of the pendulum. Theſe 2 ſtars are placed in the *Cat. Brit.* in *Caſſiopea*, the more northern about the end, in ♊ 4° 49′ 7″ with N. lat. 58° 6′ 56″, and the more ſouthern in ♊ 3° 28′ 12″, with N. lat. 57° 11′ 10″, in 1690.

May 16. the comet being compared with the more northern 11^h 1′ 59″ *p. m.* was more eaſtward than the ſtar 18′ 26″ of the pendulum, and more to the N. 26′ 4″.

May 19. the comet being compared with the ſtar of the 6th magnitude in *Cepheus* according to *Hevelius*'s *Cat.* in 1700, where it is called *ſub faſcia ſequens*, was at 10^h 23′ 29″ in the ſame R. aſc. with the ſtar: there could not be found any difference of time between their tranſits, and the comet was more northward than the ſtar 48′ 14″. On the following days nothing certain could be determined.

May 29. the comet was ſeen amongſt ſeveral unknown ſmall ſtars, and on moving the tube a little there appeared one which *Hevelius*, in his *cat.* of fixed ſtars 1660, places in *Camelopardalus*, and *calls ſupra tergum, ſive in cuſpide pedis ſiniſtri Cephei 5 magnit.* But as the parallel of this ſtar was too far diſtant from the parallel of the comet to be immediately compared with it, it was compared with an intermediate ſtar, and the comet is noted 11^h 21′ 25″ *p. m.* more E. than that ſtar 16′ 13″ of the pendulum, and more S. 1° 37′ 22″.

After this, as there were no ſtars near the path of the comet, nor any like to be, with which it could be compared, they were to be ſought farther off: and therefore on the following days it was compared with *γ Cephei*, from the parallel of which it was not very diſtant. Therefore, the teleſcope being well fixt,

		h	′	″	
June	1. *p. m.*	9	30	53	*γ Cephei* to the horary.
	2. *a. m.*	3	24	51	The comet to the horary more N. than the ſtar 26′ 59″.
	4. *p. m.*	8	28	58	*γ Cephei* to the horary.
	5. *a. m.*	2	41	9	The comet to the horary more S. than the ſtar *γ* 8′ 20″.
	p. m.	8	23	23	*γ Cephei* to the horary.
	6. *a. m.*	2	41	17	The comet to the horary more S. than the ſtar 20′ 13″.
	7. *p. m.*	8	5	3	*γ Cephei* to the horary.

8. *a. m.*

8. *a. m.* $2^h\ 32'\ 51''$ The comet to the horary more S. than the ſtar $42'\ 51''$.
p. m. 8 31 59 γ *Cephei* to the horary.
9. *a. m.* 3 4 38 The comet to the horary more S. than the ſtar $55'\ 34''$.

12. *p. m.* When there was hardly any hope of ſeeing the comet any longer, I ſaw it obſcurely, more like the footſtep of a comet than the comet itſelf. Beſides the brightneſs of the moon, and the reflexion of it's rays from the clouds, which make all obſervations difficult and doubtful, were great obſtructions. I compared it however as well as I could with a ſmall ſtar, which I afterwards found in a little map and catalogue of M. *de la Caille*, inſerted in the *Mem. de l'Acad.* 1742. on account of a comet obſerved in that year, and noted with R. aſc. at that time $91^\circ\ 21'$ and N. decl. $73^\circ\ 49'$; and marked with the letter *A*; therefore $9^h\ 33'\ 6''$ *A* to the horary: then $9^h\ 45'\ 23''$ the comet to the horary more N. than the ſtar $46'\ 34''$.

June 13. *p. m.* $9^h\ 13'\ 11''$ *A* to the horary: $9^h\ 29'\ 43''$ the comet to the horary more N. than the ſtar $36'\ 15''$.

June 14. *p. m.* $9^h\ 15'\ 44''$ *A* to the horary $9^h\ 36'\ 4''$: the comet to the horary more N. than the ſtar $25'\ 47''$. Then being ſeen at $9^h\ 55'$ the diſtances were meaſured of the comet from the ſtars.

I $25'\ 9''$, *B* $38'\ 39''$, *R* $43'\ 3''$.

But the comet ſtood juſt by the ſtar $\mathcal{Q}$, all which ſtars are noted in the place cited above.

June 15 and 16. nothing could be obſerved becauſe of clouds.

June 17 and 18. F. *Ant. Gaubil* obſerved in my abſence from home as follows;

June 17. *p. m.* $9^h\ 26'\ 30''$ *A* to the horary.
53 35 *R* to the horary.
9 55 15 The comet to the horary in the ſame parall. with *A*.
June 18. *p. m.* 9 52 14 *A* to the horary.
10 19 21 *K* to the horary.
9 24 50 The comet to the horary more S. than *A* $16'\ 30''$.
R 10 20.

The R. aſc. of the ſtar *R* is $98^\circ\ 6'\frac{1}{2}$ and N. decl. $73^\circ\ 43'$. The times of the obſervations are all true and ſufficiently correct. They were made with a tube of 6 feet, in which an *Engliſh* micrometer was inſerted.

A new conſtellation. Fig. 10.

A ſmall conſtellation was obſerved *May* 29 in purſuing the comet with the teleſcope. It is repreſented in Fig. 10. The diſtances of the ſmall ſtars that compoſed it were α from $\beta\ 12'\ 19''$: β from $\gamma\ 16'\ 45''$: γ from $\delta\ 10'\ 2''$: δ from $\varepsilon\ 16'\ 45''$; γ from $\zeta\ 19'\ 53''$: δ from $\zeta\ 28'\ 17''$: γ from ϑ, which is the *ſupra tergum* of *Camelopardalus* $58'\ 16''$: δ from the ſame $50'\ 3''$. But I leave theſe diſtances for others to meaſure more

more accurately, The ſituation of this afteriſm is not inverted but right.

h	′	″	
4	52	17	♂ diſtant from the lucid limb of ☽ 50′ 4″.
			Then ♂ was obſerved and compared with the ſtar ι. and ſo
5	3	9	♂ to the horary.
	12	47	ι. to the horary more N. than ♂ 41′ 6″.
	9	38	Temporary difference.
5	18	26	♂ to the horary.
	28	2	ι. to the horary more N. than ♂ 41′ 2″.
	9	36	Temporary difference.
5	34	34	♂ entering under the obſcure limb of ☽ wholly diſappeared, diſtant from the N. horn of ☽ 23′ 28″.
			The diameter being immediately meaſured was 32′ 53″.
			But the lucid part of ☽ was 7′ 39″.
			In the mean time whilſt ♂ lay hid behind ☽, it was obſerved and compared with ♃ ♍ and ſo:
5	58	9	♃ to the horary.
6	3	31	Lucid limb of ☽ to the horary.
	4	23	N. horn of ☽ to the horary.
		58	S. horn of ☽ to the horary.
			The S. limb of ☽ was more S. than ♃ 34′ 27″.
			Again,
6	11	36	♃ to the horary.
	17	22	Lucid limb of ☽ to the horary.
	18	12	N. horn of ☽ to the horary.
		46	S. horn of ☽ to the horary.
			The S. limb of ☽ more S. than ♃ 31′ 0″.
			Thirdly,
6	23	22	♃ to the horary.
	29	26	Lucid limb of ☽ to the horary.
	30	17	N. horn of ☽ to the horary.
		52	S. horn of ☽ to the horary.
			S. limb of ☽ was more S. than ♃ 27′ 48″.
6	38	52	There was a very ſmall ſtar approaching to the obſcure limb of ☽, and when it was juſt entering, it was diſtant from the lucid limb of ☽ 41′ 23″. Then,
6	46	2	♂ firſt appeared coming from under ☽ and diſtant from the N. horn 29′ 24″.
7	2	23	The abovementioned little ſtar entered the dark limb of ☽ diſtant from the N. horn 29′ 24″.
			Laſtly,
7	12	24	♂ was diſtant from the lucid limb of ☽ 11′ 30″.

Occultation of Mars *by the* Moon, *obſerved at* Pekin *in* China, Dec. 6. 1747. *p. m.*

Fig. 11.

The

The moments of time are true, and corrected by correſponding altitudes.

All the phaſes with a tube of 6 feet, with an *Engliſh* micrometer.

Conjunction of Mars *and* Venus, *obſerved at* Pekin, March 1748.

A ♂, the path of ♂ is near ♀ by the *Pekin* ephemerides.
B ♂, the ſame path by the *Paris* ephem. of *de la Caille.*
C ♂, the ſame path by the obſervations.
D ♂, the ſame path by the *Bononian* eph. of *Manfredi.*

The obſervations were as follows:

Fig. 12.

	h	′	″		h	′	″
Mar. 12.	6	27	52	♂ more E. than ♀	1	48	48
				more N. - -	0	53	46
13.	6	25	43	more E. - -	0	56	8
				more N. - -	0	27	26
14.	6	10	33	more E. - -	0	29	34
				more N. - -	0	14	36
15.	6	28	4	more E. - -	0	3	0
				more N. - -	0	1	30
16.	6	27	13	more W. - -	0	23	48
				more S. - -	0	10	41
17.	6	25	14	more W. - -	0	50	38
				more S. - -	0	23	24
19.	6	29	52	more W. - -	1	44	47
				more S. - -	0	48	14

All theſe differences were determined by repeated operations, with a tube of 6 feet with micrometers. The times alſo are true, and corrected by correſponding altitudes.

— of Jupiter *with* Venus, Jan. 1. 1748. *p. m.*

h	′	″	
5	15	41	♃ was diſtant from ♀ 1° 3′ 49″. Then,
	22	14	♃ to the horary.
	25	6	♀ to the horary more S. than ♃ 50′ 35″
	2	52	Again,
5	26	36	♃ to the horary.
	29	32	♀ to the horary more S. than ♃ 50′ 15″.
	2	56	Thirdly,
5	30	35	♃ to the horary.
	33	34	♀ to the horary more S. than ♃ 49′ 37″.
	2	59	

4. *Apr*

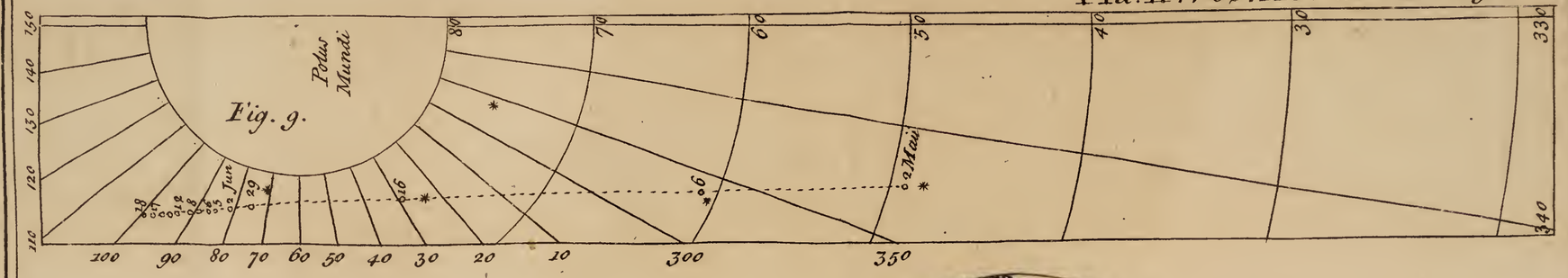


Fig. 9.
Polus Mundi
80
70
60
50
40
30
330
340
150
140
130
120
110
100
90
80
70
60
50
40
30
20
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300
350
2 Mai
6
16
29
2 Jun
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8
12
18
28

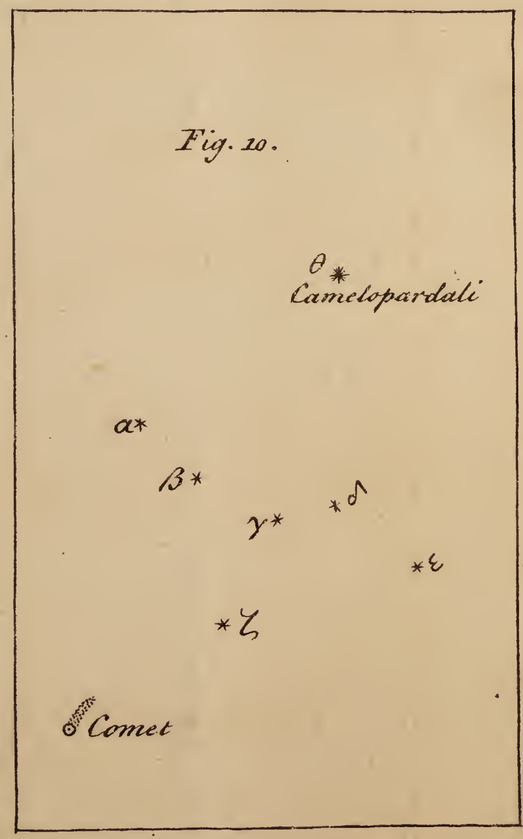
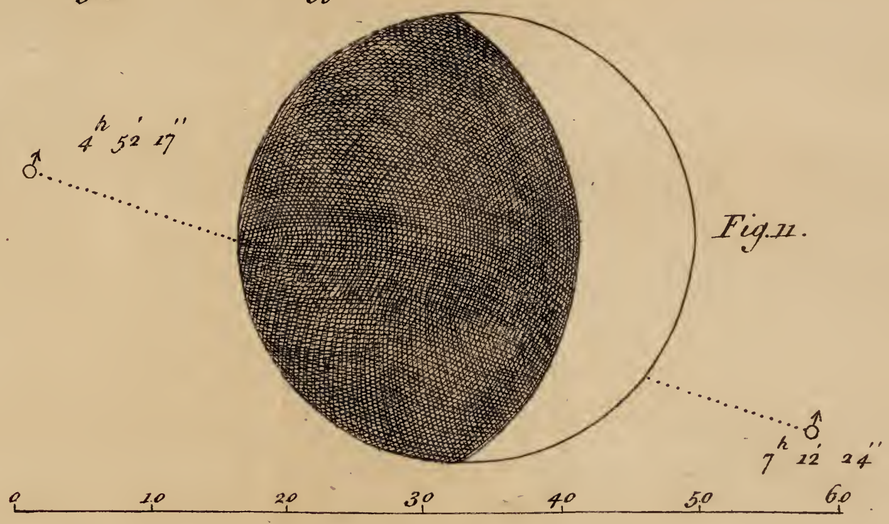
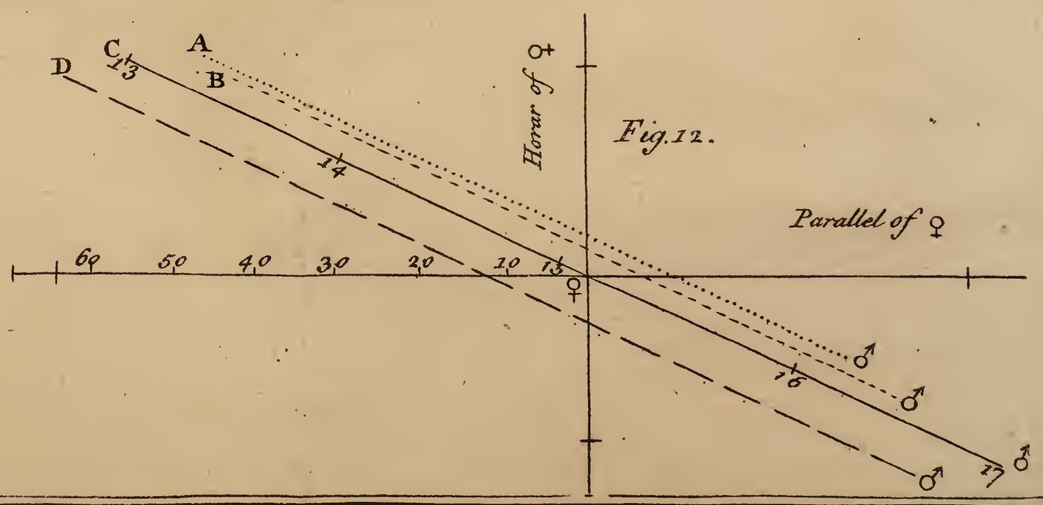
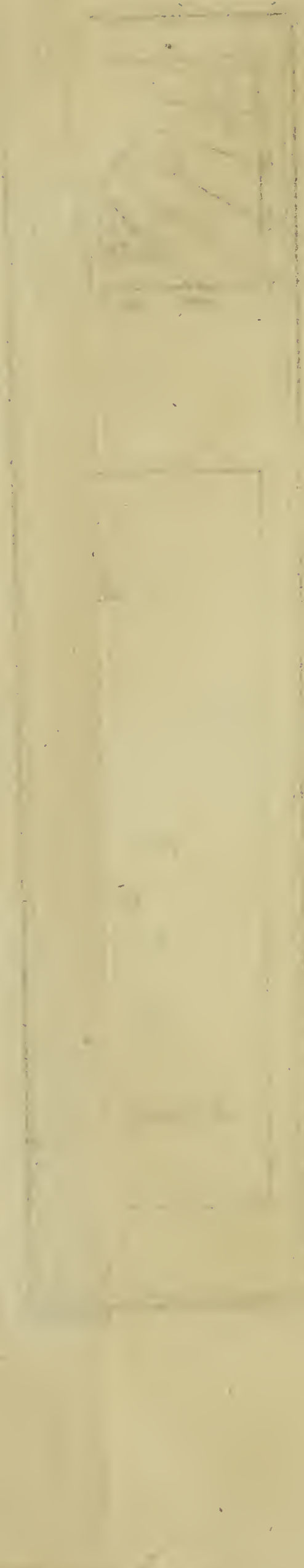


Fig. 10.
θ
Camelopardali
α
β
γ
δ
ε
ζ
Comet


4h 52' 17"
Fig. 11.
7h 12' 24"
0
10
20
30
40
50
60


D
C
A
B
13
14
16
17
Hour of ♀
Fig. 12.
Parallel of ♀
60
50
40
30
20
10
13
♀



4. *Apr.* 27. 3^h 30′ *a. m.* we ſaw the comet in the middle of the ſtars of *Pegaſus* β λ η.

May 2. we compared the comet with the ſtars mentioned by *F. Hallerſtein* in *Flamſted* 1690. The ſtar in ♈ 11° 26′ 45″. The place of the comet was concluded to be almoſt the ſame as by *F. Hallerſtein*'s obſervation.

May 3. 3^h *a. m.* α and σ of *Caſſiopea* in a right line with the comet. σ is pretty exactly in the middle.

May 4. $4^h\frac{1}{4}$ *a. m.* the comet more W. than the 3d ſtar in *Caſſiop.* in *Flamſt.* 5′ 35″, the comet more S. 1° 1′.

May 5. nothing could be obſerved exactly.

May 6. 2^h 51′ *a. m.* a line thro' α and β of *Caſſiop.* a little to the S. of the comet, diſtance of β from the ſtar ϰ = diſt. of β from the comet.

May 10. 9^h 14′ *p. m.* the laſt true alt. of the comet 20° 48′ 58″. The comet more W. than the eaſtern ſtar (it is compounded of two) 27′ 12″; in *Flamſt.* the ſtar in *Taurus* 25° and ſome min.

May 15. we compared the comet with the ſtars of *F. Hallerſtein.* True merid. alt. of the comet *p. m.* 25° 51′ 30″ 10^h 12′. The comet more S. than the ſtar 8′ merid. alt. of the northern ſtar 25° 59′ 30″. We did not well obſerve the diff. of R. aſc.

May 16. *p. m.* true merid. alt. of the comet 26° 16′ 32″; *in reticulo* 10^h 22′: the comet more E. than *Hallerſtein*'s ſtar 1° 41′.

May 17. 10^h 40′ *p. m.* laſt true alt. of the comet 26° 46′ 34″.

γ *Cephei* to the horary 7^h 54′ 58″.

Comet to the horary 10 41 43.

Path of the comet ſeems more N. than the path of the ſtar 38′ 20″.

I do not find any number of obſervations made till *June* 7. But by comparing the comet with the *Hevelian* ſtar, and others not well known to me, I ſeem to be able to conclude, that from *June* 2 to 7 the R. aſc. of the comet increaſed 6° and ſome min. and that the decl. decreaſed 55′.

June 7. *a. m.* 1^h 15′ the ſtar to the horary 35′ 30″ after the comet. The ſtar more N. 1° 30′ very doubtfully obſerved *.

June 9. 0^h 45′ the comet to the horary.

0 49 10″ the ſtar to the horary *A* †.

The comet more N. 1° 30′.

To *June* 12. nothing was obſerved with ſufficient exactneſs.

June 13. *p. m.* 9^h 30′ the diſtance of the comet from the ſtar *I*‖ 10′ 20″.

The comet more N. 4′ 25″.

The comet is more E.

——— *by* F. Anthony Gaubil, *of the* French College *of* Jeſuits *at* Pekin, *to the ſame.* ibid. p. 316. *dated* Pekin, Nov. 8. 1748.

Comet 1748.

* *Hevelius*'s ſtar *ann.* 1660 R. aſc. 2° 24′ 39″ diſt. from the pole 12° 42′ 17″.

† Star *A* in Fig. *D* of la Caille, *Mem. Acad.* 1740.

‖ Star *I* in Fig. *D* of la Caille, *Mem. Acad.* 1742.

	h	′	″		
June 17. *p. m.*	0	26	30	*A*	} to the horary *.
		53	35	*R*	
		55	15	Comet	

Comet and *R* in the ſame declination.

	h	′	″		
June 18. *p. m.*	9	52	14	*A*	} to the horary.
	10	19	21	*R*	
		24	57	Comet	

Comet more S. than *A* 16′ 30″.
R 10 20.

On the following days with a tube of above 7 feet, the aperture of which = 1° 0′ 24″ the comet was obſerved with the ſtar *D* in Fig. *B* of *la Caille*, *Mem. Acad.* 1742. Many of theſe obſervations had hardly any ſucceſs: I relate only two which ſeem to me not very exact.

		h	′	″
June 27. *p. m.*	the ſtar *D* enters the tube - - - - -	9	23	10
	goes out of the tube - - -	9	36	0
	Comet enters - - - - - - - -	9	40	28
	goes out - - - - - - -	9	52	16

The comet is concluded more N. 19′ 40″.

		h	′	″	
June 29.	*D* enters - - - - - - - - - -	11	1	40	*p. m.*
	goes out - - - - - - - - -	11	14	51	
	Comet enters - - - - - - - - -	11	23	54	
	goes out - - - - - - - - -	11	36	0	

The comet is concluded more N. 12′ or perhaps 13′.

The comet was not eaſily ſeen *June* 29, there were clouds on *June* 30, and *July* 1 and 2; and it was not afterwards purſued.

Conjunction of Mars *and* Venus.

1748 *t. v. Mar.* 15. *p. m.* 8ʰ 10′ the occidental limb of ♂ is diſtant from the occidental limb of ♀ 1′ 29″.

Eclipſes of the Satellites *of* Jupiter.

			h	′	″	
Oct. 13.	p. m.	*temp. ver.*	9	40	30	Emerſion of the 3d.
15.	- - - -		8	37	26	- - - - - - 1ſt.
20.	- - - -		10	7	56	Total immerſion of 3d.
21.	- - - -		5	52	12	Emerſion of the 2d doubtful.
28.	- - - -		8	29	20	1ſt Emerſion of the 2d.
Nov. 7.	- - - -		8	52	59	1ſt Emerſion of the 1ſt.

Theſe Obſervations were made with a tube of 15 feet.

* *A R* notes of ſtars in Fig. *D* of la Caille, *Mem. Acad.* 1742.

XXI. The

XXI. The mean *tropical ſolar year*, or that mean ſpace of time wherein the ſun, or earth, after departing from any point of the ecliptic, returns to the ſame again, conſiſts, according to Dr *Halley*'s tables, of 365^d, 5^h, $48'$, $55''$: which is leſs by $11'$, $5''$, than the mean *Julian* year, conſiſting of 365^d, 6^h, $0'$, $0''$. Hence the equinoxes and ſolſtices anticipate, or come earlier than the *Julian* account ſuppoſes them to do, by $11'$, $5''$, in each mean *Julian* year; or $44'$, $20''$ in every 4; or 3^d, 1^h, $53'$, $20''$, in every 400 *Julian* years.

In order to correct this error in the *Julian* year, the authors of the *Gregorian* method of regulating the year, when they reformed the calendar in the beginning of *Oct.* 1582, directed that 3 intercalary days ſhould be omitted or dropped in every 400 years; by reckoning all thoſe years, whoſe date conſiſts of a number of entire hundreds not diviſible by 4, ſuch as 1700, 1800, 1900, 2100, *&c.* to be only common, and not biſſextile or leap years, as they would otherwiſe have been; and conſequently omitting the intercalary days, which, according to the *Julian* account, ſhould have been inſerted in *Feb.* in thoſe years. But at the ſame time they ordered that every fourth hundredth year, conſiſting of a number of entire hundreds, diviſible by 4, ſuch as 1600, 2000, 2400, 2800, *&c.* ſhould ſtill be conſidered as biſſextile or leap years, and, of conſequence, that 1 day ſhould be intercalated as uſual in thoſe years.

This correction, however, did not entirely remove the error: for the equinoxes and ſolſtices ſtill anticipate 1^h, $53'$, $20''$ in every 400 *Gregorian* years. But that difference is ſo inconſiderable as not to amount to 24 hours, or to one whole day, in leſs than 5082 *Gregorian* years.

The ſpace of time betwixt one mean conjunction of the moon with the ſun and the next following, or a mean *ſynodical month*, is equal to 29^d, 12^h, $44'$, $3''$, $2'''$, 56^{vi}, according to Mr *Pound*'s tables of mean conjunctions. The common lunar year conſiſts of 12 ſuch months. The intercalary or *embolimæan* year conſiſts of 13 ſuch months. In each cycle of 19 lunar years, there are 12 common, and 7 intercalary or *embolimæan* years, making together 235 ſynodical months.

It was thought, at the time of the General Council of *Nice*, which was holden in 325, that 19 *Julian* ſolar years were exactly equal to ſuch a cycle of 19 lunar years, or to 235 ſynodical months; and therefore, that, at the end of 19 years, the new moons or conjunctions would happen exactly at the ſame times, as they did 19 years before: and upon this ſuppoſition it was, that, ſome time afterwards, the ſeveral numbers of that cycle, commonly called the golden numbers, were prefixed to all thoſe days in the calendar, on which the new moons then happened in the reſpective years correſponding to thoſe numbers; it being imagined, that whenſoever any of thoſe numbers ſhould for the future, be the golden number of the year, the new moons would invariably happen on thoſe days in the ſeveral months, to which that number was prefixed.

But this was a miſtake:

Remarks upon the Solar *and the* Lunar *years, the* Cycle *of* 19 *years, commonly called the* Golden Number, *the* Epact, *and a method of finding the time of* Eaſter, *as it is now obſerved in moſt parts of* Europe. *Being part of a letter from the R. Hon.* Geo. *E. of* Macclesfield *to* M. Folkes, *Eſq; P. R. S.* № 495. p. 417. *Read* May 10. 1750.

Of the Solar Year.

Of the Lunar Year, Cycle of 19 Years, *and the* Epact.

	d	h	′	″	‴
For 19 *Julian* ſolar years contain - - - - -	6939	18	0	0	0
Whereas 235 ſynodical months contain only - -	6939	16	31	56	30
And are therefore leſs than 19 *Julian* ſolar years by	0	1	28	3	30

This difference amounts to a whole day very nearly in 310.7 years, the new moons anticipating, or falling earlier, by 24 hours in that ſpace of time, than they did before: and therefore now in 1750, the new moons happen above $4\frac{1}{2}$ days ſooner, than the times pointed out by the golden numbers in the calendar.

In order therefore to preſerve a ſort of regular correſpondence betwixt the ſolar and the lunar years, and to make the golden numbers, prefixed to the days of the month, uſeful for determining the times of the new moons, it would be neceſſary when once thoſe golden numbers ſhould have been prefixed to the proper days, to make them anticipate a day at the end of every 310.7 years, as the moons will actually have done; that is, to ſet them back one day, by prefixing each of them to the day preceding that, againſt which they before ſtood.

But as ſuch a rule would neither be ſo eaſily comprehended or retained in memory, as if the alteration was to be made at the end or at the begining of complete centuries of years; the rule would be much more fit for practice, and keep ſufficiently near to the truth, if thoſe numbers ſhould be ſet back 9 days in the ſpace of 2800 years; by ſetting them back 1 day, firſt at the end of 400 years, and then at the end of every 300 years for eight times ſucceſſively: whereby they would be ſet back, in the whole, 9 days in 2800 years. After which they muſt again be ſet one day back at the end of 400 years, and ſo on, as in the preceding 2800 years. By which means the golden numbers would always point out the mean times of the new moons, within a day of the truth.

It is plain however, that the lunar year will have loſt one day more than ordinary, with reſpect to the ſolar year, whenever the new moons ſhall have anticipated a whole day, as they will have done at thoſe times when it is neceſſary that the golden numbers ſhould, by the rule juſt now given, be ſet back one day: and conſequently the epact, for that and the ſucceeding years, muſt exceed by an unit the ſeveral correſponding epacts of the preceding 19 years.

For the epact is the difference, in whole days, betwixt the common *Julian* ſolar and the lunar year; the former being reckoned to conſiſt of 365, and the latter of only 354 days. If therefore the ſolar and the lunar year at any time ſhould commence on the ſame day, the ſolar would, at the end of the year, have exceeded the lunar by 11 days: which number 11 would be the epact of the next year: 22 would be the epact of the year following, and 33 the epact of the year after that, the epacts increaſing yearly by 11. But as often as this yearly addition makes the epact exceed 30, thoſe 30 are rejected as making an intercalary month, and only

only the exceſs of the epact above 30 is accounted the true epact for that year. Thus when the epact would amount to 31, 32, 33, 34, *&c.* the 30 is rejected, and the epact becomes 1, 2, 3, 4, *&c.*

Since therefore the lunar year will have loſt a day more than ordinary in reſpect of the ſolar year, whenever it is neceſſary to ſet the golden numbers one day back, as was before obſerved; it follows, that the epact muſt at the ſame time be increaſed by an unit more than uſual; the difference betwixt the ſolar and the lunar year having been juſt ſo much greater than uſual. That is, 12 muſt be added, inſtead of 11, to the epact of the preceding, in order to form what will be the epact of the then preſent year. Which addition of an unit extraordinary to one epact, will occaſion all the ſubſequent epacts (which will follow each other in the uſual manner, each exceeding the foregoing by 11) to be greater by an unit than their reſpectively correſponding epacts of the preceeding 19 years.

If therefore, inſtead of the golden numbers, the epacts of the ſeveral years were prefixed, in the manner the *Gregorians* have done, to the days of the calendar, in order to denote the days on which the new moons fall in thoſe years whereof thoſe numbers are the epacts; there would never be occaſion to ſhift the places of thoſe epacts in the calendar; ſince the augmentation by an unit extraordinary of the epacts themſelves would anſwer the purpoſe, and keep all tolerably right. Thus in a very eaſy method may the courſe of the new moons be pointed out, either by the golden numbers, or by the epacts, according to the *Julian* account or manner of adjuſting the year, which goes on regular and uniform without any variation.

But the regulating theſe things for thoſe who uſe the *Gregorian* account, is an affair of more intricacy; and for them it will require more conſideration to determine, when the epacts are to be more than ordinarily augmented, and at what times they are to continue in their uſual courſe; nay, to know when they are not only not to be extraordinarily augmented, but alſo when they are to be diminiſhed by an unit, by increaſing one of them by 10 only inſtead of 11 as uſual: and this happens much oftener with the *Gregorians*, than the increaſing one of them by 12 inſtead of 11. For, in every *Gregorian* ſolar year, whoſe date conſiſts of any number of entire hundreds not diviſible by 4, it is ſuppoſed that the equinox has anticipated one whole day; and therefore one day, that which ought to be the intercalary one, is omitted; and conſequently the preceding ſolar year, where one day was loſt, exceeded the lunar year by 10 days only inſtead of 11.

In order therefore to adapt the beforemention'd rule to the *Gregorian* account, and to know in what years the epacts ſhould either be extraordinarily augmented or diminiſhed, and the golden numbers ſhould either be ſet backwards or forwards in the calendar; the following rules and directions muſt be obſerved.

Firſt. That in the years 1800, 2100, 2700, 3000, *&c.* where the number of entire hundreds is diviſible by 3, but not by 4, the *Gregorian* ſolar, as well as the lunar year, will have loſt a day; and conſequently the

the difference betwixt them will be the ſame as uſual: therefore in thoſe years there muſt be no alteration, either in the epacts or the golden numbers; but the former muſt go on in the ſame manner, and the latter ſtand prefixed to the ſame days in the calendar, for another, as they did for the laſt hundred years.

2dly. The like will happen in the years 2000, 2800, 3200, *&c.* where the number of entire hundreds is diviſible by 4, but not by 3: For neither the *Gregorian* ſolar nor the lunar year is to be altered; and therefore the epacts muſt go on, and the golden numbers ſtand, as they did before.

But 3dly, In the years 2400, and 3600, whoſe number of entire hundreds is diviſible both by 3 and 4, the *Gregorian* ſolar year goes on as uſual, and the lunar year has loſt a day. The difference therefore betwixt them being 12, the epact of the preceding year muſt be augmented by that number inſtead of 11, in order to form the epact of the then preſent year; whereby a new ſet of epacts will be introduced, exceeding their precedent correſponding epacts by an unit: and the golden numbers muſt be ſet one day back in the calendar.

4thly and laſtly, In the years 1900, 2200, 2300, 2500, *&c.* where the number of hundreds is diviſible neither by 3 nor 4; the *Gregorian* ſolar year having loſt one day, and the lunar none, the difference betwixt them being only 10; that number only, and not 11 is to be added to the epact of the preceding, in order to form the epact of that, the then preſent year; whereby a new ſet of epacts will be introduced, all of them leſs by an unit than their precedent correſponding epacts: and the golden numbers muſt be ſet a day forwarder in the calendar; that is, be prefixed to the day following that, againſt which they ſtood in the precedent hundred years.

This method would preſerve a ſort of regularity betwixt the ſolar and the lunar years; and, by means of the rules and directions beforementioned, the days of the new moons might be pointed out, either by the golden numbers or by the epacts, placed in the calendar for that purpoſe; according to the *Julian* account for ever, and according to the *Gregorian* account till the year 4199 incluſive, after which there muſt be ſome little variation made in the four laſt precepts or rules: but it would be to little purpoſe now, to attempt the framing of new ſet of rules for ſo diſtant a time.

The *Gregorians* have choſen to make uſe of the epacts to determine the days of the new moons, and follow pretty nearly the rules preſcribed above; except that they order the epacts to have an additional augmentation of an unit 8 times in 2500 years, beginning with the year 1800, as at the end of 400 years; to which 400 years, if there be added 3×700, or 2100 years, the period of 2500 years will be completed in 3900. After which they do not make their extraordinary augmentation of an unit in the epacts, till at the end of another term of 400 years; which defers that augmentation from the year 4200 to the year 4300. And this

this is the reaſon that the rules above deliver'd will require a variation in the year 4200; whereas it is directed in this paper that the epacts ſhould be augmented, or (which is the ſame thing) the golden numbers be ſet back in the calendar 9 times in 2800 years. This ariſes from the *Gregorians* ſuppoſing, that the difference betwixt 19 ſolar and as many lunar years would not amount to a whole day in leſs than 312 ½ years; whereas it has appeared above, that it would amount to a whole day in 310.7 years. But although the rule preſcribed in this paper comes much nearer to the truth, yet the error in either caſe is very inconſiderable, being ſo ſmall as not to amount to a whole day in many thouſand years; and therefore is not worth regarding.

A method of finding the time of Eaſter, *as it is obſerved in moſt parts of* Europe.

From what has been already ſaid, a method may be obtained, for fixing with ſufficient exactneſs, the time of the celebration of the feaſt of *Eaſter*, which is governed by the *vernal equinox*, and by the age of the moon neareſt to it. The former whereof, when once rightly adjuſted, may (by the corrections mentioned in that part of this paper which relates to the ſolar year) be made to continue to fall at very near the ſame time with, or at moſt not to differ a whole day from the true *equinox*: and the ſame rules and directions, which, as was before ſhewn, would, without any great error, point out the times of the firſt day of the moon, would with equal certainty point out the 14th, 15th, or any other: and thus the times of the oppoſitions or the full moons might be as well marked out thereby, as thoſe of the conjunctions or the new moons.

I ſhall not at preſent take notice of the canon of the Council of *Nice*, in 325, which directs the time of celebrating *Eaſter*: or of the reaſons upon which that canon was founded. Nor ſhall I endeavour to explain the rule now in uſe in the Church of *England* for finding *Eaſter*: for, beſides that ſuch an explanation would extend this paper to an improper length, thoſe points have already been treated of by ſeveral much abler hands, and particularly by our countryman the learned Dr *Prideaux*. Nor is it my intention to enter far into the methods uſed by the *Gregorians*, or thoſe of the Church of *Rome*, or by any other nations or countries, for finding the time of that feaſt. As to our own, I ſhall only obſerve, that the method now uſed in *England*, for finding the 14th day of the moon, or the eccleſiaſtical full moon, on which *Eaſter* dependeth, is, by proceſs of time, become conſiderably erroneous: as the golden numbers, which were placed in the calendar, to point out the days on which the new moons fall in thoſe years of which they are reſpectively the golden numbers, do now ſtand ſeveral days later in the ſame than thoſe new moons do really happen. Which error, as was before obſerved, ariſes from the anticipation of the moons ſince the time of the Council of *Nice*: and as the *vernal equinox* has alſo anticipated 11 days ſince that time; neither that equinox, nor the new moons, do now happen on thoſe days upon which the Church of *England* ſuppoſes them ſo to happen.

When Pope *Gregory* XIII. reformed the *Julian* ſolar year, he likewiſe made a correction as to the time of celebrating the feaſt of *Eaſter*, by placing

placing the epacts (which he directed to be made uſe of for the future inſtead of the golden numbers) much nearer to the true times of the new moons than the golden numbers then ſtood in the old calendar: I ſay, *much nearer to the true times*; becauſe in fact the epacts, as placed by him, were not prefixed to the exact days upon which the new moons then truly fell. And this was done with deſign, and for a reaſon which it is not material to the purpoſe of this paper to mention.

But the Church of *England*, and that of *Rome* or the *Gregorians*, do ſtill agree in this; that both of them mark (the former by the golden numbers, and the latter by the epacts correſponding to them) the days on which their eccleſiaſtical new moons are ſuppoſed to happen: and that 14th day of the moon incluſive, or that full moon, which falls upon, or next after, the 21ſt of *March*, is the paſchal limit or full moon to both: and the *Sunday* next following that limit or full moon, is by both Churches celebrated as *Eaſter* day. But the 21ſt of *March* being reckoned, according to the *Gregorian* account or the new ſtyle, 11 days ſooner than by the *Julian* account or the old ſtyle, which is ſtill in uſe among us; and their eccleſiaſtical new moons being 3 days earlier than thoſe of the Church of *England*; it happens that although the Church of *England* and that of *Rome* often do, yet more frequently they do not, celebrate the feaſt of *Eaſter* upon the ſame natural day.

It might however be eaſier for both, and could occaſion no inconvenience, now that Almanacks, which tell the exact times of the new moons, are in moſt peoples hands; if all the golden numbers and epacts now prefixed to thoſe days of the calendar, in our book of Common Prayer, and in the *Roman Breviary*, on which the reſpective eccleſiaſtical new moons happen, were omitted in the places where they now ſtand; and were ſet only againſt thoſe 14th days of the moon, or thoſe full moons, which happen betwixt the 21ſt of *March* and the 18th of *April*, both incluſive. Since no 14th day or full moon, which happens before the 21ſt of *March*, or after the 18th day of *April*, can have any ſhare in fixing the time of *Eaſter*. By which means the trouble of counting to the 14th day, and the miſtakes which ſometimes ariſe therefrom, would be avoided.

We do as yet in *England* follow the *Julian* account or the Old Style in the civil year; as alſo the old method of finding thoſe moons upon which *Eaſter* depends: both of which have been ſhewn to be very erroneous.

If therefore this nation ſhould ever judge it proper to correct the civil year, and to make it conformable to that of the *Gregorians*, it would ſurely be adviſable to correct the time of the celebration of the feaſt of *Eaſter* likewiſe, and to bring it to the ſame day upon which it is kept and ſolemnized by the inhabitants of the greateſt part of *Europe*, that is, by thoſe who follow the *Gregorian* account. For tho' I am aware that their method of finding the time of *Eaſter* is not quite exact, but is liable to ſome errors; yet I apprehend, that all other practicable methods of doing it would be ſo: and if they were more free from error, they would probably be more intricate,

and

and harder to be underſtood by numbers of people, than the method of determining that feaſt either by a cycle of epacts, as is practiſed by the *Gregorians*, or by that of 19 years or the golden numbers, in the manner propoſed in the following part of this paper: and it is of no ſmall importance, that a matter of ſo general a concern, as the method of finding *Eaſter* is, ſhould be within the reach of the generality of mankind, at leaſt as far as the nature of the thing will admit.

For which reaſon, in caſe the legiſlature of this country ſhould before the year 1900, think fit to make our civil year correſpond with that of the *Gregorians*, and alſo to celebrate all the future feaſts of *Eaſter* upon the ſame days upon which they celebrate them; this laſt particular might be eaſily effected, without altering the rule of the Church of *England* for the finding of that feaſt: and this only by advancing the golden numbers, prefixed to certain days in the calendar, 8 days forwarder for the new moons, or 21 days forwarder for the 14th days or full moons, than they now ſtand in our calendar.

In order to explain this, it muſt be obſerved, that the *Gregorian* account, or the new ſtyle, is 11 days forwarder than the *Julian* account, or the old ſtyle, which we ſtill make uſe of; that is, the laſt day of any of our months is the 11th day of their next ſucceeding month. If therefore their eccleſiaſtical new moons fell on the ſame days with thoſe of the Church of *England*, the golden number 14, which now ſtands againſt the laſt day of *February* in our, that is the *Julian*, calendar, ſhould, when we ſhould have adopted the *Gregorian* calendar, be prefixed to the 11th of *March*. But ſince their eccleſiaſtical new moons happen 3 days, earlier than our eccleſiaſtical new moons at preſent do; ſo much ſhould be deducted from thoſe 11 days, by which the golden numbers ought otherwiſe to be advanced; and the golden number 14 ſhould not be placed againſt the 11th, but the 8th of *March*: which being reckoned the firſt day of the moon, if we count on to the 14th day of the ſame inclusive, that would be found to fall on the 21ſt of *March*; on which day the *Gregorian* paſchal limit or full moon will happen when the golden number is 14. And the like courſe ſhould be taken with the reſt of the 19 golden numbers; which ought to be placed 8 days forwarder than they now ſtand, if they are to point out the new moon; or 21 days forwarder than they are at preſent, if they are to mark the 14th day of the moon or the full moon: the latter of which, as has been ſhewn, would be more eligible, than to prefix thoſe numbers to the days on which the new moons happen.

Thus may the rule and method now uſed in the Church of *England*, be moſt eaſily adapted to ſhew the time of *Eaſter*, as it is obſerved by the *Gregorians*, till the year 1900, at which time, and at the other proper ſucceeding times, if the golden numbers in the calendar ſhall either be advanced or ſet backward a day, according to the foregoing rules and directions for that purpoſe, they will continue to ſhew us the new or the full moons of the Church of *Rome* or the *Gregorian* calendar with great exact-

neſs, till the year 4199: when, as has been already mentioned, there muſt be a little variation made in thoſe rules and directions.

There is however one exception to thoſe general rules and directions, which will be taken notice of in the next paragraph.

Upon theſe principles I framed the table accompanying this paper, and ſhewing, by means of the golden numbers, all the *Gregorian* paſchal limits or full moons, from the reformation of the calendar, *&c.* by Pope *Gregory* to the year 4199 incluſive. Which ſpace of time is therein divided into 16 unequal portions or periods; at the beginning of each of which, all the golden numbers, when once they ſhall have been properly placed in the calendar, muſt either be advanced or ſet back one day, with reſpect to the place where they ſtood in the preceding period, agreeably to the foregoing rules: except thoſe numbers which ſhall happen to ſtand againſt the 4th and 5th of *April* to ſhew the paſchal new moons, or againſt the 17th and 18th of the ſame month to mark out the paſchal full moons; both which numbers at ſome times, and only one of them at others, muſt keep the ſame place for that, which was allotted to them in the immediately preceding period.

In order to determine at what times, and on what occaſions, this exception is to take place; let it be obſerved, that, in the months of *Jan. Mar. May*, and ſome others in our preſent calendar, as well as in the table above-mentioned, ſome of the golden numbers ſtand double or in pairs, and follow one the other immediately; whilſt others, on the contrary, generally ſtand ſingle and by themſelves.

Now, when any of thoſe pairs, or 2 numbers which uſually accompany each other, happen, in purſuance of the foregoing rules, to be prefixed the one to the 4th and the other to the 5th of *April* for the new moons, or the one to the 17th and the other to the 18th of *April* for the paſchal limits or full moons: and when any of thoſe numbers, which generally ſtand ſingle, are prefixed, according to the ſaid rules, to the 5th of *April* for the new moons, or to the 18th for the full moons: in theſe caſes thoſe pairs or ſingle numbers that are ſo ſituated, muſt not be ſet forward or advanced at the beginning of the next period, but muſt keep their places during another period, if the foregoing rules direct all the golden numbers to be advanced a day; which muſt be complied with in reſpect to all the other golden numbers, except thoſe ſo ſituated as above. Inſtances whereof may be ſeen in the table, under the reſpective periods beginning with the years 1900, 2600, 3100, and 3800.

But if, in conformity to the foregoing rules, all the golden numbers are to be ſet one day backward; thoſe pairs or ſingle numbers, tho' ſituated as is above-mentioned, muſt not keep their places, but muſt move one day backward like all the other golden numbers; as they may be ſeen to do in the periods beginning with the years 2400 and 3600.

To give a plain and intelligible account of the reaſon, on which the directions now given with reſpect to this exception are founded, would extend this paper, already too long, far beyond its due and proper bounds.

bounds. I ſhall therefore content myſelf with obſerving, that it depends chiefly upon the nature of the *Menſes Pleni* and *Menſes Cavi*, into which the lunar year is uſually divided: and that, in order to make uſe of the golden numbers for finding the time of the *Gregorian Eaſter*, it will be neceſſary not only to conform to the general rules laid down in the former part of this paper; but alſo to follow the directions juſt now given, with reſpect to the abovementioned exception to thoſe general rules.

But I ſhould not do juſtice to *Peter Davall*, of the *Middle Temple* Eſq; Secretary of the *Royal Society*, did I not here acknowledge, that, before I had ſo fully conſidered theſe matters as I have ſince done, I had the firſt hint of applying the golden numbers to find the *Gregorian* paſchal limit or full moon, from him; who has ſince that time compoſed and drawn up tables, *&c.* which may poſſibly be of conſiderable and general uſe in this nation hereafter.

A TABLE, ſhewing, by means of the Golden Numbers, the ſeveral days on which t Paſchal Limits or Full Moons, according to the *Gregorian* Account, have already happen or will hereafter happen; from the Reformation of the Calendar in the Year of our Lo 1582, to the Year 4199 incluſive.

To find the Day on which the Paſchal Limit or Full Moon falls in any given Year; Look, in the Columr Golden Numbers belonging to that Period of Time wherein the given Year is contained, for the Golden Nu ber of that Year; over-againſt which, in the ſame Line continued to the Column intitled *Paſchal Full Mo* you will find the Day of the Month, on which the Paſchal Limit or Full Moon happens in that Year: And *Sunday* next after that Day is *Eaſter* Day in that Year, according to the *Gregorian* Account.

Golden Numbers from the Year 1583 to 1699, and ſo on to 4199, all incluſive.															Paſchal Full Moon
1583 to 1699	1700 to 1899	1900 to 2199	2200 to 2299	2300 to 2399	2400 to 2499	2500 to 2599	2600 to 2899	2900 to 3099	3100 to 3399	3400 to 3499	3500 to 3599	3600 to 3699	3700 to 3799	3800 to 4099	4100 to 4199	Days of t Month, a Sunday Le
3	14	—	6	17	6	17	—	9	—	1	12	1	12	—	4	March 21
—	3	14	—	6	—	6	17	—	9	—	1	—	1	12	—	— 22
11	—	3	14	—	14	—	6	17	—	9	—	9	—	1	12	— 23
—	11	—	3	14	3	14	—	6	17	—	9	—	9	—	1	— 24
19	—	11	—	3	—	3	14	—	6	17	—	17	—	9	—	— 25
8	19	—	11	—	11	—	3	14	—	6	17	6	17	—	9	— 26
—	8	19	—	11	—	11	—	3	14	—	6	—	6	17	—	— 27
16	—	8	19	—	19	—	11	—	3	14	—	14	—	6	17	— 28
5	16	—	8	19	8	19	—	11	—	3	14	3	14	—	6	— 29
—	5	16	—	8	—	8	19	—	11	—	3	—	3	14	—	— 30
13	—	5	16	—	16	—	8	19	—	11	—	11	—	3	14	— 31
2	13	—	5	16	5	16	—	8	19	—	11	—	11	—	3	April 1
—	2	13	—	5	—	5	16	—	8	19	—	19	—	11	—	— 2
10	—	2	13	—	13	—	5	16	—	8	19	8	19	—	11	— 3
—	10	—	2	13	2	13	—	5	16	—	8	—	8	19	—	— 4
18	—	10	—	2	—	2	13	—	5	16	—	16	—	8	19	— 5
7	18	—	10	—	10	—	2	13	—	5	16	5	16	—	8	— 6
—	7	18	—	10	—	10	—	2	13	—	5	—	5	16	—	— 7
15	—	7	18	—	18	—	10	—	2	13	—	13	—	5	16	— 8
4	15	—	7	18	7	18	—	10	—	2	13	2	13	—	5	— 9
—	4	15	—	7	—	7	18	—	10	—	2	—	2	13	—	— 10
12	—	4	15	—	15	—	7	18	—	10	—	10	—	2	13	— 11
1	12	—	4	15	4	15	—	7	18	—	10	—	10	—	2	— 12
—	1	12	—	4	—	4	15	—	7	18	—	18	—	10	—	— 13
9	—	1	12	—	12	—	4	15	—	7	18	7	18	—	10	— 14
—	9	—	1	12	1	12	—	4	15	—	7	—	7	18	—	— 15
17	—	9	—	1	—	1	12	—	4	15	—	15	—	7	18	— 16
6	17	17	9	—	9	—	1	12	12	4	15	4	15	15	7	— 17
14	6	6	17	9	17	9	9	1	1	12	4	12	4	4	15	— 18
																— 19
																— 20
																— 21
																— 22
																— 23
																— 24

X

XXII. Monſieur *le Monnier* writes to me, that there is, at *Leyden*, an *Arabick* manuſcript of *Ibn jounis* (if I am not miſtaken in the name, for it is not diſtinctly written in the letter), which contains a hiſtory of aſtronomical obſervations. M. *le Monnier* ſays, that he inſiſted ſtrongly on publiſhing a good tranſlation of that book. And as ſuch a work would contribute much to the improvement of Aſtronomy, I ſhould be glad to ſee it publiſhed. I am very impatient to ſee ſuch a work which contains obſervations, that are not ſo old as thoſe recorded by *Ptolemy*. For having carefully examined the modern obſervations of the ſun with thoſe of ſome centuries paſt, although I have not gone farther back than the 15th century, in which I have found *Walther*'s obſervations made at *Nuremberg*; yet I have obſerved that the motion of the ſun (or of the earth) is ſenſibly accelerated ſince that time; ſo that the years are ſhorter at preſent than formerly: the reaſon of which is very natural; for if the earth, in it's motion, ſuffers ſome little reſiſtance (which cannot be doubted, ſince the ſpace through which the planets move, is neceſſarily full of ſome ſubtile matter, were it no other than that of light) the effect of this reſiſtance will gradually bring the planets nearer and nearer the ſun; and as their orbits thereby become leſs, their periodical times will alſo be diminiſhed. Thus in time the earth ought to come within the region of *Venus*, and in fine into that of *Mercury*, where it would neceſſarily be burnt. Hence it is manifeſt, that the ſyſtem of the planets cannot laſt for ever in it's (preſent) ſtate. It alſo inconteſtably follows, that this ſyſtem muſt have had a beginning: for whoever denies it, muſt grant me, that there was a time, when the earth was at the diſtance of *Saturn*, and even farther; and conſequently that no living creature could ſubſiſt there. Nay there muſt have been a time, when the planets were nearer to ſome fixt ſtars than to the ſun; and in this caſe they could never come into the ſolar ſyſtem. This then is a proof, purely phyſical, that the world, in it's preſent ſtate, muſt have had a beginning, and muſt have an end. In order to improve this notion, and to find with exactitude, how much the years become ſhorter in each century; I am in hopes that a great number of older obſervations will afford me the neceſſary ſuccours.

Part of a letter from Leonard Euler, *Prof. Math. at* Berlin, *and F. R. S. To the Rev. Mr* Caſpar Wetſtein, *Chaplain to the Prince of* Wales, *concerning the gradual approach of the* Earth *to the* Sun. *Tranſlated from the* French, *by* T. S. *M. D. F. R. S.* N° 493. p. 203. *dated* Berlin, June 28. 1749. *Read* Nov, 2. 1749.

XXIII. I am ſtill thoroughly convinced of the truth of what I advanced *, that the orbs of the planets continue to be contracted, and conſequently their periodical times grow ſhorter. But in order to put this fact out of doubt, we ought to be furniſhed with good ancient obſervations, and alſo to be very ſure of the time elapſed, ſince thoſe obſervations, to this day: which we are not, with regard to the obſervations that *Ptolemy* has left us. For Chronologiſts, in fixing the moments of thoſe obſervations, run into a miſtake, by ſuppoſing the ſun's mean motion to be known; which ought rather itſelf to be determined by theſe ſame obſervations. Now, if we reduce the days marked by *Ptolemy* to

Part of a letter from Mr Profeſſor Euler *to the Rev. Mr* Wetſtein, *Chaplain to the Prince, concerning the contraction of the* Orbits *of the* Planets. *Tranſlated from the* French *by*

* In the preceding paper.

the

T. S. *M. D. and F. R. S.* N° 494. p. 356. *dated* Berlin, Dec. 20. 1749. *Read* March 1. 1749.

the *Julian* calendar, we run the riſque of committing an error of a day or two, in the whole number of days elapſed, from that to our time; becauſe the courſe of the *Julian* years, according to which every 4th ought to have been *biſſextile*, has been frequently interrupted by the *Pontifices*; of which we find ſome ſure marks in *Cenſorinus* and *Dion Caſſius*. Wherefore it might well happen, ſince the times marked by *Ptolemy*, that there has really been a day or 2 more than we reckon, and conſequently, that *Ptolemy*'s equinoxes, ought to be put a day or 2 back; which would lengthen the years of thoſe times. I was in hopes, that the *Arabian* obſervations would not be liable to this inconvenience; becauſe the *Julian* calendar has not been interrupted for theſe laſt 1200 years. The late Dr *Halley* had alſo remarked, that the revolutions of the moon are quicker at preſent than they were in the time of the ancient *Chaldeans*, who have left us ſome obſervations of eclipſes. But as we meaſure the length of years by the number of days and parts of a day, which are contained in each of them; it is a new queſtion, whether the days, or the revolutions of the earth round it's axis, have always been of the ſame length. This is unanimouſly ſuppoſed, without our being able to produce the leaſt proof of it: nor indeed do I ſee, how it could be poſſible to perceive ſuch an inequality, in caſe it had really exiſted. At preſent we meaſure the duration of a day by the number of oſcillations, which a pendulum of a given length makes in this ſpace of time: but the Ancients were not acquainted with theſe experiments, whereby we might have been informed, whether a pendulum of the ſame length made as many vibrations in a day formerly as now. But even though the Ancients had actually made ſuch experiments, we could draw no inferences from them, without ſuppoſing, that gravity, on which the time of an oſcillation depends, has always been of the ſame force: but who will ever be in a condition to prove this invariability in gravity? Thus, even ſuppoſing that the days had ſuffered conſiderable changes; and that gravity had been altered ſuitably thereto, ſo that the ſame pendulum had always completed the ſame number of vibrations in a day; it would nevertheleſs be ſtill impoſſible for us to perceive this inequality, were it ever ſo great. And yet I have ſome reaſons, deduced from *Jupiter*'s action on the earth, to think, that the earth's revolution round it's axis continually becomes more and more rapid. For the force of *Jupiter* ſo accelerates the earth's motion in it's orbit round the ſun, that the diminution of the years would be too ſenſible, if the diurnal motion had not been accelerated nearly in the ſame proportion. Wherefore, ſince we hardly at all remark this conſiderable diminution in the years, from thence I conclude, that the days ſuffer much the ſame diminution; ſo that the ſame number will anſwer nearly to a year.

A new method of making a Mural Quadrant, *which*

XXIV. The great uſefulneſs of arches, firmly fixed to walls in the plane of the meridian, is well known to all who are the leaſt acquainted with aſtronomical ſtudies. Hence it comes to paſs, that few obſervatories

ries are thought to be well furniſhed without one: but however it is found, that there is no wall ſo ſolid and immovable, and no bond of iron or other metal ſo ſtrong, as to keep this inſtrument perfectly true with regard to the axis of the earth. I have thought therefore of a new contrivance, and propoſe a *mural arch*, furniſhed with a teleſcope and micrometer, to be conſtructed under the following conditions;

1. That it may be ſeen at any point of time, whether the plane of the inſtrument be placed vertically: and

2. Whether a perpendicular paſſes exactly through the centre of the quadrant, and beginning of the diviſion of the limb.

3. That the aberration of the plane of the quadrant from the vertical line may be corrected, without altering the poſition of the beginning of the diviſion on the limb, with regard to the perpendicular: and again that

4. The aberration of the beginning of the diviſion on the limb, from the perpendicular may be corrected, without changing the notable poſition of the plane of the quadrant, with regard to the vertical line: in like manner

5. That the deviation of the plane of the quadrant from the plane of the meridian, may be amended without altering the perpendicular ſituation of the plane of the quadrant, and of the beginning of the diviſion on the limb.

6. That it may be quite free from the variation that may be produced from the extenſion of the metals by heat and cold.

7. That it may eaſily be rectified; that is, that it may eaſily be ſeen, whether the line, paſſing from the object, through the interſection of threads in the tube to the eye, is exactly parallel to the line paſſing thro' the centre of the quadrant, and the diviſion ſhewn by the rule, and to ſet it eaſily right when there is occaſion; a buſineſs otherwiſe very laborious and difficult.

To obtain all theſe requiſites, let there be

1. An iron *fulcrum a a a, c c b*, of which the part to be applied to the wall is deſcribed in *Fig.* 13. and the other in *Fig.* 14. It conſiſts of an iron ſquare *a a a*, and a tranſverſe rule *c c*, ſtrongly faſtened with nails to the ſquare. In *b*, the horizontal arm of the ſquare is bent, that it may project behind, having beſides, a round horizontal hole, the uſe of which will be ſhewn below, and another ſmaller vertical one, formed to receive the ſkrew *m*. Fig. 13, 14.

2. On the back of the *fulcrum*, at the vertical arm of the ſquare, two ears *e h k*, and *d k*, are faſtened with nails. The upper one *e h k* ends below in a cylinder *h*, and ſkrew *k*. The lower one *d g* at *g* is hollowed, and conically excavated in it's lower ſurface: and the *axis* of the cylinder *h*, and the *apex* of the cone *g*, muſt be in the ſame line, and that parallel to the anterior plane of the ſquare *a a*, *Fig.* 14. but the utmoſt exactneſs in theſe is not neceſſary.

ſhall be free from many of the inconveniencies to which thoſe now in uſe are ſubject, by Chriſtian Lewis Gerſten, *F. R. S.* No 483. p. 507. March &c. 1747. *Read* May 7. 1747.

3. Into-

3. Into the wall itſelf, parallel to the plane of the meridian, 3 iron corbels, *a h*, *g h*, *c e*, muſt be let in; and ſtrongly faſtened; two of which, *a h* and *g h*, are nearly equal, and in the ſame perpendicular line. The upper one *h a* has a cylindrical hole *h*, of a ſufficient capacity to admit the cylinder *h* of the *fulcrum*, *Fig.* 14. The lower corbel *b g*, has inſtead of the hole, a conical *apex g*, to enter into the cavity of the ear *g*, *Fig.* 13. The diſtance between theſe corbels muſt be ſuch, that the cylinder *h*, *Fig.* 13, 14, being let into the hole *h*, *Fig.* 16. and the *apex g*, *Fig.* 16. into the cavity *g*, *Fig.* 13. the whole weight of the *fulcrum* may be ſuſtained by the *apex g*, and the *fulcrum* may be turned about horizontally with eaſe. Therefore the part *c* of the upper ear, *Fig.* 14. muſt not preſs upon the corbel, but be at ſome little diſtance from it. But to keep the *apex g*, *Fig.* 16. from ſlipping out of the cavity *g*, *Fig.* 13. a female ſkrew may be added to the male one *k*, by means of which, preſſing the lower part of the corbel *a h* the *fulcrum* is ſufficiently depreſſed vertically to the *apex g*.

Fig. 16.

4. That the cylinder *h*, *Fig.* 14. may be kept ſteady in the hole *h*, *Fig.* 16. let there be added another ſmaller horizontal ſkrew *f*, or 2 on the oppoſite ſides, touching the cylinder in the hole. That the *axis* of the hole *h*, and the *apex g*, *Fig.* 16. may be in the ſame perpendicular, is no difficulty to effect in practice, becauſe theſe corbels may be ſo diſpoſed in the very building of the wall. Firſt, the lower one *g b*, and then, the upper one *h a*, being ſet by a perpendicular, the line of which muſt paſs through the *axis* of a brazen diſk exactly filling the cavity of the hole *h*.

5. The third corbel *c e*, *Fig.* 16. conſiſts of a thick male ſkrew ſtanding out a good way from the wall. The hole *b*, *Fig.* 14. being of ſufficient capacity to let this ſkrew paſs, the upper part of the ſkrew muſt be taken off, that it may have a horizontal plane, on which the ſkrew in *Fig.* 14. may reſt. Therefore the cylinder *h*, *Fig.* 14. being let into the hole *h*, *Fig.* 16. and the cavity *g* of the ear *d*, *Fig.* 14. being applied to the cone *g*, *Fig.* 16. and a female ſkrew being added at *k*, *Fig.* 14. and applied to the lower part of the corbel *a h*, *Fig.* 16. to the ſkrew of the corbel *c e*, let there be applied a plain female ſkrew, orbicular, and indented in the edge, that it may be the more eaſily turned by a key made on purpoſe, and brought near to the part of the corbel *c*. Then, by turning the *fulcrum* about horizontally, let the ſkrew *e*, *Fig.* 16. into the hole of the horizontal arm of the *fulcrum*, and turn the ſkrew *m* about, till it touches the plane of the thick ſkrew of the corbel *e*, and the corbel itſelf ſuſtains ſome part of the weight of the *fulcrum*. Then let there be added another plain and indented female ſkrew to the male one *e*, *Fig.* 16. and let it be turned to the plane of the horizontal arm of the ſquare, which is bent on purpoſe thus to receive this female ſkrew, that it may not hinder the ſuſpenſion of the quadrant on the *fulcrum*, and that the greater length may be allowed to the thick ſkrew of the corbel *e*. Thus the part *b* of the *fulcrum*, *Fig.* 14. reſts vertically on the ſkrew of the corbel *e c*, *Fig.* 16.

and

and is kept in the azimuthal poſition by the 2 indented female ſkrews of the corbel *c e*. Now if any aberration happens in the poſition of the mural plane, it may at any time be corrected by means of theſe indented ſkrews. The reader will eaſily imagine that the hole of the horizontal arm *b* muſt be large enough, and of an oval figure, that it's narrowneſs may not obſtruct the azimuthal motion.

6. The anterior part of the *fulcrum*, *Fig.* 14. has 3 corbels, *n*, *o*, and *p*. The firſt, *n*, is in form of a cube or parallelipiped, only it's upper ſurface is excavated ſemicylindrically. The ſecond, *o*, is in form of a hook, and is deſcribed ſeparately, in *Fig.* 15. The third, *p*, is only a promi- Fig. 15. nent male ſkrew. They all muſt be fitted as firmly and exactly as poſſible.

7. The quadrant itſelf muſt be of ſolid metal. It's anterior face is repreſented in *Fig.* 19. It muſt be of a ſufficient thickneſs, and properly Fig. 19. exceeded by it's limb. To rectify the plane of the limb, there muſt be a rule compoſed *kk* of two, one of which *rr* is perpendicular to the plane of the other *kk*, ſo that it may not eaſily be bent to either ſide. The edge of the rule *rr* muſt be perfectly ſtrait, and ſhew the right line which is in the plane of the anterior ſurface of the limb. This rule is fixed to the back of the quadrant by ſkrews. Now if this rule falls in well with the plane of the limb in *m* and *n*, and another rectilinear rule to examine it Fig. 20. is fixed to the centre, it will eaſily appear whether the limb and edge *rr* of the rule are in the ſame plane, and conſequently whether the plane of the limb is right. For only one right line can be drawn from the points *m* and *n*, which by the hypotheſis really exiſts in the the edge of the rule *rr*: and but one plane can be drawn through the right line *mn* and the point *a*. Now if the examining rule fixt at *a* exactly touches every where the edge of the rule *rr*, and the limb of the quadrant, the plane of the limb muſt neceſſarily be in the plane of the triangle *anm*. After examination and correction, this rule *rr* is ſuperfluous, and may therefore be taken away.

8. In the back of the quadrant, *Fig.* 20. let there be two brazen ſupports *ab* and *e f*, well faſtened with ſkrews to the ſurface of the quadrant. Let the ſupport *ab* have an oval hole in *b*; let there be 2 pointed ſkrews in *c* and *d*. The ſides of the hole are convex above, that the corbel *o*, *Fig.* 14. being let into this hole, the hollow part *a* of the corbel, *Fig.* 15. may be filled, and the points of the horizontal ſkrews, *Fig.* 20. *c* and *d*, may fit the lower convex part of the corbel or hook, *Fig.* 15. on the two oppoſite parts, that ſo there may be no danger of ſhaking.

9. Another ſupport *e f* fixt in *e* to the ſkrews in the plane of the quadrant, has a rectangular hole at *f*, which is entered by the vertical male ſkrews *h* and *i*, the uſe of which is as follows. The corbel or hook *o*, *Fig.* 14. being let into the hole of the ſupport *ab*, *Fig.* 20. the corbel *n* *Fig.* 14. is alſo let into the rectangular hole of the ſupport *e f*, and the *apex* of the ſkrew *h*, which ought to be hemiſpherically convex, ſtands in the cavity of the corbel *n*, *Fig.* 14. and ſo by the motion of the ſkrew *h*, the poſition

of the quadrant becomes ſomething variable by riſing and falling. The hole *f* ought to be pretty large for this purpoſe; but when once a convenient poſition is determined by the upper ſkrew, then the ſhaking of the quadrant becomes uſeleſs, and it is made faſt to the corbel by the lower ſkrew *i*.

10. By theſe two ſupports the quadrant may be kept in it's due poſition, and corrected when there is occaſion, with regard to the beginning of the diviſions on the limb, to make it agree with the perpendicular drawn thro' the centre of the quadrant. But the plane of the quadrant muſt alſo be perpendicular. To effect this there muſt be 2 plano-orbicular female ſkrews, embracing the male ſkrew *p*, *Fig.* 14; the firſt of theſe, which muſt be indented, muſt be applied to the male one *p* before the quadrant is applied to the corbels of the *fulcrum*. But when the quadrant hangs on the ſupports *c d* and *f e*, *Fig.* 20. and the ſkrew *p* is lodged in the hole *c*, *Fig.* 19. and 20, which muſt be ſufficiently large and of an oval figure, that it's poſition may be varied by the upper ſkrew *h*, *Fig.* 20. of the ear; then the back of the quadrant is ſuſtained by the indented orbicular female ſkrew, applied to the male one *p*, *Fig.* 14. above deſcribed, and the face by the other orbicular female ſkrew, which is to be turned about by a ſort of key thruſt into ſome little holes made on purpoſe. The plane therefore of theſe female ſkrews muſt be ſo large as not to enter the hole of the quadrant. And thus the quadrant is not only held tight on both ſides by theſe 2 ſkrews, but alſo can be moved backwards or forwards on occaſion, becauſe it's ſuſpenſion on the corbels *o* and *n*, *Fig.* 14. does not hinder this motion. But becauſe the too great length of the ſkrew *p* is on obſtacle, it will be proper to make the anterior female ſkrew of ſuch a form, as is deſcribed in the ſection *Fig.* 23. where the margin *a b* muſt touch the anterior face of the quadrant, and the neck *c d* muſt enter the hole *c*, *Fig.* 19.

Fig. 23.

11. The centre of the quadrant *a*, *Fig.* 19. is hollowed cylindrically to admit the joint of the rule. In the back of the quadrant, by means of the ſkrews, is fixt a plate *m n*, *Fig.* 20. having a ſquare hole *m*, anſwering to the centre; and let this ſquare be inſcribed in a circle of the hole *a*, *Fig.* 19. or a little leſs. The plate *m n*, *Fig.* 20. muſt have a proper thickneſs, and be doubly bent according to the ſquare, and end in the face in the part *b*, *Fig.* 19. diſtant enough from the plane of the quadrant, to hold a thin ſtyle which enters the centre of a pin, and the ſuſtaining thread of the perpendicular. The pin is delineated in *Fig.* 21. where *a* is the head, and *b* a cylinder exactly filling the cavity of the centre of the quadrant and rule, *c* a ſquare piece to be admitted into the hole *m*, *Fig.* 20. *d* a male ſkrew, to which a female one is to be fitted.

Fig. 21.

12. *Fig.* 22. ſhews the quadrant with the rule and *apparatus* of the perpendicular: *p r* is a line inſcribed on the ſurface of the quadrant, which would paſs thro' the centre if it was continued, where the beginning is of the diviſions on the limb. *h i k g* is a parallelipiped, hollowed as in the figure, faſtened by ſkrews to the plane of the quadrant, in which

Fig. 22.

which the thread *m o* hangs perpendicularly on the line *p r*, which may be eaſily performed. Another thread *i k* is parallel to the plane of the quadrant, but at a diſtance exactly equal to the height of the centre in the pin *c*. Let a third be added like the ſecond in the oppoſite ſide of the parallelipiped *h g v*.

13. The thread, which is to be hung on the thin ſtyle that enters the centre of the pin *c*, is to be made of human hair, eaſily ſuſtaining a weight of half an ounce *f*, and muſt ſwing freely in the cavity of the parallelipiped *h i k g*. The ſmaller threads of the parallelipiped *m o*, *i k*, *&c.* muſt be alſo made of the ſame hair, and have an equal thickneſs. The uſe of them is to ſhew eaſily the poſition of the quadrant with regard to the perpendicular *e f*. For by levelling by the eye thro' the thread *m o* to the line *p r*, one may judge exactly whether *p r*, the beginning of the diviſions on the limb, agrees with the perpendicular *e f*. Again, by levelling thro' the thread *i k* to the other oppoſite, one may ſee whether the plane of the quadrant is parallel to the perpendicular. But if inſtead of the parallelipiped, 2 little bridges are ſubſtituted to ſuſtain theſe 3 threads, the ſame end will be more ſhortly obtained, and as a ſmall ſpace is ſufficient for the oſcillation of the thread *c f*, the diſpoſition of theſe 3 threads may be ſuch, that the level may be taken by convex glaſſes; which will be convenient for thoſe who have not good eyes. The orbicular margin of the female ſkrew *b*, and the male ſkrew of the *fulcrum p*, *Fig.* 14. which it embraces, ſhould project ſo far beyond the ſurface of the quadrant, as not to hinder the oſcillation of the line *c f*, or the place of thoſe ſkrews and of the hole in the quadrant ſhould be without the ſpace of the oſcillation of the line. But if the ſtructure of the obſervatory will permit a view of the ſtars from the horizon to the zenith, then the rule ought to have a free acceſs to the line *p r*, and ſo the parallelipiped *h i k g* muſt be placed a little lower into the appendix, or a little higher into the vertical arm, and the appendix itſelf ought to have a convenient inciſion.

14. I proceed now to the rule itſelf, which is drawn as to the greateſt part ſcenographically in *Fig.* 22. and diſtinguiſhed by the letters *n n n n*, obſerving a juſt magnitude and proportion of it's parts. I ſhall now give a particular explanation of the ſtructure of this rule, becauſe it is very peculiar. *n n n n* is the plane of the rule which turns about upon the pin *c*. The danger of it's bending is prevented by another rule *d d d*, to be ſtrongly faſtened perpendicularly to the plane *n n n*. The diviſions of the limb are ſhewn in the aperture or window of the rule *x x*. *q q* is the teleſcope. Now if you would have the rule to ſhew exactly the degree *o* or 1 of altitude on the limb, there will be nothing wanting in the machine, but to have the line from the object thro' the decuſſation of the threads of the teleſcope to the eye, parallel exactly to the line paſſing thro' the centre of the quadrant, and the degree *o* or 1 of altitude on the limb. But as the tube cannot be fix'd at firſt after this manner to the rule, I would have it ſo connected therewith, that in the

poſition deſcribed in *Fig.* 22. it may have ſome ſort of motion, not only to the altitude, but alſo to parts of the azimuth. The motion to the altitude is performed by the ſkrews *u u*, and the motion to the azimuth by the ſkrews *w w*, all which are more particularly and
Fig. 25. diſtinctly delineated in *Fig.* 25. *& ſeq.*

15. The motion to the altitude is effected after the following manner.
Fig. 30. A foot or little bridge, *Fig.* 30. is faſtened to the tube in a convenient place; the baſe *b c k* is plain and rectangular, at *b* and *c k* the thickneſs of the metal is leſs. The part *i* is perpendicularly faſtened to the baſe, to this is connected another voluble part *n a m h*, by means of the ſtyle *h*, which paſſes thro' the joints of both parts. In *a* the voluble part is excavated, bending in ſuch a manner as to receive the tube faſtened into this canal with tin. The baſe of this foot *b c k* is faſtened to the plane
Fig. 31. of the rule by two brazen pieces, *Fig.* 31. which I call *depreſſors of the foot*. Theſe depreſſors have 2 holes, *k* and *m*, to receive the ſkrews and the 2 *apices h* and *i*, which are to be thruſt into little holes perforated for this purpoſe in the plane of the rule. All this will be better under-
Fig. 26. ſtood by the ichnographical horizontal delineation *Fig.* 26, where *a a* is part of the tube, *b c*, *c e* the foot, *e e* the baſe of the foot, *b c* the voluble part of the foot faſtened to the tube, *f f* the 2 depreſſors of the foot, *g g* male ſkrews, which enter *matrices* excavated in the rule, and in this manner depreſſing the foot at will to the plane of the rule. *Fig.*
Fig. 29. 29. ſhews the vertical delineation; *a a* part of the tube, *e e*, *f f* ſkrews of the depreſſors: and the foot itſelf is hid under the tube: *g*, *h* are 2 male ſkrews, the ends of which are plain, and keep the foot cloſe and unmoveable, and when the horizontal poſition of the tube is to be altered by elevating or depreſſing, it is eaſily performed by the revolutions of theſe ſkrews: but then the ſkrews of the depreſſors of the foot *f f*, *e e*, muſt not depreſs the foot too ſtrongly to the plane of the rule.

16. Now follows the motion to the parts of the azimuth, which be-
Fig. 33. ing delineated *Fig.* 33. we ſhall call the *tabula plicata*. It conſiſts of a rectangular plane of braſs *k f g h*, on which reſts at right angles another plane *a b c f*, the ſides of which, *a f* and *b c*, are in the curvature of a circle drawn from the centre of the hole *q*, the margins, *a f* and *d b c g*, are hollowed at right angles, in the ſame manner as the baſe of the foot *Fig.* 30. To this muſt anſwer 2 depreſſors like that in *Fig.* 31. only theſe, *b c*, muſt have a proper curvature. The plane *k f g h* has rectangular apertures *m r n* and *o ſ p*, to receive the appendages of the
Fig. 32. tube, which I ſhall deſcribe preſently. In *Fig.* 32. *a a* is part of the tube, *e e* the vertical part of the *tabula plicata*, faſtened to the plane of the rule by means of the pin *b*, and turning about the pin, *c c* and *d d*, the depreſſors of the table and their ſkrews. *h h* is a ſection of
Fig. 33. part of the horizontal table, which is mark'd in *Fig.* 33. by the letters *k f g h*. *g g* are peculiar appendages, faſtened to the tube, which end in male ſkrews, and part of this anſwers to the quadrangular perforations *m n* and *o p*, *Fig.* 33. ſo that according to the length *m r* or *o s*,

they

they may be removed this way and that at any convenient diſtance, and yet by means of the female ſkrews *k k*, the little orbs *i i* being interpoſed beforehand, be ſtrongly faſtened whenſoever you pleaſe to the plane of the table. But if, *Fig.* 25. the teleſcope is raiſed or depreſſed by the ſkrews *u u* againſt the ſides of the foot, this is permitted by the pin of the complicated table, the ſkrews of the depreſſors *x x* being a little looſened. Again when it is to be performed to the azimuthal motion, the appendages *w w*, the female ſkrews being on each ſide relaxed, conſequently the tube itſelf is moved forward, which is alſo permitted by the juncture of the foot. But if the appendages *g g*, *Fig.* 32. are connected tranſverſly by a ſtrong piece of metal, this azimuthal motion may be rendered eaſy and ſecure by the uſe of one ſkrew.

17. Beſides the rule has a peculiar appendage, which ſweeps the back of the limb. It conſiſts, *Fig.* 25, and 26. of a part bent at right angles *A m*, very ſtrongly faſtened to the plane of the rule, and another voluble one *k o*, with a ſtyle *m*, connected by it's joints with the immoveable *A m*, in the voluble part the little orb *i*, *Fig.* 26. turning about the cylinder *q*, which ends in a ſkrew, ſweeps the back of the limb, and is preſſed againſt it by the *clauſtrum n s* with the ſkrews *s s s s*, *Fig.* 25. The rule being then moved to any diviſion is faſtened by the ſkrew *v*, to which is objected a thin plate *u t*, *Fig.* 26. which hinders an immediate contact, that the point may not excavate the metal of the limb.

18. In the glaſſes of the teleſcope I do not require what they call a centration, that is, that the greateſt thickneſs may be in the middle of the glaſs, a tedious and laborious buſineſs. I would only have the glaſſes, eſpecially the object glaſs, have a conſtant ſituation in the tube, to which, if they are taken out to be cleaned, they may eaſily be reſtored. The *Engliſh* artificers commonly fix the eye glaſſes, eſpecially of reflecting teleſcopes, ſtrongly in cylinders cut ſpirally, and ſo place them in the tube with fit *matrices*. But if the ſpires are good, and a mark is made in the margin of the cylinder, anſwering to another made in the cavity of the tube, the glaſs will neceſſarily keep the ſame ſituation, tho' it be moved 100 times, provided it be inſerted again into the tube, ſo that one mark may anſwer the other.

19. The micrometer, *Fig.* 28. has a neck, the margin of which is ſcrupulouſly divided into 8 equal parts by lines converging to the centre, or if more vertical threads are required, into as many other parts as you pleaſe. To theſe diviſions are eaſily applied threads either of ſilk or metal, and are faſtened in the neck either by wax, or very thin pins made either of box or metal. *a* is a quadrangular prominence, having another like it in the oppoſite part, but either greater or leſs: let the thickneſs of both be equal to the thickneſs of the tube into which they are inſerted, or a little leſs. The tube itſelf is delineated in *Fig.* 27. In *d* it has a rectangular notch, receiving the prominence of the ring *a*, *Fig.* 28. without ſhaking. The ring of the micrometer muſt be well fitted

Fig. 28.

Fig. 27.

fitted to the cavity of the tube, and when inſerted muſt exactly touch it's inner ſides. *b* is an eye glaſs, at a due diſtance from the threads of the micrometer, *f* the *foramen oculare* cut into a cochleated *operculum b*. When this *operculum* is removed, another muſt be ſubſtituted furniſhed with ſmoaked glaſſes. That the horizontal thread of the micrometer may be always in the ſame ſituation, or if diſturbed be eaſily reſtored to it's place, let the greater tube of the teleſcope, *Fig.* 29. end in a cuſp *i*. Laſtly, when the ocular tube *b* is inſerted, and reduced to it's due ſituation, let a line be drawn on the external ſurface of the tube *b*, to the direction of the margin of the horizontal cuſp *i*, and afterwards let theſe 2 tubes, *a* and *b*, be faſtened to each other by very ſmall ſkrews. But if you deſire a micrometer furniſhed with a moveable thread, then the ſtructure muſt be conveniently altered.

Fig. 29.

20. To proceed now to the rectification of the rule; a plank of thick and ſolid wood muſt be provided, with a horizontal ſurface of nearly the length and breadth of the rule. In the extreme part of the horizontal ſurface let a brazen pin be vertically erected, ending at the top in a ſkrew, and exactly filling the cavity of the central hole. Let it have one of the extremes plain, or a parallelipipedal and prominent brazen table. *h h*, *Fig.* 24. denotes part of a plank, *f e g k* a prominent brazen table, well faſtened to the plank by ſkrews *e e e e*, but ſo that the heads of the ſkrews may not appear above the ſurface of the table. Let the upper ſurface of the metal be well poliſhed, and agree with the upper plane of the plank, and let there be a thin line *a b* drawn upon it, which if continued would paſs thro' the *axis* of the pin. Let there be alſo two brazen parallelipipeds, *c d*, faſtened to the table with 2 ſkrews, but at a proper diſtance from the extremity *g k*, as may be collected from what follows. Let each of them have beneath 4 cylindrical *apices* thruſt into holes bored in the table, that their ſituation may be as firm us poſſible; let theſe 2 parallelipipeds touch each other exactly, and let the plane of contact be perpendicular to the line *a b*. Now therefore if the diviſions of the limb of the quadrant are ſhewn by the inner margin of the aperture *x x*, *Fig.* 22. or by ſome line extended thro' that aperture, then the rule of the quadrant is ſo laid upon the plank, it's upper ſurface being firſt ſo placed horizontally, that the vertical pin of the plank may paſs thro' the central hole, the teleſcope looking upwards, and ſo the parallelipiped *d*, *Fig.* 24. being removed, the margin of the aperture *x x* ſhewing the diviſions *Fig.* 22. or the extended line may be exactly applied to the perpendicular ſurface of the parallelipiped *c*, *Fig.* 24. And when this is done, the margin of the aperture, or the extended thread, will be in a plane perpendicular to the line *a b*. Moreover the rule being faſtened to the table *f g* by means of the ſkrew *v*, *Fig.* 25, 26. let the teleſcope with the plank be directed to any remote object, immoveable; and let the point therein be noted, which is covered by the decuſſation of the threads, and let the plank remain unmoved in that poſition. Afterwards let the rule be inverted, the parallelipipedon *c* be

Fig. 24.

Fig. 25, 26.

removed

removed and reſtored to it's former place *d*, *Fig.* 24; and let the margin of the aperture *x x*, *Fig.* 22. or the thread be applied to the perpendicular plane of the parallelipiped *d*. And as in this ſtate the leſt part of the rule reſts upon the plank, and ſo it is neceſſary that the reſt ſhould preponderate with the teleſcope, a prop furniſhed with ſkrews muſt be combined with the plank. Therefore when the rule is applied in it's inverted ſituation to the ſame perpendicular plane of the line *a b*, *Fig.* 24. you muſt again view the object thro' the teleſcope, and ſee whether the point of decuſſation of the threads is in the ſame point of the object as before. If this is the caſe, the rule has no need of correction. For when the right line *a b* is unmoved, paſſing thro' the centre of the pin of the rule, and the ſame point of the object appears thro' the teleſcope both in a right and an inverted poſition, the line paſſing from the object to the eye thro' the decuſſation of the threads muſt neceſſarily be parallel to the line *a b*. But as this caſe will hardly ever happen at the firſt trial, but the croſſing threads will generally touch another point of the object, the error may be either in the altitude, or azimuth, or both together. In each caſe the poſition of the teleſcope is to be corrected to half the angle of aberration by the ſkrews *u u* and *w w*, *Fig.* 22. as far as this can be determined by the judgment of the eyes. Then the rule is to be laid on again in a right ſituation as before, and the object to be viewed anew, and then you muſt invert it again, and ſee whether the ſame point appears. If not, the poſition of the teleſcope muſt be again corrected by the quantity of half the error. This examination muſt be repeated, as often as any difference appears. When this is done, all the ſkrews *d d*, *c c*, *k k*, *Fig.* 32. and *e e*, *f f*, *g h*, *Fig.* 29. muſt be made faſt, that the teleſcope may remain in that ſtate. If this manifold inverſion of the rule on the plank, which may however be performed in a reaſonably ſhort time, ſeems too tedious, he may add a micrometer with a moveable thread, or rather a white table, *b c d e*, *Fig.* 18. which has 2 black *faſciæ*, *h l*, *g f*, croſſing each other at right angles, in a horizontal and vertical ſituation, which he may diſpoſe at a convenient diſtance, and then ſo direct the plank with the teleſcope laid on it in a right ſituation, that the point of decuſſation of the *faſciæ* may be in the point of decuſſation of the threads in the tube. The rule being inverted, without moving the plank, an aſſiſtant muſt be near the table, to perform the directions of the obſerver by ſigns, and he muſt have 2 other black *faſciæ*, *k m* and *n o*, which he may move at the beck of the obſerver, in a ſituation parallel to *g f* and *h l*, which may eaſily be done by ſome peculiar ſtructure in the plank, *n o* horizontally and *k m* vertically. But when the rule is inverted on the plank, if the point of decuſſation falls on a point of the table, for inſtance *g*, then the aſſiſtant muſt ſo diſpoſe both the *faſciæ k m* and *n o* ſucceſſively, that the vertical thread of the tube may fall on the *faſcia h e*, and the horizontal one of the tube on the *faſcia* of the table *k m*. When this is finiſhed the in-

Fig. 32. Fig. 29.

tervals

tervals *f o* and *k h* are to be biſected, and the *faſciæ* on the table to be placed by a motion parallel to thoſe points of biſection. Then, without moving the plank, the rule with the teleſcope is reſtored to it's right ſituation, and the poſition of it rectified by the ſkrews, till the point of decuſſation of the *faſciæ* on the table, coincides with the point of decuſſation of the threads in the tube. But if you look at the table, the firſt time with the rule and tube inverted, and the ſecond time in the right ſituation, mark the point of decuſſation on the table, and by putting the moveable *faſciæ* in the right place, the error of the rule may be corrected in the ſame ſituation. And ſo the poſition of the tube with regard to the rule will be ſuch, that the ſame point of the object may be ſeen in the tube, either in the right or inverted poſition of the teleſcope, and conſequently the rule of the quadrant will be rectified.

21. But if the rule cannot be hindered from being too heavy, for a convenient direction of it to the ſtars, there are two ways of remedying this inconvenience. The firſt is that which *Flamſtead* long ago applied to his ſextant, and deſcribed in his *Hiſt. Cæleſt.* The exterior limb of the quadrant is cut with correſpondent notches, and ſwept by the perpetual ſkrew in the verſatile appendage of the rule. But then the apparatus of the appendage, *f q*, *Fig.* 26. ſhould not be omitted, that the plane of the rule may be exactly contiguous to the plane of the limb. For in the mural ſextant at *Peterſburg*, made by the famous *Rowley*, I obſerved this defect, that the margin of the aperture *x x*, *Fig.* 22. which ſhews the diviſions of the limb, preſſes the limb indeed very well, but the other part of the rule is too far diſtant from the plane of the limb, ſo that the teleſcope ſhakes, though the ſkrew be turned ever ſo cloſe. But this whole artifice ſeems to me to be too laborious, and not convenient enough in obſervations. For it does not appear to me ſafe enough, either to examine, or correct, or compare, the diviſions of the limb by the revolutions of the ſkrew; but to raiſe the weight of the rule, and by this artifice to cauſe a more eaſy direction of the tube to the ſtars, is abundantly too laborious. I would chooſe to make uſe of one 100 times more
Fig. 17. ſimple. Let *a b c*, *Fig.* 17. be a mural quadrant, *a d* the rule; in *m*, vertical to the centre *a* of the quadrant, let there be an axis of an iron bar, *g m f*, ſo that the arm *m f*, and it's revolution, may be very nearly in the plane of the quadrant, and the other *m g* in another diſtant parallel. Let the length *m f* be about $\frac{1}{2}$ the length of the rule *a d*, and the length *m g*, 3 or 4 inches. Let the angle *g m f* of the rotation *m*, and of the ſuſpenſions *g f*, be a right one. At *f* let the rule be connected by a little chain or ſmall cord, ſo that *m f* may be equal and parallel to *a e*. At *g* let a ſmall cord *g h* be faſtened, and extended horizontally to the extremity of the room; there let it be ſupported by a pulley *h*, and let down almoſt to the ground, with the weight *k* hung to it. Now if this weight *k* is made ſufficient for the rule, it will remain in any ſituation, whether it be elevated or depreſſed. It will be but a ſmall obſtacle, that the centre of gravity of our rule will be diſtant from the point of ſuſpenſion, becauſe

the

the preſſure of the elaſticity in the back of the limb, will ſufficiently moderate the unequal action of gravity.

22. It remains now to ſhew that the inſtrument deſcribed is ſufficient for all it's requiſites.

1. As the thread *ik*, *Fig.* 22. and the other ſimilar to it, in the oppoſite part of the parallelipiped have a ſituation parallel to the plane of the quadrant, and the ſame diſtance and point of ſuſpenſion *c*, it will eaſily appear by looking, whether the thread of the perpendicular is in the plane of theſe threads, and ſo the plane of the quadrant in a vertical poſition.

2. Becauſe the thread *mo*, is in the plane of the line *pr*, and that perpendicular to the plane of the quadrant, it will be eaſily known by looking through *mo* and *pr*, whether the perpendicular is in this plane, and conſequently, whether the beginning of diviſion *pr* is in a vertical plane.

3. Becauſe the quadrant is ſuſpended by 2 points, firſt, by the hook *o*, *Fig.* 14. and then in the cavity of the corbel *n*, nothing hinders it's vertical motion but the 2 female ſkrews, embracing the male one *p*. By theſe ſkrews, therefore, the ſituation of the plane of the quadrant to the perpendicular, may be corrected; and as by this correction, the horizontal arm of the quadrant will not be inclined, it follows, that this correction is independent on the horizontal ſituation of the quadrant.

4. The hook, *Fig.* 15. is not only concave in *a*, but alſo convex, ſo that it's ſection *ab* is circular: therefore this form of the hook does not hinder the quadrant from being a little raiſed or depreſſed in the corbel *n* by the ſkrew *h*, *Fig.* 20. Therefore, as the poſition of the beginning of diviſion, depends, at the ſame time, on the poſition of the horizontal arm, it is evident, by the motion of the ſkrew *h*, that the poſition of the beginning of diviſion, with regard to the perpendicular, may be corrected.

5. As the iron *fulcrum* itſelf has ſome horizontal motion in the corbels *ah* and *gb*, *Fig.* 16. and the *axis* of rotation is perpendicular, it follows, that all the reſt remaining, the deviation of the plane of the quadrant, from the plane of the meridian, may be corrected. And if even the *axis* of rotation of the *fulcrum* ſhould not be exactly perpendicular, yet it will not hinder the obſerver from diſcovering to what part the inclination of this *axis* tends; and ſo he may make his corrections as occaſion requires.

6. The quadrant itſelf is of one ſolid metal. Now if this is extended or contracted by heat or cold, it will always remain ſimilar to itſelf. Nor does the ſuſpenſion of it hinder it's extenſion. For in the *fulcrum*, the corbel *u*, *Fig.* 14. has a horizontal canal, in which the apex of the ſkrew *h*, *Fig.* 20. reſts, and the hole *c*, *Fig.* 19. is large enough for the ſmall extenſion or contraction which heat or cold produces, nor is the plain ſurface of the female ſkrews, which cover the hole on both ſides, and faſten the arm, any obſtacle.

7. Laſtly, as to the rectification, that ſufficiently appears from the precepts. Every one will underſtand, that it is more eaſy and expe-

ditious than thoſe in uſe; and as the plank conſtructed for the rectification of the rule may be preſerved, the obſerver may at any time without much labour examine his rule anew. Thus the inſtrument anſwers all it's requiſites.

But as it very rarely happens, that houſes are ſo built, as to have walls in the plane of the meridian, or at leaſt, places fit for conſtructing theſe walls, the mural arches have hitherto required a building expreſsly diſpoſed for this purpoſe. But any one will eaſily underſtand that our contrivance is applicable to almoſt any place. If, for inſtance, in ſuch an opening as is uſually made for doors, 2 corbels *a b*, *g b*, *Fig.* 16. are let into the wall, the third *e c* does not require a wall exactly contiguous; but may be fixed ſtrongly enough in a piece of iron projecting a good way from the plane of the wall.

Deſcription *and* Uſes *of an* Equatorial Teleſcope, *by Mr* James Short, *F. R. S. to the Pref.* No. 493. p. 241. Oct. &c. 1749. *Read* Dec. 7. 1749.

XXV. I have made 3 of theſe inſtruments, one of which was bought by Count *Bentink* for the Prince of *Orange*; the other two I have ſtill by me, one of which I ſhall ſhew to the Society. I do not pretend to any thing new in the combination of theſe circles, of which this inſtrument conſiſts, the ſame combination having ſeveral times been made before me by way of a dial: but I believe the putting ſo large a teleſcope upon this machinery, and applying it to the uſes which I have done, is ſomewhat new.

Deſcription and Uſes of the Equatorial Teleſcope, or Portable Obſervatory. Fig. 34.

This inſtrument conſiſts of 2 circular planes or plates, *A A*, which are ſupported upon 4 pillars; and theſe are again ſupported upon a croſs-foot, or pedeſtal moveable at each end by the 4 ſcrews *B B B B*: the two circular plates *A A* are moveable, the one above the other, and are called the horizontal plates, as repreſenting the horizon of the place; and upon the upper one are placed 2 ſpirit-levels to render them at all times horizontal: theſe levels are fixed at right-angles to one another: this upper plate is moved by a handle *C*, which is called the horizontal handle, and is divided into 360°, and has a *Nonius* index divided into every 3′.

Above this horizontal plate there is a ſemicircle *D D*, divided into twice 90°; which is called the meridian ſemicircle, as repreſenting the meridian of the place, and is moved by a handle *E*, which is called the meridian handle, and has a *Nonius* index divided into every 3′.

Above this meridian ſemicircle is faſten'd a circular plate, upon which are affixed 2 other circular plates *F F*, moveable the one upon the other, and are called the equatorial plates; one of them, repreſenting the plane of the equator, is divided into twice 12 hours, and theſe are ſubdivided into every 10′ of time. This plate is moved by a handle *G*, called the equatorial handle, and has a *Nonius* index for ſhewing every minute.

Above this equatorial plate there is a ſemicircle *H H*, which is called the declination-ſemicircle, as repreſenting the half of a circle of declination, or horary circle, and is divided into twice 90°, being moved by the han-

dle

Fig. 13.

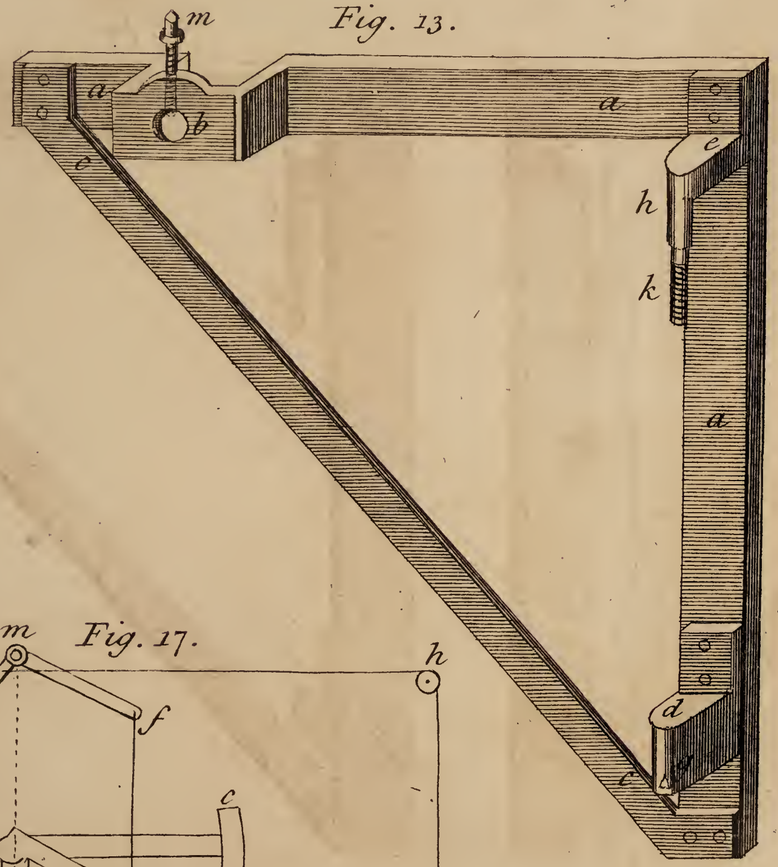


Fig. 14.

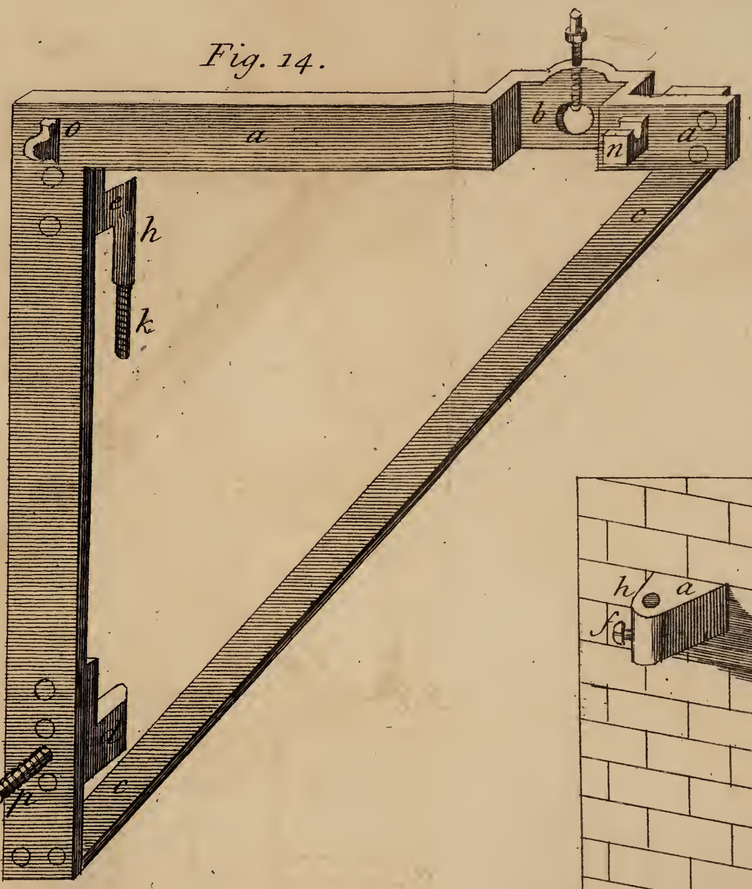


Fig. 15.

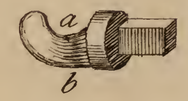


Fig. 16.

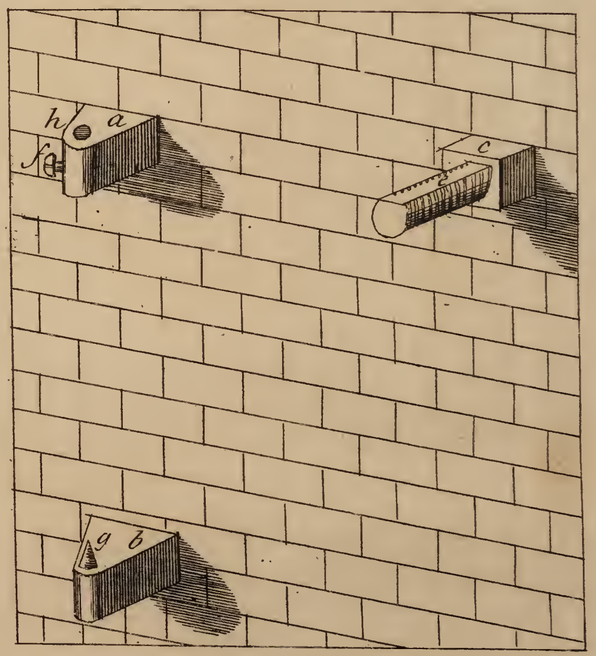
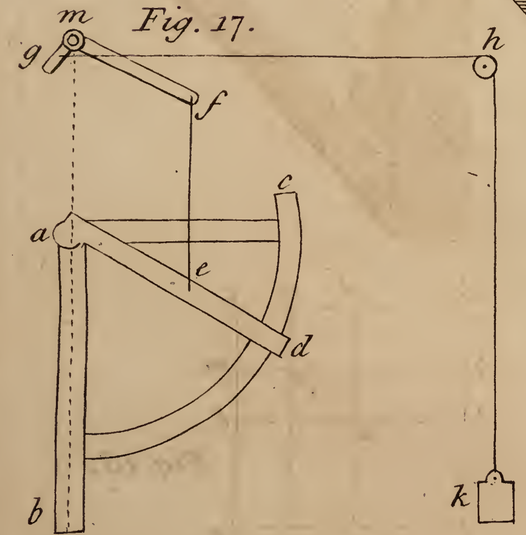


Fig. 17.



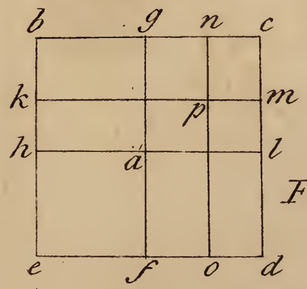


Fig. 18.

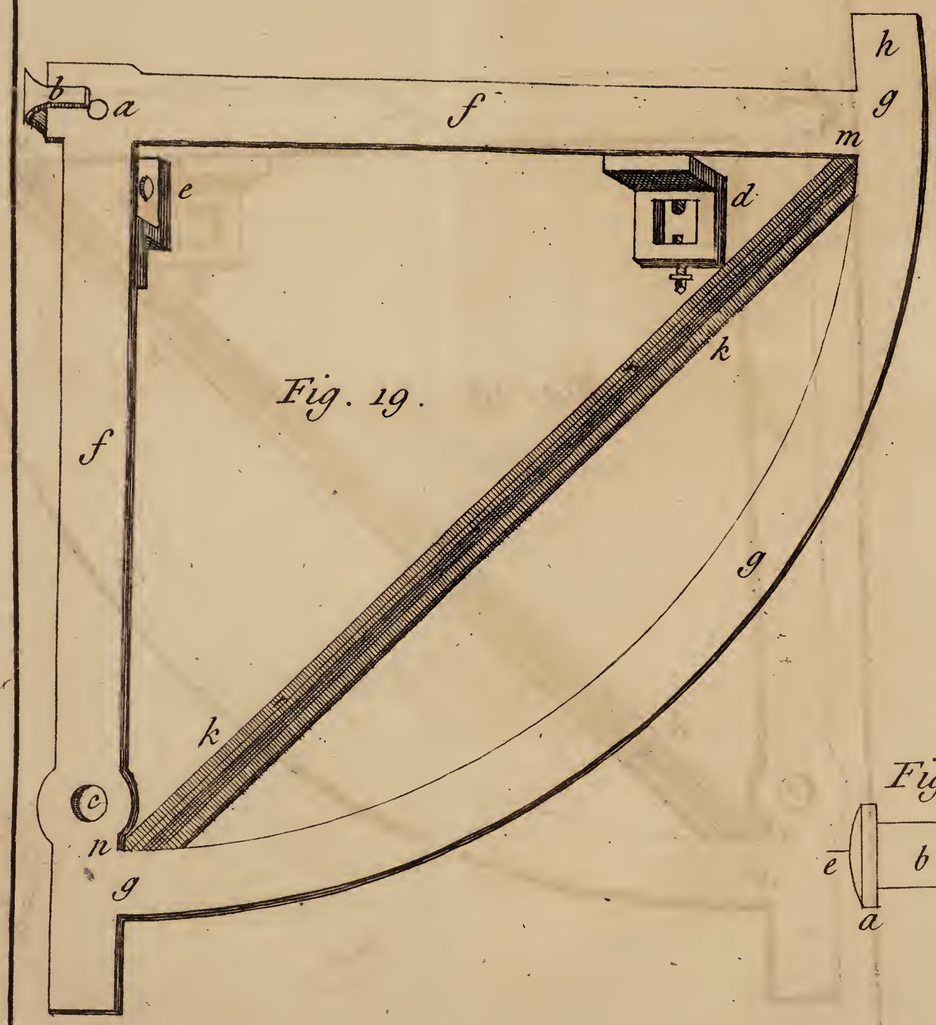


Fig. 19.

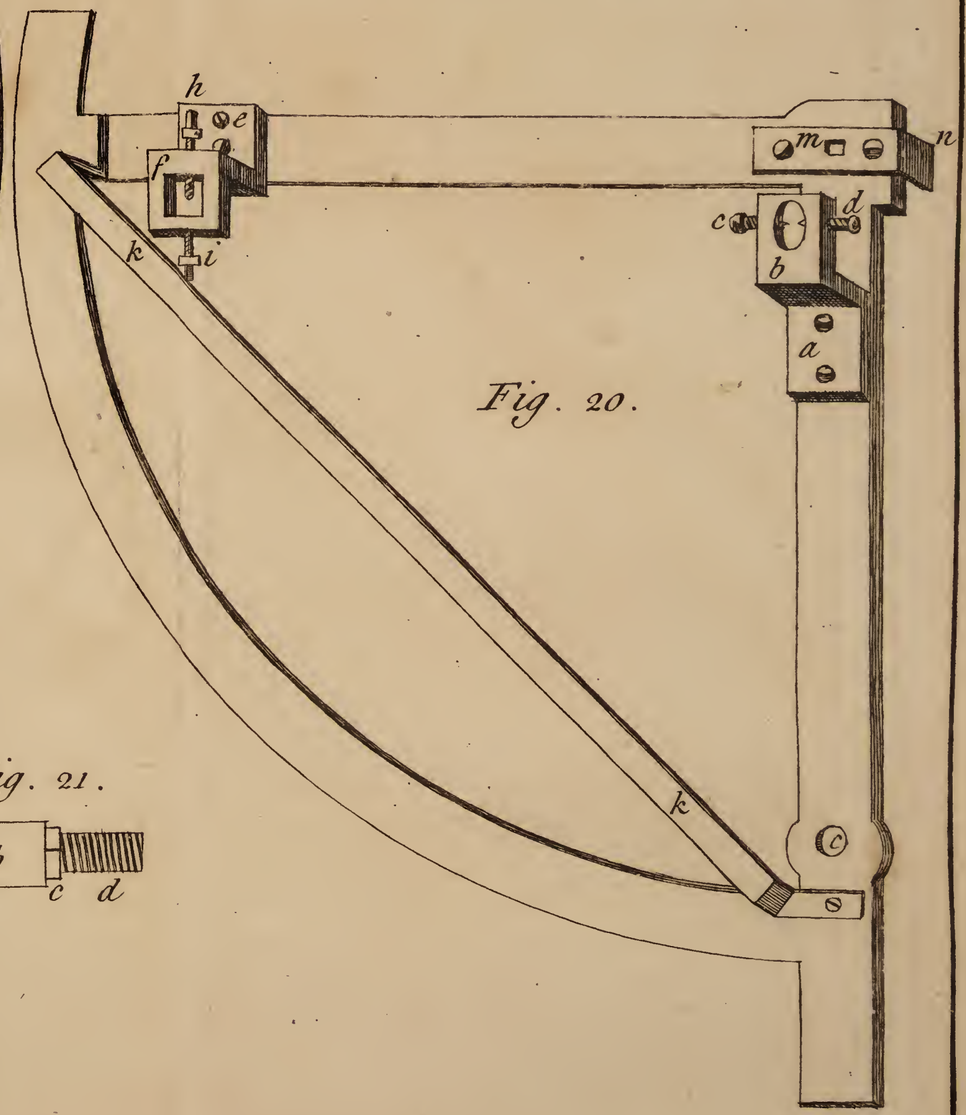


Fig. 20.

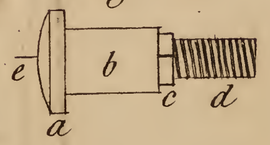


Fig. 21.

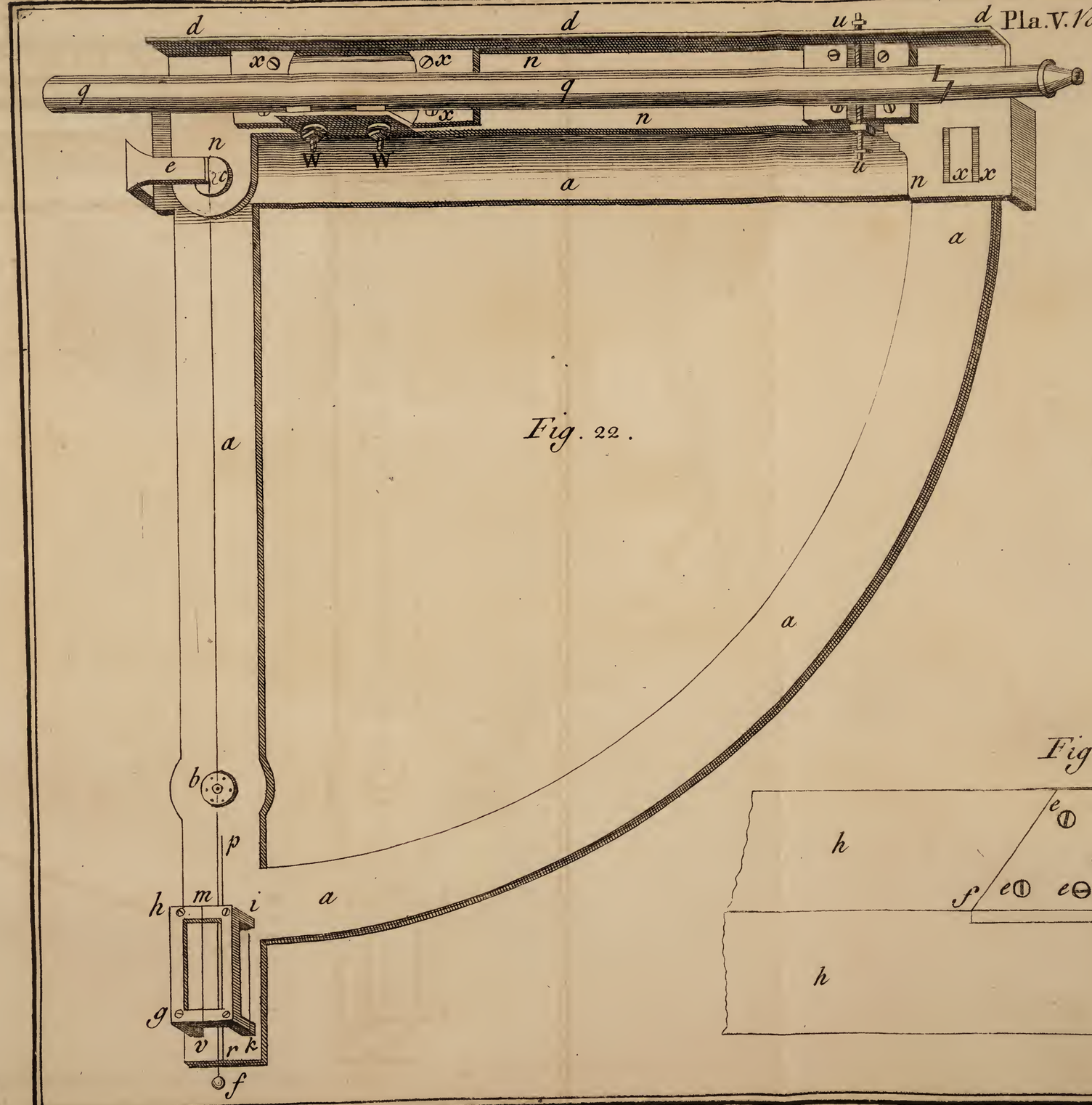


Fig. 22.

Fig. 23.

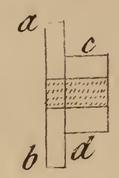
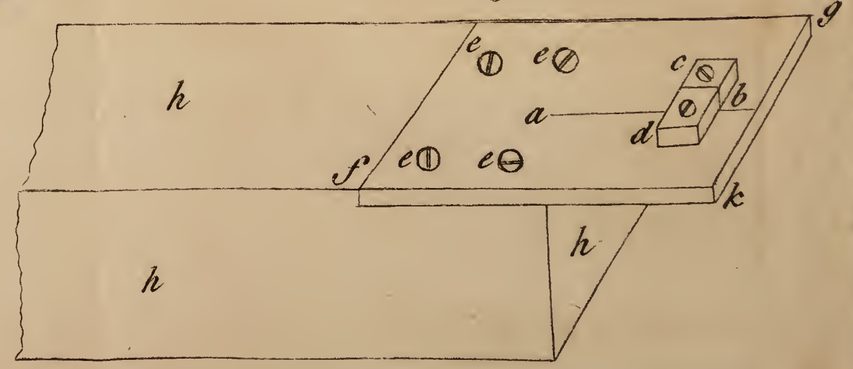


Fig. 24.



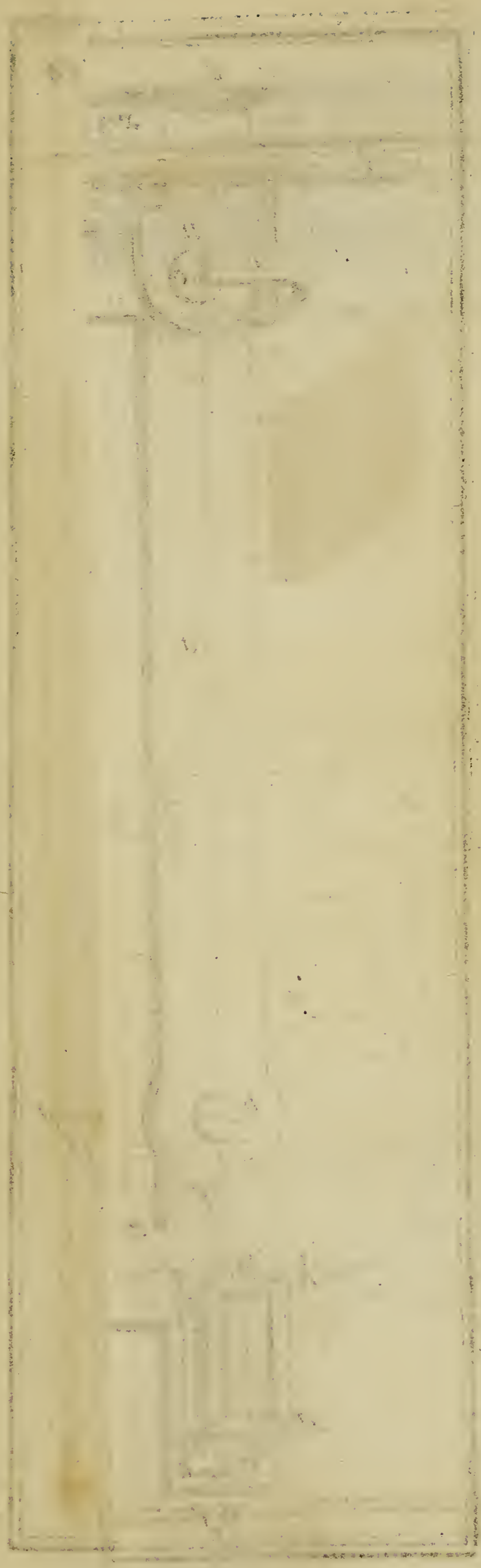


Fig. 27.

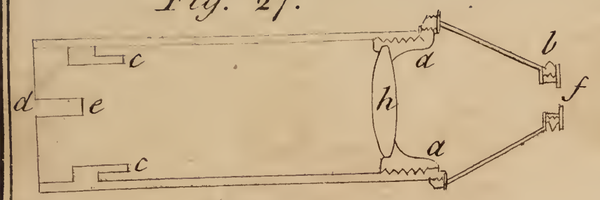


Fig. 28.

Fig. 26.

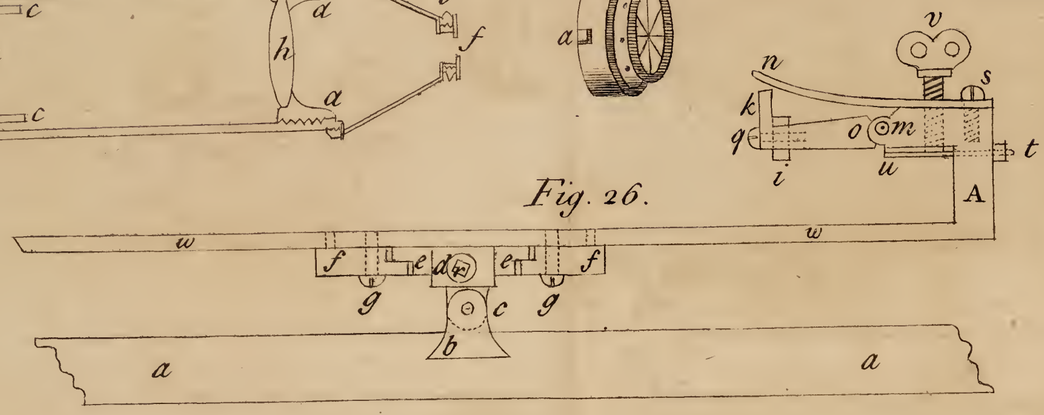


Fig. 30.

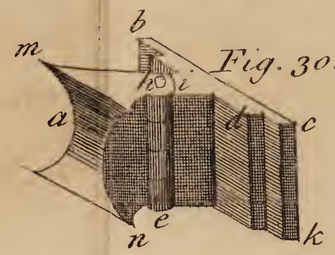


Fig. 31.

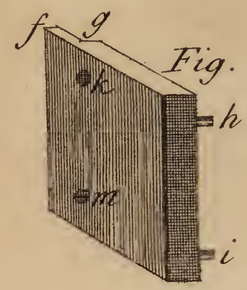


Fig. 29.

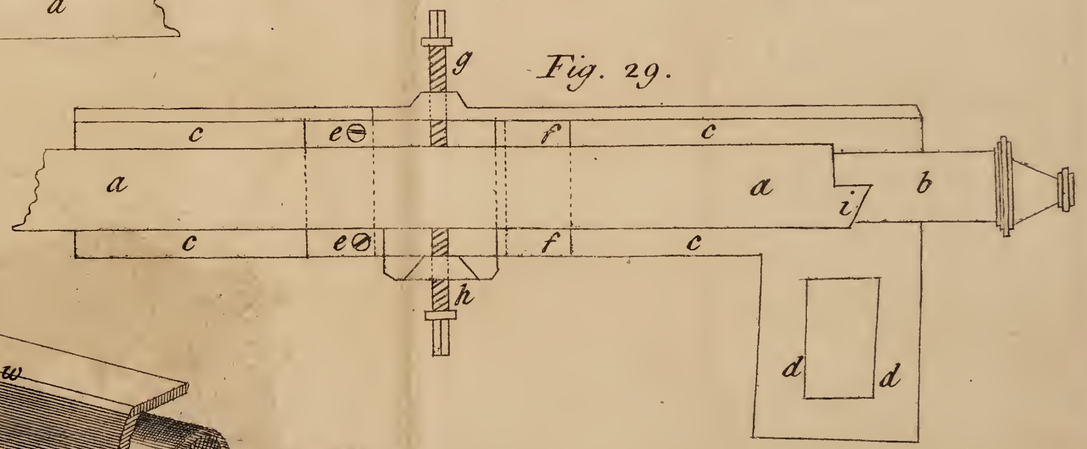


Fig. 25.

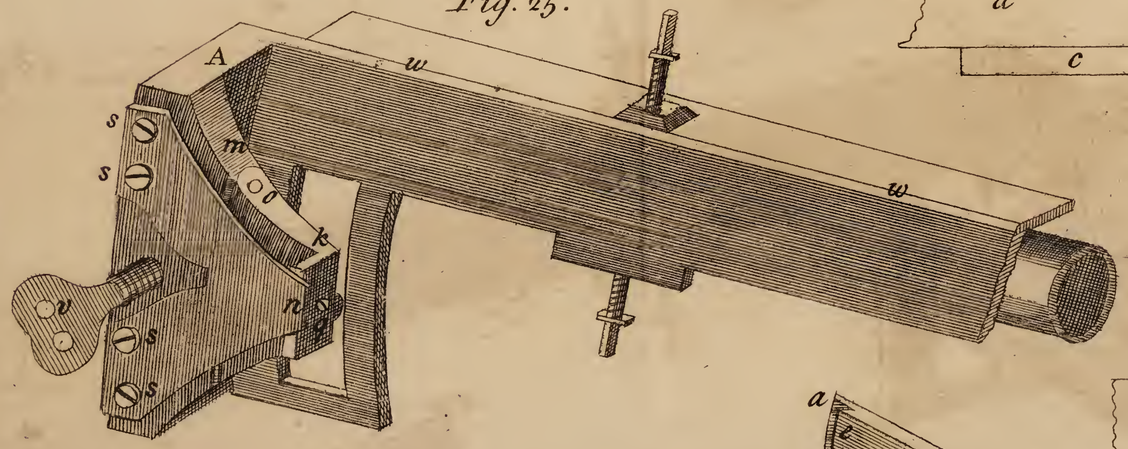


Fig. 32.

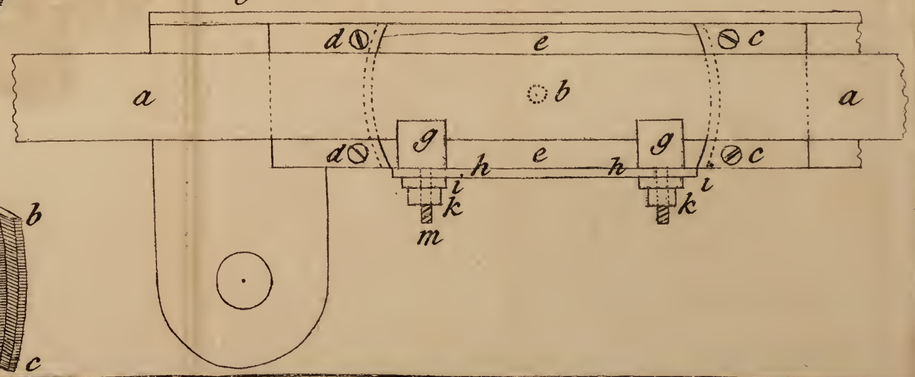
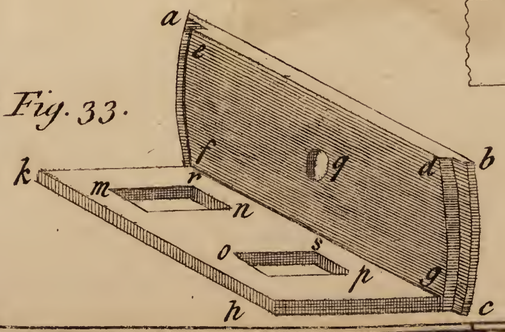
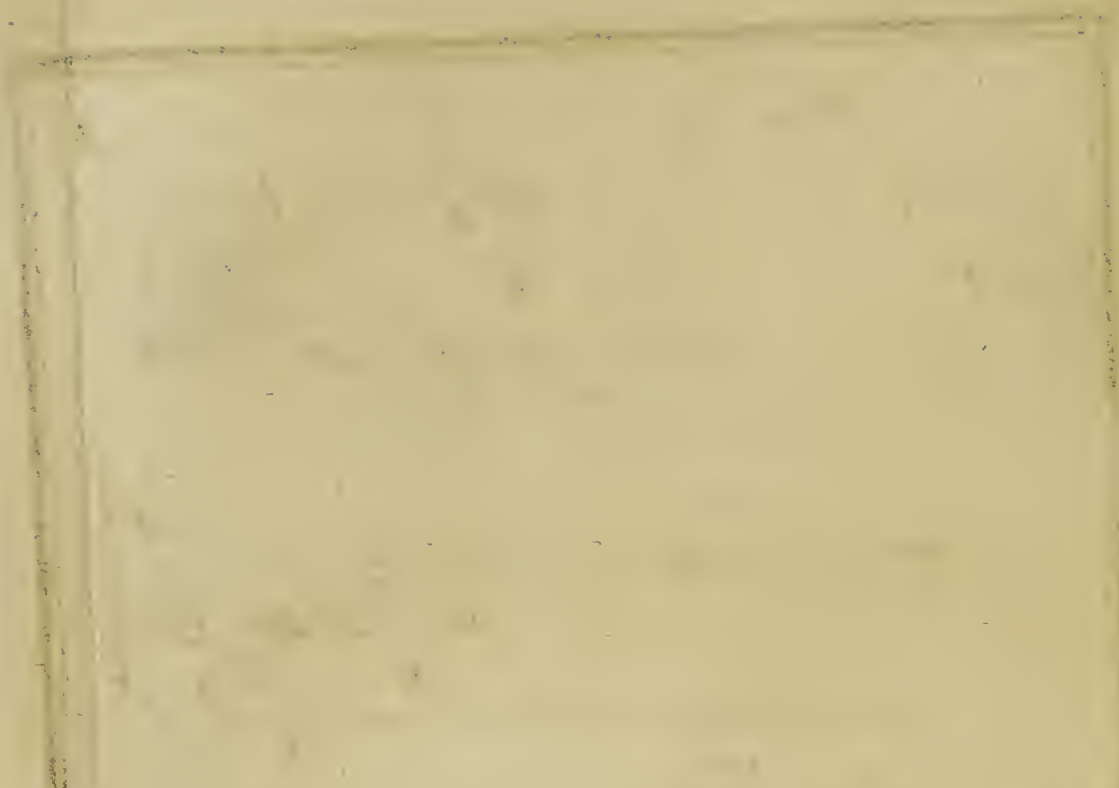


Fig. 33.





dle *K*, which is called the declination-handle. It has alſo a *Nonius* index for ſubdividing into every 3′.

Above this declination-ſemicircle is faſtened a reflecting teleſcope *LL*, of the *Gregorian* conſtruction, the focal length of it's great ſpeculum being 18 inches.

In order to adjuſt the inſtrument for obſervation, the firſt thing to be done, is to make the horizontal plates level or horizontal, by means of the 2 ſpirit-levels, and the 4 ſcrews in the croſs-pedeſtal. This being done, you move the meridian ſemicircle, by means of the meridian handle ſo as to raiſe the equatorial plates to the elevation of the equator of the place; which is equal to the complement of the latitude (and which, if not known, may likewiſe be found by this inſtrument, as ſhall be afterwards ſhewn). And thus the inſtrument is ready for obſervation.

To find the hour of the day, and Meridian of the place.

Firſt find, from aſtronomical tables, the ſun's declination for the day, and for that particular time of the day; then ſet the declination-ſemicircle to the declination of the ſun, taking particular notice whether it is N. or S. and ſet the declination-ſemicircle accordingly.

You then turn about the horizontal handle, and the equatorial handle, both at the ſame time, till you find the ſun preciſely concentrical with the field of the teleſcope. If you have a clock or watch at hand, mark that inſtant of time; and by looking upon the equatorial plate, and *Nonius* index, you will find the hour and minute of the day, which comparing with the time ſhewn by the clock or watch, ſhews how much either of them differ from the ſun. In this manner you find the hour of the day.

Now, in order to find the meridian of the place, and conſequently to have a mark, by which you may always know your meridian again, you firſt move the equatorial plate by means of the equatorial handle, till the meridian of the plate, or hour-line of 12, is in the middle of the *Nonius* index; and then, by turning about the declination-handle till the teleſcope comes down to the horizon, you obſerve the place or point which is then in the middle of the field of the teleſcope; and a ſuppoſed line drawn from the center of this field to that point in the horizon, is your meridian line. The beſt time of the day for making this obſervation for finding your meridian, is about three hours before noon, or as much after noon. The meridian of the place may be found by this method ſo exact, that it will not differ at any time from the true meridian above 10″ of time; and if a proper allowance be made for the refraction at the time of obſervation, it may be found much more exact. This line thus found will be of uſe to ſave trouble afterwards; and is indeed, the foundation of all aſtronomical obſervations.

To find a ſtar or planet in the day-time, even at noon-day.

The inſtrument remaining as rectified in the laſt experiment, you ſet the declination-ſemicircle to the declination of the ſtar or planet you want to ſee; and then you ſet the equatorial plate to the right aſcenſion of the ſtar or planet at that time, and, looking thro' the teleſcope, you will ſee the ſtar or planet; and after you have once got it into the field, you cannot loſe it: for as the diurnal motion of a ſtar is parallel to the equator

by your moving the equatorial handle ſo as to follow it, you will at any time, while it is above the horizon, recover it, if it be gone out of the field.

The eaſieſt method for ſeeing a ſtar or planet in the day-time is this: your inſtrument being adjuſted as before-directed, you bring the teleſcope down ſo as to look directly at your meridian mark; and then you ſet it to the declination, and right aſcenſion, as before-mentioned.

By this inſtrument moſt of the ſtars of the firſt and ſecond magnitude have been ſeen even at mid-day, and the ſun ſhining bright; as alſo *Mercury*, *Venus*, and *Jupiter*: *Saturn* and *Mars* are not ſo eaſy to be ſeen, upon account of the faintneſs of their light, except when the ſun is but a few hours above the horizon.

And in the ſame manner in the night-time, when you can ſee a ſtar, planet, or any new phænomenon, ſuch as a comet, you may find it's declination and right aſcenſion immediately, by turning about the equatorial handle and declination-handle, till you ſee the ſtar, planet, or phænomenon; and then, looking upon the equatorial plate, you find it's right aſcenſion in time; and you find, upon the declination-ſemicircle, it's declination in degrees and minutes.

In order to have the other uſes of this inſtrument, you muſt make the equatorial plates become parallel to the horizontal plates: and then this inſtrument becomes an *equal altitude inſtrument*, a *tranſit inſtrument*, a *theodolite*, a *quadrant*, an *azimuth inſtrument*, and a *level*. The manner of applying it to theſe different purpoſes is too obvious to need any explanation.

As there is alſo a box with a magnetic needle faſtened in the lower plate of this inſtrument, by it you may adjuſt the inſtrument nearly in the meridian; and by it likewiſe you may find the variation of the needle: if you ſet the horizontal meridian, and the equatorial meridian, in the middle of their *Nonius* indexes, and direct your teleſcope to your meridian mark, you obſerve how many degrees from the meridian of the box the needle points at; and this diſtance or difference is the variation of the needle.

Improvement of the Celeſtial Globe, *by Mr* James Ferguſon. No. 483. p. 535. Mar. &c. 1747. *Read* May 14. 1747. Fig. 35.

XXVI. On the *axis* of the globe above the hour-circle, is fixed the arch *A* at one end by the ſcrew *D*, ſo as to leave ſufficient room for turning the hour-index occaſionally: the other end at *B*, being always over the pole of the ecliptic, has a pin fixed into it, whereon the collets *C* and *B* are moveable by their wires *F* and *G*, when the ſcrew *E* is ſlackned, and may be made faſt at pleaſure by this ſcrew; ſo that the turning of the globe round will carry the wires round with it, ſhewing thereby the apparent motions of the ſun and moon by the little balls on their ends at *H* and *I*. On the collet *C*, in which the ſun's wire is fix'd, there is alſo fix'd the circular plate *L*, whereon the $29\frac{1}{2}$ days of the moon's age are engraven, which have their beginning juſt below the ſun's wire; conſequently the plate *L* cannot be turned without carrying the ſun's wire along with it; by which

which means the moon's age is always counted from the ſun; and the moon's wire being turned ſo as to be under the day of her age on this plate, will ſet her at her due diſtance from the ſun for that time. Theſe wires being quadrants from *C* to *H*, and from *B* to *I*, muſt ſtill keep the ſun and moon directly over the ecliptic; becauſe the center of their motions at *C* and *B* is perpendicularly over the pole of the ecliptic; in the *arctic* circle. But, becauſe the moon does not keep her courſe in the ecliptic, as the ſun appears to do, but has a declination of $5\frac{1}{3}$ degrees on each ſide of it in every lunation, ſhe is made to ſcrew on her wire as far on both ſides as her declination or latitude amounts to. For this purpoſe *K* is a ſmall piece of paſteboard, to be applied over the ecliptic at right angles; the middle line *o o* ſtanding perpendicularly thereon. From this line there is $5\frac{1}{3}$ degrees marked on each ſide upon the outward limb; which reaching to the moon, makes her to be eaſily adjuſted to her latitude at any time. ——*N. B.* The horizon ſhould be ſupported by two ſemicircular arches, inſtead of the uſual way of doing it by pillars: becauſe the arches will not ſtop the progreſs of the balls, when they go below the horizon in an oblique ſphere.

To rectify the Globe. Elevate the pole to the latitude of the place; then bring the ſun's place in the ecliptic to the brazen meridian, and ſet the hour-index to XII at noon: keeping the globe in this poſition, ſlacken the ſcrew *E*, and ſet the ſun directly over his place in the meridian; which done, ſet the moon's wire under the day of her age for that time on the plate *D*, and ſhe will ſtand over her place in the ecliptic for that time, and you will ſee in what conſtellation ſhe then is. Laſtly, faſten the wires by the ſcrew *E*, and the globe will be rectify'd.

To find the riſing and ſetting of the ſun and moon, with their amplitudes on the horizon.

The globe being rectify'd as above to the given time, turn it round in the uſual way, and you will ſee the ſun and moon riſe and ſet for that day on the ſame points of the horizon as they do in the Heavens. The times of their riſing and ſetting are ſhewn by the hour-index, which likewiſe ſhews the time of the moon's paſſing over the meridian. If you want to ſee to greater exactneſs the riſing and ſetting of the moon, find her latitude for that day by the *Ephemeris*; and as it is S. or N. ſcrew her ſo many degrees from the ecliptic, meaſuring them by the paſteboard *K*, appling it to the ecliptic as abovemention'd; and then turning the globe round, you will ſee the time of the moon's riſing and ſetting by the hour-index, and her amplitude on the horizon for that time, as it is affected by her latitude, which will ſometimes be very conſiderable.

This may be very uſeful, eſpecially in giving lectures upon the globes; becauſe a large company at ſome diſtance will eaſily ſee this ſun and moon going above and below the horizon as they riſe and ſet, and likewiſe their appulſes to different fix'd ſtars: whereas in the uſual way, when there is only the ſun's place in the ecliptic ſhewn, it is not eaſy for any one to keep his eye upon that part of the ecliptic as the globe is turned round, unleſs it be in ſome remarkable circle of longitude; and it is not

very

very eaſy to know the moon's place, unleſs at her conjunction, oppoſition, or quadratures.

This ſimple *apparatus* ſhews all the varieties that can happen in the riſing and ſetting of the ſun and moon, which are very curious, eſpecially about the poles, where the ſun is preſent for one half of the year, and abſent for the other half; the moon in winter ſhining conſtantly without ſetting from the firſt to the third quarter, in the ſun's abſence; and in ſummer the full moon is never ſeen at the poles; for ſhe ſets at the firſt quarter, and riſes not till the third, ſave what may happen on account of her latitude.

All the *phænomena* of the harveſt-moon become very plain by this additional part: and in making ſome trials I find, that, to ſome places of the earth, the moon will not differ above an hour in her riſing for 15 nights together, but will differ ſometimes 23^h in her ſetting, within the compaſs of that 15 days; and for the next 15 ſhe will ſet within 1^h of the ſame time, and differ 23^h in her riſing. This is taken in round numbers, but may be conſider'd with more exactneſs by thoſe who are better acquainted with the celeſtial motions. I ſhall only add, that the places of the earth where theſe *phænomena* happen, are thoſe lying under the polar circles.

A letter from the Widow of the late Mr John Senex, *F. R. S. to* M. Folkes, *Eſq;* P. R. S. *concerning the large Globes prepared by her late Huſband, and now ſold by herſelf, at her Houſe over-againſt* St Dunſtan's Church *in* Fleet-Street. No. 493. p. 290. Oct. *&c.* 1749. *Read* Jan. 19. 1748-9.

XXVII. The Royal Society being lately acquainted with ſome improvements that were ſaid to have been made upon the globes at *Nuremberg*, and deſired to encourage and recommend the ſame *, I am obliged to return you my moſt grateful acknowledgments for your kind interpoſition in behalf of mine. It is ſufficiently known, that works of art, made in our own country, have, for the moſt part, a degree of exactneſs much ſuperior to thoſe of foreign countries: and I hope I may be allowed to ſay in particular, and without diſparagement to the performances of others, that my globes will be found, upon examination, as truly made, as accurate, and as well adapted for the purpoſes of Geography and Aſtronomy, as any now extant. For (not to mention that the *terreſtrial* is formed from the beſt maps that could be made or procured, and contains no material error in the ſituation of any places where obſervations have been really and truly made) the *celeſtial*, upon the niceſt examination, will be found to have this advantage above all others, that the figures of the *conſtellations* there given, were originally dilineated by a gentleman, whoſe ſkill in performances of this nature was very well known and allowed; under the direction of the Great Dr *Halley*, to whoſe kindneſs my late huſband was upon all occaſions particularly indebted. And beſides this, to each ſtar are added *Bayer*'s letters of reference; a circumſtance extremely uſeful, either for the tracing out the path of a comet, or for deſcribing any new *phænomenon* in the Heavens.

It may be further obſerved, that celeſtial globes, as they are commonly fitted up, are adjuſted only to one particular year; though indeed they

* See Vol. VIII. p. 217.

may

Fig. 35.

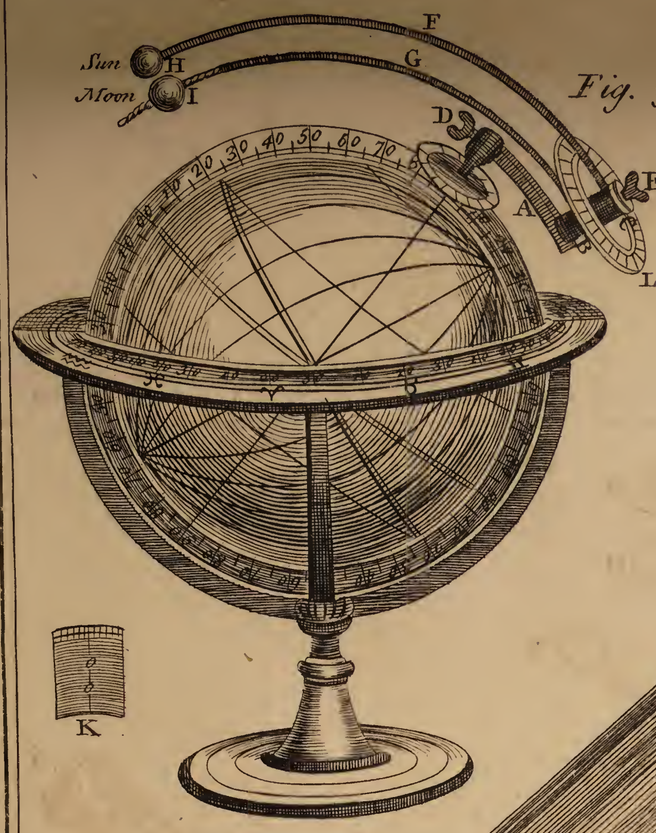
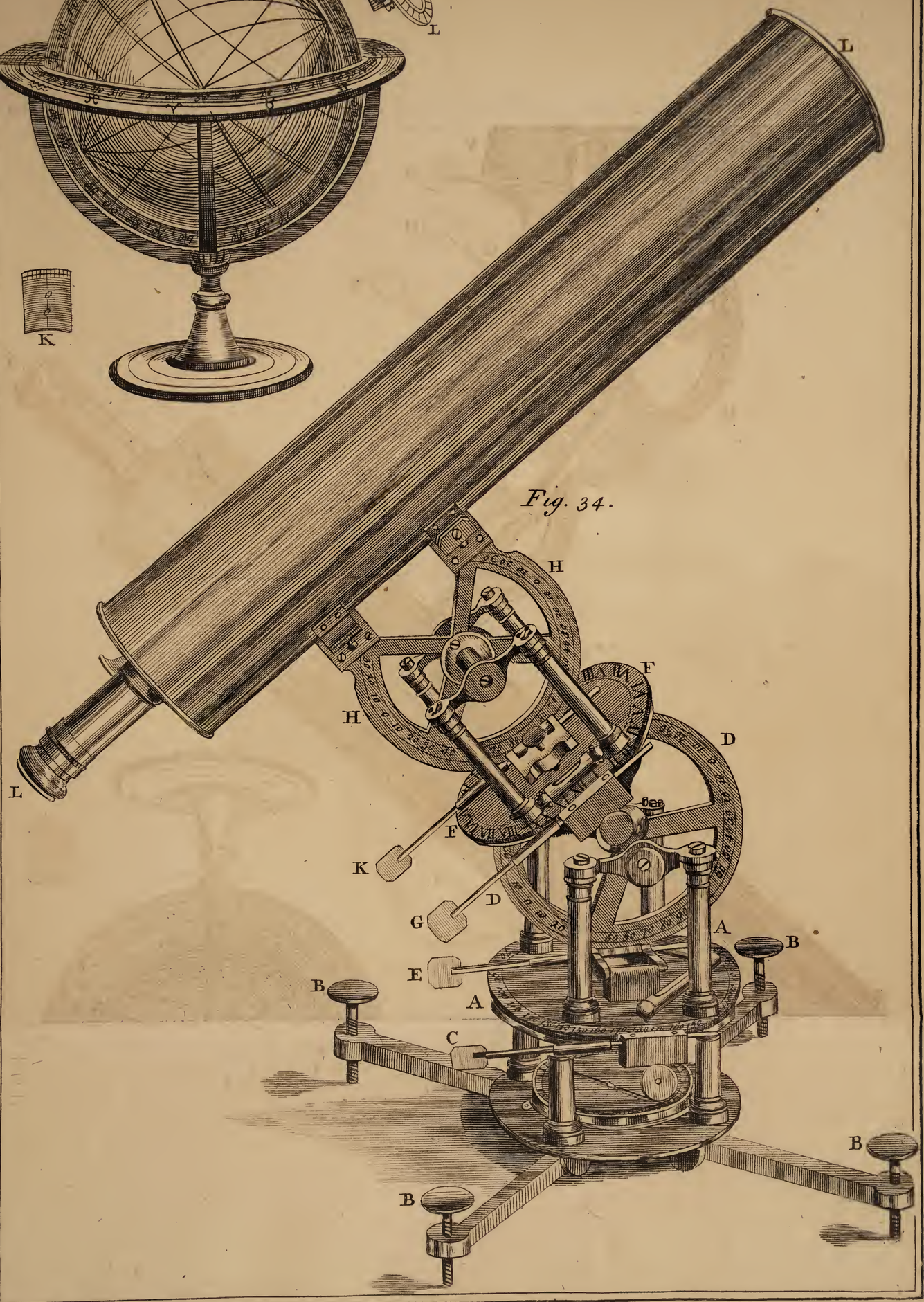


Fig. 34.



may ſerve without any ſenſible error, during the life of any ſingle perſon; whereas mine, particularly the two greateſt, *viz.* of 17 and 28 inches in diameter, have this further advantage, that they ſerve indifferently for any age paſt or to come. For by means of a nut and ſkrew, which will be hereafter deſcribed, the globe is made to turn round an iron axle; whereby the pole of the equator (though fixed in common globes) is made here to revolve about the pole of the ecliptic, and repreſents the ſlow motion *forwards* obſerved among the fixed ſtars, but really owing to the ſlow motion *backwards* of the equinoctial points.

Upon this account it is, that the conſtellation of *Aries* is got into the ſign of *Taurus*, and the conſtellation of *Taurus* into that of *Gemini*, and ſo of the reſt. Hence likewiſe it is, that ſtars which roſe or ſet at particular ſeaſons of the year in the times of *Heſiod*, *Eudoxus*, *Virgil*, *Pliny*, &c. by no means anſwer at this time to their deſcriptions; but by the improvement I am here ſpeaking of, my globes (allowing for the *preceſſion of the equinox*, as it is called, *i. e.* one degree in 72 years) may, without any trouble, be adjuſted to the accounts given by any of thoſe writers.

By this means likewiſe, every one may judge of the truth of ancient obſervations without the labour of a tedious *calculus*, which ſome are not able, and others are not willing, or at leiſure, to go through. By this means likewiſe, ſome paſſages in thoſe ancient writers may be corrected, when manuſcripts afford no aſſiſtance. For theſe frequently ſuffer by the hands they go through, whilſt the Heavens remain invariably the ſame.

As by this apparent motion in the Heavens, not only the *longitudes*, *declinations*, and *right aſcenſions*, of the fixed ſtars are affected, but the poſition of the *colures* is of courſe altered; yet by the help of this contrivance all may be reſtored, and the age of an author in ſome ſort be aſcertained.

The famous aſtronomical argument likewiſe of Sir *I. Newton*, in his *Chron. p.* 86, 87, *&c.* may hereby be more particularly enquired into, and conſidered; all which uſes will be ſpeedily ſhewn and demonſtrated by a regular ſeries of propoſitions, in a treatiſe, as I am well aſſured, that is preparing for the preſs, by the Rev. and Learned Mr *George Coſtard*, Fellow of *Wadham College*, in *Oxford*.

Theſe, Sir, are ſome of the great advantages of my globes over others; and I therefore hope they will merit the encouragement of a Society founded for promoting real and uſeful learning; and that the importation of any globes from abroad may be rendered leſs neceſſary, if not entirely uſeleſs.

PAPERS omitted.

1. A Catalogue of the immerſions and emerſions of the ſatellites of *Jupiter*, that will happen in 1750, by *James Hodgſon*, F. R. S. Maſter of the Royal Mathematical School, in Chriſt's Hoſpital. N° 487. p. 373.

2. The ſame, for the year 1751. N° 493. p. 282.

CHAP.

CHAP. IV.

MECHANICKS, ACOUSTICKS.

The Action of Springs. *In a letter from* James Jurin, *M. D. F. R. S. & Coll. Med.* Lond. *to* M. Folkes, *Esq*; P. R. S. No. 472. p. 46. Jan. *&c.* 1744. *Presented* April 12. 1744.

I. THE theory of ſprings not only is of great uſe in the more curious parts of mechanicks, as the ſtructure of watches, *&c.* but may give light to many operations of nature, there being few ſubſtances but what are endued with ſome degree of elaſticity; and particularly the bodies of animals, and of vegetables likewiſe, being known to conſiſt, in a great meaſure, if not wholly, of organs ſtrongly elaſtic. For which reaſon it is not to be wondered at, that this theory has engaged the thoughts of ſeveral eminent Mathematicians of the laſt and preſent age; as Dr *Hook*, Mr *John Bernouilli*, M. *Camus*, &c. But, as all that I have yet ſeen upon this ſubject goes no further, than to compare the effects of different ſprings one with another in one caſe only, where they are ſuppoſed to be bent to the ſame degree, and that without ſhewing how the effect of any of them may be reduced to, or compared with, that of any other natural cauſe, I flatter myſelf, that the general propoſition I am going to lay down may merit your attention, both on account of its ſimplicity, and of its comprehending all poſſible caſes of a body acting upon a ſpring, or a ſpring upon a body, where no other power intervenes: and alſo of its reducing the effect to that moſt known and ſimple one, the effect of gravity upon falling bodies. In order to which to prevent any miſapprehenſion, it will be proper to fix the meaning of ſuch terms as I ſhall have occaſion to make uſe of.

1. By a ſpring, I mean a body of any ſhape perfectly elaſtic.

2. By the natural ſituation of a ſpring, I mean the ſituation it will reſt in, when not diſturbed by any external force.

3. By the length of a ſpring, I mean the greateſt length, through which it can be forced inwards. This would be the whole lengh, were the ſpring conſidered as a mathematical line; but in a material ſpring is the difference between the whole length when the ſpring is in its natural ſituation, and the length or ſpace it takes up when wholly compreſſed or cloſed.

4. By the ſtrength of a ſpring, I mean the leaſt force or weight, which, when the ſpring is wholly compreſſed or cloſed, will reſtrain it from unbending itſelf.

5. By the ſpace through which a ſpring is bent, I mean that ſpace or length through which one end of the ſpring is removed from its natural ſituation.

6. By the force of a ſpring bent or partly cloſed, I mean the leaſt force or weight, which, when the ſpring is bent through any ſpace leſs than its whole length, will confine it to the ſtate it is then in, without ſuffering it to unbend any farther.

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This being premiſed, I ſhall next, for the foundation of what follows, lay down a principle, which was verified by experiment, in the preſence of our Royal Founder about 70 years ago, by * Dr *Hook*; and has been lately confirmed by the accurate hand of Mr *George Graham*.

PRINCIPLE. *Ut Tenſio, ſic Vis:* That is, if a ſpring be forced or bent inwards, or drawn outwards, or any way removed from it's natural ſituation, it's reſiſtance is proportional to the ſpace by which it is removed from that ſituation.

Thus, if the ſpring *C L*, *Fig.* 36. reſting with the end *L* againſt any immoveable ſupport, but otherwiſe lying in it's nature ſituation, and at full liberty, ſhall, by any force, *p*, be preſſed inwards, or from *C* towards *L*, through the ſpace of one inch, and can be there detained by that force *p*, the reſiſtance of the ſpring, and the force *p*, exactly counterbalancing one another; then the force 2 *p*, will bend the ſpring thro' the ſpace of 2 inches, 3 *p* thro' 3 inches, 4 *p* thro' 4 inches, *&c.* the ſpace *C l*, *Fig.* 37. thro' which the ſpring is bent, or by which the end *C* is removed from it's natural ſituation, being always proportional to the force which will bend it ſo far, and will detain it ſo bent.

And if one end *L* be faſtened to an immoveable ſupport, *Fig.* 38. and the other end *C* be drawn outwards to *l*, and be there detained from returning back by any force *p*, the ſpace *C l*, thro' which it is ſo drawn outwards, will be always proportional to the force *p*, which is able to detain it in that ſituation.

And the ſame principle holds in all caſes, where the ſpring is of any form whatſoever, and is, in any manner whatſoever, forcibly removed from it's natural ſituation.

Here, Sir, I preſume, you will think it material to take notice, that the elaſtick force of the air is a power of a different nature, and governed by different laws, from that of a ſpring. For ſuppoſing the line *L C*, *Fig.* 36. to repreſent a cylindrical volume of air, which, by compreſſion, is reduced to *L l*, *Fig.* 37. or, by dilatation, is extended to *L l*, *Fig.* 38. it's elaſtick force will be reciprocally as *L l*, *Fig.* 36, and 37. whereas the force or reſiſtance of a ſpring will be directly as *C l*.

I now proceed to my general propoſition, and it's corollaries; in which if I ſhall happen at any time to expreſs myſelf with leſs accuracy, as in making weights, times, velocities, *&c.* to become promiſcuouſly the ſubjects of geometrical or arithmetical operations, I deſire, once for all, to be underſtood, not as ſpeaking of thoſe quantities themſelves, but of lines, or numbers, proportional to them.

THEOREM. If a ſpring of the ſtrength *P*, and the length *C L*, *Fig.* 39. lying at full liberty upon a horizontal plane, reſt with one end *L* againſt an immoveable ſupport; and a body of the weight *M*, moving with the velocity *V*, in the direction of the axis of the ſpring, ſtrike directly

* Lectures *de Potentia reſtitutiva*, 1678.

upon the other end C, and thereby force the ſpring inwards, or bend it thro' any ſpace CB; and a middle proportional, CG, be taken between the line $CL \times \frac{M}{P}$, and $2a$, a being the height to which a heavy body would aſcend *in vacuo* with the velocity V; and, upon the radius $R = CG$, be erected the quadrant of a circle GFA; I ſay,

1. When the ſpring is bent thro' any right ſine of that quadrant, as CB, the velocity v of the body M, is, to the original velocity V, as the co-ſine to the radius: that is, $v = V \times \frac{BF}{R}$.

2. The time t of bending the ſpring thro' the ſame ſine CB, is to T the time of a heavy body's aſcending *in vacuo* with the velocity V, as the correſponding arch to $2a$: that is $t = T \times \frac{GF}{2a}$.

DEMONSTRATION. 1. While the ſpring is bending thro' the ſpace CB, let the ſpace, thro' which it is at any time bent, be called x, τ the time of bending it thro' the ſpace x, and v the velocity of the body at the end of the time τ; and let $CL = L$, $CB = l$.

Then, if p be the force, with which the ſpring, when bent thro' the ſpace x, reſiſts the motion of the body; by Dr *Hook*'s principle, $L : x :: P : p = \frac{Px}{L}$.

And ſince, in the caſe of forces that act uniformly, the quantities of motion generated are proportional to the generating forces, and the times jointly, if $M\dot{v}$ be the naſcent quantity of motion taken from the body by the reſiſtance $\frac{Px}{L}$ in the naſcent time $\dot{\tau}$, $MV : -M\dot{v} :: MT :: \frac{Px\dot{\tau}}{L}$ or, $-\dot{v} = \frac{VPx\dot{\tau}}{MLT}$.

Alſo, ſince, in the ſame caſe of forces acting uniformly, the ſpaces are proportional to the velocities, and the times jointly, $x : 2a :: v\tau : VT$, or $\dot{\tau} = \frac{TV\dot{x}}{2av}$.

Therefore, $-\dot{v} = \frac{VPx}{MLT} \times \frac{TV\dot{x}}{2av}$, or, $2v\dot{v} = -\frac{V^2Px\dot{x}}{MLa}$; and the fluents of theſe two quantities are v^2 and $-\frac{V^2Px^2}{2MLa}$. But the former

of

of theſe was V^2, when x, and conſequently, the latter was nothing; therefore $v^2 - V^2 = -\frac{V^2 P x^2}{2MLa}$, or $v^2 = V^2 - \frac{V^2 P x^2}{2MLa}$.

But, by the conſtruction, $\frac{2MLa}{P} = R^2$; therefore, $v^2 = V^2 - \frac{V^2 x^2}{R^2}$, or, $v^2 = V^2 \times \frac{R^2 - x^2}{R^2}$; and, when x becomes equal to l, and v to v, $v^2 = V^2 \times \frac{R^2 - l^2}{R^2}$; and, by the property of the circle, $R^2 - l^2$ being equal to BF^2, $v^2 = V^2 \times \frac{BF^2}{R^2}$, or $v = V \times \frac{BF}{R}$. *Q. E. D.* 1°.

2. We have above, $\dot{\tau} = \frac{\mathcal{T} V \dot{x}}{2av}$; and $v^2 = V^2 \times \frac{R^2 - x^2}{R^2}$; or, $v = V \times \frac{\sqrt{R^2 - x^2}}{R}$: therefore, $\dot{\tau} = \frac{\mathcal{T} V \dot{x}}{2a} \times \frac{R}{V \times \sqrt{R^2 - x^2}}$, or, $\dot{\tau} = \frac{\mathcal{T}}{2a} \times \frac{R\dot{x}}{\sqrt{R^2 - x^2}}$.

Now let CD, *Fig.* 40. be equal to x; and draw the co-ſine DE, the radius CE, and the perpendicular $ed = \dot{x}$: then will the triangle DEC be ſimilar to the naſcent triangle deE; and conſequently, $DE : de :: CE : eE = \frac{CE \times de}{DE} = \frac{R\dot{x}}{\sqrt{R^2 - x^2}}$.

Therefore, $\dot{\tau} = \frac{\mathcal{T}}{2a} \times eE$, and $\tau = \mathcal{T} \times \frac{GE}{2a}$. And when a becomes equal to CB, and τ to t, the arch GE becomes equal to the arch GF: therefore $t = \mathcal{T} \times \frac{GF}{2a}$. *Q. E. D.* 2°.

Whereas I have repreſented the ſpring as reſting againſt an immoveable ſupport at L, it will, perhaps, be objected, that no ſupport can be really immoveable; ſince any body, how great ſoever, may be moved out of it's place by the leaſt force. But this objection may eaſily be removed, by ſuppoſing the ſpring to be continued till it becomes of twice the length CL, and that a ſecond body, equal to M, ſtrikes againſt the oppoſite end of the ſpring with the ſame velocity in a contrary direction; in which caſe the point L will be perfectly immoveable. Scholium I.

Under this theorem are comprehended the 3 following caſes: Scholium II.

In caſe 1. The ſpring is bent thro' it's whole length, or is intirely compreſſed and cloſed, before the moving force of the body is conſumed, and it's motion ceaſes.

In caſe 2. The moving force of the body is conſumed, and it's motion ceaſes, before the ſpring is bent thro' it's whole length, or wholly cloſed.

In caſe 3. The moving force of the body is conſumed, and it's motion ceaſes, at the inſtant that the ſpring is bent thro' it's whole length, and is intirely cloſed.

For this reaſon, and in order to make the following corollaries of more ready uſe, I ſhall take the liberty of diſtributing them into 3 claſſes, the firſt of which are as general as the theorem itſelf, extending to all the 3 caſes, but are more particularly uſeful in caſe 1. The ſecond claſs of corollaries extend to both the ſecond and third caſe; but are more particularly uſeful in caſe 2. The third claſs extend only to caſe 3. and, by that means, are much more ſimple than either of the former.

CLASS I. *General corollaries, but of more particular uſe in* caſe 1; *wherein the ſpring is wholly cloſed, before the motion of the body ceaſes.*

When the ſpring is bent thro' any right ſine CB, *Fig.* 39. the loſs of velocity is to the original velocity, as the verſed ſine to the radius, or $V - v = V \times \frac{Gg}{R}$.

For, ſince $v = V \times \frac{BF}{R}$, $V - v = V - V \times \frac{BF}{R} = V \times \frac{R - BF}{R} = V \times \frac{Gg}{R}$.

Coroll. 1.

Coroll. 2. When the ſpring is bent thro' any right ſine CB, the diminution of the ſquare of the velocity is to the ſquare of the original velocity, as the ſquare of that right ſine to the ſquare of the radius, or $V^2 - v^2 = V^2 \times \frac{CB}{R^2}$.

For, ſince $v = V \times \frac{BF}{R}$, $v^2 = V^2 \times \frac{BF^2}{R^2}$, and $V^2 - v^2 = V^2 - V^2 \times \frac{BF^2}{R^2} = V^2 \times \frac{R^2 - BF^2}{R^2} = V^2 \times \frac{CB^2}{R^2}$.

Coroll. 3. When the ſpring is bent thro' any ſpace l, v the velocity of the body is equal to $V \times \sqrt{\frac{2MLa - Pl^2}{2MLa}}$, or to $V \times \sqrt{\frac{2Ma - pl}{2Ma}}$; and is proportional to $\sqrt{\frac{2MLa - Pl^2}{ML}}$, or to $\sqrt{\frac{2Ma - pl}{M}}$.

For, ſince $v^2 = V^2 \times \frac{BF^2}{R^2} = V^2 \times \frac{R^2 - l^2}{R^2}$; if, for R^2, we ſubſti-

tute

tute it's value $\frac{2MLa}{P}$, we have $v^2 = V^2 \times \frac{2MLa - Pl^2}{2MLa}$, or $v = V \times \sqrt{\frac{2MLa - Pl^2}{2MLa}}$: and, as by Dr *Hook*'s principle, $L : l :: P : p$, or $Pl = pL$, $v = V \times \sqrt{\frac{2MLa - pLl}{2MLa}}$, or, $v = V \times \sqrt{\frac{2Ma - pl}{2Ma}}$.

But $\frac{V}{\sqrt{a}}$, by *Galileo*'s doctrine, is a conſtant quantity; and therefore v is proportional to $\sqrt{\frac{2MLa - Pl^2}{ML}}$, or to $\sqrt{\frac{2Ma - pl}{M}}$.

The time t of bending the ſpring thro' any ſpace l, is proportional to the arch GF divided by $\sqrt{a}$; l being the right ſine of the arch, and $R = \sqrt{\frac{2MLa}{P}}$, being the radius. *Coroll.* 4.

For, by the theorem, $t = T \times \frac{GF}{2a}$; and $\frac{T}{\sqrt{a}}$ is a conſtant quantity.

The diminution of the product of the weight of the body into the ſquare of the velocity, or (to uſe the expreſſion of ſome late writers) the diminution of the *Vis viva*, that is, $MV^2 - Mv^2$, by bending a ſpring thro' any ſpace l, is always equal to $\frac{C^2 Pl^2}{2LA}$, or to $\frac{C^2 pl}{2A}$; where A is the height from which a heavy body will fall *in vacuo* in a ſecond of time, and C is the celerity gained by that fall. *Coroll.* 5.

For, by *Coroll.* 2. $V^2 - v^2 = V^2 \times \frac{CB^2}{R^2} = \frac{V^2 l^2}{R^2}$; and R^2, by the conſtruction, being equal to $\frac{2MLa}{P}$, $V^2 - v^2 = V^2 l^2 \times \frac{P}{2MLa}$.

But, by *Galileo*'s theory, $\frac{V^2}{a} = \frac{C^2}{A}$; therefore, $V^2 - v^2 = \frac{C^2 Pl^2}{2MLA}$ and $MV^2 - Mv^2 = \frac{C^2 Pl^2}{2LA} = \frac{C^2 pl}{2A}$.

The diminution of the *Vis viva*, by bending a ſpring thro' any ſpace l, is always proportional to $\frac{Pl^2}{L}$, or to pl: and, if either the ſpring be given, or $\frac{P}{L}$ be given in different ſprings, the loſs of the *Vis viva* will be as l^2, or as p^2. *Coroll.* 6.

For,

For, by *Coroll.* 5. $MV^2 - Mv^2 = \frac{C^2 Pl^2}{2LA} = \frac{C^2 pl}{2A}$; and $\frac{C^2}{A}$ being a conſtant quantity, $MV^2 - Mv^2$ is as $\frac{Pl^2}{L} = pl$: And, if $\frac{P}{L}$ be given, $MV^2 - Mv^2$ will be as l^2; or as $l^2 \times \frac{P^2}{L^2}$; or as $l^2 \times \frac{p^2}{l^2}$; or as p^2.

Coroll. 7. The loſs of the *Vis viva*, by bending a ſpring through it's whole length, or by wholly cloſing it, is equal to $\frac{C^2 PL}{2A}$, and is proportional to PL: and, if PL be given, the loſs of the *Vis viva* is always the ſame.

This is evident from *Coroll.* 5. and 6.; foraſmuch as l is now equal to L.

CLASS II. *Corollaries of more particular uſe in* Caſe 2. *wherein the motion of the body ceaſes before the ſpring is wholly cloſed.*

If the motion of the body ceaſe when the ſpring is bent through any ſpace l, the initial velocity V is equal to $Cl\sqrt{\frac{P}{2MLA}}$, or to $C\sqrt{\frac{pl}{2MA}}$.

Coroll. 8. For, by *Coroll.* 5. $V^2 - v^2 = \frac{C^2 Pl^2}{2MLA} = \frac{C^2 pl}{2MA}$. And here, the motion of the body ceaſing, $v^2 = 0$. Therefore $V^2 = \frac{C^2 Pl^2}{2MLA} = \frac{C^2 pl}{2MA}$; or $V = Cl\sqrt{\frac{P}{2MLA}} = C\sqrt{\frac{pl}{2MA}}$.

Coroll. 10. If the motion of the body ceaſe, when the ſpring is bent through any ſpace, l, the time, t, of bending it, is equal to $1''$ of time, multiplied by $\frac{m}{2}\sqrt{\frac{ML}{2PA}}$, or to $1'' \times \frac{m}{2}\sqrt{\frac{Ml}{2pA}}$, where m is to 1, as the circumference of a circle to the diameter.

For, by the theorem, $t = \mathcal{T} \times \frac{GF}{2a}$; and, by *Galileo*'s theory, $\frac{\mathcal{T}}{\sqrt{a}} = \frac{1''}{\sqrt{A}}$. Therefore $t = \frac{1''}{\sqrt{A}} \times \frac{GF}{2\sqrt{a}}$.

Alſo, by the theorem, $v^2 = V^2 \times \frac{R^2 - l^2}{R^2}$; and therefore v^2 being now equal to 0, $R^2 = l^2$, and, *Fig.* 41. l becomes the radius of the circle; and l being likewiſe equal to the right ſine of the arch GF, that arch becomes a quadrant, and is equal to $\frac{2l \times m}{4}$. Therefore $t = \frac{1''}{\sqrt{A}} \times \frac{2lm}{4 \times 2\sqrt{a}}$, or $t = 1'' \times \frac{lm}{4\sqrt{A} \times \sqrt{a}}$.

Fig. 41.

But

But l being equal to $R = \sqrt{\frac{2MLa}{P}}$, $\frac{l}{\sqrt{a}} = \sqrt{\frac{2ML}{P}}$; therefore $t = 1'' \times \frac{m}{4\sqrt{A}} \times \sqrt{\frac{2ML}{P}}$, or, $t = 1'' \times \frac{m}{2}\sqrt{\frac{ML}{2PA}} = 1'' \times \frac{m}{2}\sqrt{\frac{Ml}{2pA}}$.

In the same case, the time of bending the spring is proportional to $\sqrt{\frac{ML}{P}}$, or to $\sqrt{\frac{Ml}{p}}$; and if $\frac{L}{P}$ be given, t will be as $\sqrt{M}$; and, if both $\frac{L}{P}$, and also M, be given, t will always be the same, whatever be the original velocity, or through whatever space the spring be bent. *Coroll.* 11.

If the motion of the body cease, when the spring is bent through any space l, the product of the initial velocity, and the time of bending the spring, or Vt, is equal to $1'' \times \frac{mCl}{4A}$; and is proportional to l, the space through which the spring is bent. *Coroll.* 12.

For, by *Coroll.* 8. $V = Cl\sqrt{\frac{P}{2MLA}}$, and, by *Coroll.* 9. $t = 1'' \times \frac{m}{2}\sqrt{\frac{ML}{2PA}}$; therefore, $Vt = 1'' \times \frac{mCl}{4A}\sqrt{\frac{MLP}{MLP}} = 1'' \times \frac{mCl}{4A}$; and, as $1''$, m, C and A, are given quantities, Vt is as l.

Hence, any two of the three quantities, V, t, and l, being given, the other is readily determined.

In the same case, the initial quantity of motion, or MV, is equal to $Cl\sqrt{\frac{PM}{2LA}}$, or to $C\sqrt{\frac{plM}{2A}}$. *Coroll.* 13.

For, by *Coroll.* 8. $V = Cl\sqrt{\frac{P}{2MLA}} = C\sqrt{\frac{pl}{2MA}}$; wherefore $MV = Cl\sqrt{\frac{PM}{2LA}} = C\sqrt{\frac{plM}{2A}}$.

In the same case, MV is proportional to $l\sqrt{\frac{PM}{L}}$, or to $\sqrt{plM}$, or to $\frac{Plt}{L}$, or to pt: And, if $\frac{P}{L}$ be given, MV is as $l\sqrt{M}$, or as lt. *Coroll.* 14.

For, in the preceding *Coroll.* $\frac{C}{\sqrt{A}}$ is a given quantity; and, by *Coroll.* 11. t is as $\sqrt{\frac{ML}{P}} = \sqrt{\frac{ML}{p}}$.

If

Coroll. 15. If the quantity of motion MV bend a ſpring of the ſtrength P, and length L, through the ſpace l, and be wholly conſumed thereby, no different quantity of motion equal to the former, as $nM \times \frac{V}{n}$, will bend the ſame ſpring through the ſame ſpace, and be wholly conſumed thereby.

For, by the preceding *Coroll.* if the ſpring be bent through the ſpace l, and each of theſe quantities of motion be conſumed thereby; $l\sqrt{M} : l\sqrt{nM} :: MV : nM \times \frac{V}{n}$. But $MV = nM \times \frac{V}{n}$; and therefore, $l\sqrt{M} = l\sqrt{n}$ M, or $1 = n$, and $M = nM$, and $V = \frac{V}{n}$. Therefore the quantity of motion $nM \times \frac{V}{n}$ is not only equal to MV, but is compoſed of an equal maſs, and an equal velocity.

Coroll. 16. But a quantity of motion leſs than MV, in any given *ratio*, may bend the ſame ſpring through the ſame ſpace l, and the time of bending it will be leſs in the ſame given *ratio*.

For, let 1 to n be the given *ratio*; and let the leſſer quantity of motion be $\frac{M}{nn} \times nV$; which is to MV, as 1 to n. Then, by *Coroll.* 14. the ſpring being given, $l\sqrt{M} : l\sqrt{\frac{M}{nn}} :: MV : \frac{M}{nn} \times nV = \frac{MV}{l\sqrt{M}} \times l\sqrt{\frac{M}{nn}} = \frac{MV}{n}$. Therefore the quantity of motion $\frac{M}{nn} \times nV$, being equal to $\frac{MV}{n}$, will bend the ſpring through the ſame ſpace l.

Likewiſe, by the ſame *Coroll.* MV is as lt; and l being given, the quantity of motion is as t: Therefore the time of bending the ſpring will be leſs in the ſame *ratio*, as the quantity of motion is leſs.

Coroll. 17. A quantity of motion greater than MV, in any *ratio* given, may be conſumed in bending the ſpring through the ſame ſpace; and the time of bending it will be greater in the ſame given *ratio*.

This appears after the ſame manner as the preceding, by making n a fractional number inſtead of a whole one.

Coroll. 18. If the motion of the body ceaſe, when the ſpring is bent through any ſpace l, the initial *Vis viva*, or MV^2, is equal to $\frac{C^2 P l^2}{2LA}$, or to $\frac{C^2 pl}{2A}$: and $2aM = \frac{Pl^2}{L} = pl$.

For, by *Coroll.* 8. $V = Cl\sqrt{\frac{P}{2MLA}} = C\sqrt{\frac{pl}{2MA}}$, or $V^2 = \frac{C^2 l^2 P}{2MLA}$ $= \frac{C^2 pl}{2MA}$: therefore $MV^2 = \frac{C^2 Pl^2}{2LA} = \frac{C^2 pl}{2A} = \frac{V^2 P^2 l^2}{2La} = \frac{V^2 pl}{2a}$.

In

In the ſame caſe, the initial *Vis viva* is proportional to $\frac{Pl^2}{L} = pl$ and *Coroll.* 19.

if $\frac{P}{L}$ be given, the *Vis viva* is as l^2, or as p^2.

For, in the preceding Coroll. $\frac{C^2}{A}$ being a given quantity, the *Vis viva*

is as $\frac{Pl^2}{L} = pl$; and, if $\frac{P}{L}$ be given, it will be as l^2, or as p^2; for-

aſmuch as p and l increaſe in the ſame proportion.

If the *Vis viva*, MV^2, bend a ſpring thro' the ſpace l, and be *Coroll.* 20.
totally conſumed thereby, any other *Vis viva*, equal to the former, as

$nnM \times \frac{V^2}{nn}$, will bend the ſame ſpring thro' the ſame ſpace, and be

totally conſumed thereby.

For, the ſpring being the ſame, $\frac{P}{L}$ is given; and therefore by *Coroll.*

19. the *Vis viva*, which will be conſumed in bending the ſpring thro'
the ſpace l, is as l^2.

But the time, in which the ſame ſpring will be bent thro' the *Coroll.* 21.
ſame ſpace, by the *Vis viva* $nnM \times \frac{V^2}{nn}$, will be to the time, in which

it is ſo bent by the *Vis viva* $M \times V^2$, as n to 1; n being any whole
or fractional number.

For, by *Coroll.* 11. ſince $\frac{L}{P}$ is given, the time is as VM.

If the motion of the body ceaſe, when the ſpring is bent thro' it's CLASS III.
whole length, or is wholly cloſed, the initial velocity V is equal to *Corollaries in caſe 3. wherein the motion of the body ceaſes, at the inſtant that the ſpring is wholly cloſed.*
$C\sqrt{\frac{PL}{2MA}}$.

For, by *Coroll.* 8. $V = C\sqrt{\frac{pl}{2MA}}$; and l being now equal to L *Coroll.* 22.

Fig. 42. p becomes equal to P; and therefore $V = C\sqrt{\frac{PL}{2MA}}$.

In the ſame caſe, the initial velocity V is proportional to $\sqrt{\frac{PL}{M}}$. *Coroll.* 23.

For $\frac{C}{\sqrt{A}}$, in the preceding *Coroll.* is a given quantity.

Coroll. 24. In the ſame caſe, if PL be given, either in the ſame, or in different ſprings, the initial velocity V is reciprocally as $\sqrt{M}$.

This is plain from the preceding *Coroll.*

Coroll. 25. If the motion of the body ceaſe, when the ſpring is wholly cloſed, the product of the initial velocity, and the time ſpent in cloſing the ſpring, or Vt, is equal to $1'' \times \frac{mCL}{4A}$; and is proportional to L, the length of the ſpring.

For, by *Coroll.* 22. $V = C\sqrt{\frac{PL}{2MA}}$; and, by *Coroll.* 10. $t = 1'' \times \frac{m}{2}\sqrt{\frac{ML}{2PA}}$: therefore, $Vt = 1'' \times \frac{mCL}{4A}$; and $1''$, m and $\frac{C}{A}$, being given quantities, Vt is as L.

Coroll. 26. In the ſame caſe, the initial quantity of motion, or MV, is equal to $C\sqrt{\frac{PLM}{2A}}$.

For, by *Coroll.* 23. $V = C\sqrt{\frac{PL}{2MA}}$.

Coroll. 27. In the ſame caſe, MV is proportional to $\sqrt{PLM}$, or to Pt: and, if PL be given, either in the ſame, or different ſprings, MV is as $\sqrt{M}$.

This appears, partly, from the preceding *Coroll.* where $\frac{C}{\sqrt{A}}$ is a given quantity; and, conſequently, MV is as $\sqrt{PLM}$; and PL being given, MV is as $\sqrt{M}$: and, partly, from *Coroll.* 11. where t is as $\sqrt{\frac{ML}{P}}$, and, conſequently, Pt is as $\sqrt{PLM}$.

Coroll. 28. In the ſame caſe, if $\frac{P}{L}$ be given, either in the ſame, or in different ſprings, the initial quantity of motion is as the length of the ſpring into the time of bending it.

For, by *Coroll.* 27. MV is as Pt; and, if P be as L, MV is as Lt.

Coroll. 29. If the quantity of motion MV bend a ſpring thro' it's whole length, and be conſumed thereby, no other quantity of motion equal to the former, as $nM \times \frac{V}{n}$, will cloſe the ſame ſpring, and be wholly conſumed thereby.

This is proved in the ſame manner as *Coroll.* 15. putting only L for l.

Coroll. 30. But a quantity of motion leſs or greater than MV, in any given *ratio*, may cloſe the ſame ſpring, and be wholly conſumed in cloſing it: and

the

the time ſpent in cloſing the ſpring will be reſpectively leſs or greater, in the ſame given *ratio*.

This is eaſily proved from *Coroll.* 16.

If the motion of the body ceaſe, when the ſpring is wholly cloſed, the initial *Vis viva*, or MV^2, is equal to $\frac{C^2PL}{2A}$: and $2aM = PL$. *Coroll.* 31.

For, by *Coroll.* 22. $V = C\sqrt{\frac{PL}{2MA}}$, or $V^2 = \frac{C^2PL}{2MA}$, or $MV^2 = \frac{C^2PL}{2A} = \frac{V^2PL}{2a}$.

In the ſame caſe, the initial *Vis viva* is as the rectangle under the ſtrength and length of the ſpring. *Coroll.* 32.

For, by the preceding *Coroll.* $MV^2 = \frac{C^2PL}{2A}$, and $\frac{C^2}{A}$ is a given quantity; wherefore MV^2 is as PL.

In the ſame caſe, if $\frac{P}{L}$ be given, the initial *Vis viva* is as P^2, or as L^2. *Coroll.* 33.

This is evident from the preceding *Coroll.*

If the *Vis viva* MV^2 bend a ſpring thro' it's whole length, and be conſumed in cloſing it, any other *Vis viva* equal to the former, as nn $M \times \frac{V^2}{nn}$, will cloſe the ſame ſpring, and be conſumed thereby. *Coroll.* 34.

This is evident from *Coroll.* 32.

But the time of cloſing the ſpring by the *Vis viva* $nn\,M \times \frac{V^2}{nn}$, will be to the time of cloſing it by the *Vis viva* MV^2, as n to 1. *Coroll.* 35.

For, by *Coroll.* 11. ſince the ſpring is given, the time is as $\sqrt{M}$.

If the *Vis viva* MV^2 be wholly conſumed in cloſing a ſpring of the ſtrength P, and length L; the *Vis viva*, $nn\,MV^2$, will be ſufficient to cloſe, *Coroll.* 36.

1. Either a ſpring of the ſtrength nnP, and length L.
2. Or a ſpring of the ſtrength nP, and length nL.
3. Or of the ſtrength P, and length nnL.
4. Or, if n be a whole number, the number nn of ſprings, each of the ſtrength P, and length L, one after another.

For, $MV^2 : nn\,MV^2 :: PL : nn\,PL$; and therefore, by *Coroll.* 32. the *Vis viva*, $nn\,MV^2$, will cloſe any ſpring that has $nn\,PL$ for the product of it's ſtrength and length. But $nn\,PL$ is compoſed either of $nnP \times L$, or of $nP \times nL$, or of $P \times nnL$.

Alſo the loſs of the *Vis viva*, in bending a given ſpring, being always the ſame, by *Coroll.* 7. and the *Vis viva*, MV^2 being wholly loſt

in bending a ſingle ſpring PL; the loſs of the *Vis viva*, $nnMV^2$, in cloſing one ſuch ſpring, will be MV^2; and it's loſs in cloſing a ſecond ſuch ſpring, will again be MV^2, and ſo on: conſequently, the number nn of ſuch ſprings will be cloſed one after another, by that time the *Vis viva*, $nnMV^2$, is wholly conſumed.

Scholium III.

If the ſpring, inſtead of being at firſt wholly unbent, as we have hitherto conſidered it, be now ſuppoſed to have been already bent through ſome ſpace CB, before the body ſtrikes it; and the velocity of the body be required, after the ſpring is bent through any further ſpace, BD, *Fig.* 43. this caſe, as well as the three other above-mention'd, will be found to come under our theorem.

For, if v be the velocity with which the body is ſuppoſed to ſtrike againſt the bent ſpring at B, it is evident, that this may be conſidered, either as the original velocity, or as the remainder of a greater velocity V, with which the body might have ſtruck upon the ſpring at C, and which, upon bending the ſpring from C to B, would now be reduced to v. For, in either caſe, the effect in bending the ſpring from B to D, will be exactly the ſame.

In order, therefore, to determine this imaginary velocity V, let a middle proportional, BF, be taken between $CL \times \frac{M}{P}$, and 2α, α being the height to which a body will aſcend *in vacuo* with the velocity v; draw BF prependicular to CB, and, with the radius CF, deſcribe the quadrant $CGFEA$. Then will our preſent caſe be exactly reduced to that of the theorem; CB, CD, repreſenting the ſpaces through which the ſpring is bent; BF and DE the velocities in the points B and D; GF and GE the times of bending the ſpring through the ſpaces CB, CD; and CG repreſenting the imaginary velocity V, with which the body might have ſtruck the ſpring at C.

For, by the theorem, $BF^2 : CG^2 :: v^2 : V^2$; and $v^2 : V^2 :: \alpha : a$. Therefore $CG^2 = BF^2 \times \frac{a}{\alpha}$. But $BF^2 = 2\alpha \times \frac{LM}{P}$, by the conſtruction; and, conſequently, $CG^2 = \frac{2\alpha LM}{P} \times \frac{a}{\alpha} = \frac{2aLM}{P}$, as in the conſtruction of the theorem.

From this caſe we ſhall draw a few corollaries, as well for their uſefulneſs upon other occaſions, as to ſhew how the theory of ſprings may be ſafely applied to the action of gravity upon aſcending or falling bodies.

Coroll. 37. If the body M, with the velocity v, ſufficient to carry it to the height α, ſtrike at B, upon a ſpring already bent through the ſpace $CB = l$; and do thereby bend it through ſome farther ſpace $BD = s$; at the end of which ſpace, or at D, the body has a velocity ſufficient to carry it to ſome height, as ε; then $\varepsilon = \frac{2\alpha ML - Ps \times 2l + s}{2ML}$.

For,

For, by the theorem, $\alpha : \varepsilon :: BF^2 : DE^2$, or $DE^2 = BF^2 \times \frac{\varepsilon}{\alpha} = \frac{2\alpha ML}{P} \times \frac{\varepsilon}{\alpha}$ or $DE^2 = \frac{2\varepsilon ML}{P}$.

Alſo, $DE^2 + CD^2 = CE^2 = CF^2 = BF^2 + CB^2$, that is, $\frac{2\varepsilon ML}{P} + l^2 + 2ls + s^2 = \frac{2\alpha ML}{P} + l^2$; or $\frac{2\varepsilon ML}{P} = \frac{2\alpha ML}{P} - 2ls - s^2$; or $2\varepsilon ML = 2\alpha ML - Ps \times \overline{2l + s}$.

If the motion of the body ceaſe upon bending the the ſpring through the ſpace $BD = s$, that is, if $\varepsilon = o$; then the height to which the body might aſcend *in vacuo*, with the velocity v, or $\alpha = \frac{Ps \times \overline{2l + s}}{2ML}$. *Coroll.* 38.

For, by the laſt, when $\varepsilon = o$, $2\alpha ML = Ps \times \overline{2l + s}$.

If p, the force of the ſpring when bent through the ſpace CB, be equal to M, the weight of the body; the height to which the body would aſcend *in vacuo* with the velocity v, is to the ſpace through which it will bend the ſpring, by ſtriking upon it at B with that ſame velocity, as $2l + s$ to $2l$, or $\alpha : s :: 2l + s : 2l$. *Coroll.* 39.

For, by the laſt, $\alpha = \frac{Ps \times \overline{2l + s}}{2ML}$; and $\frac{P}{L}$ being equal to $\frac{p}{l}$, $\alpha = \frac{ps \times \overline{2l + s}}{2Ml}$; and, if $p = M$, $\alpha = s \times \frac{\overline{2l + s}}{2l}$.

If $p = M$, and p do alſo continue conſtantly the ſame while the ſpring is bending from B to D (both which ſuppoſitions are neceſſarily made in reducing the action of a ſpring to that of gravity upon an aſcending body), the ſpring muſt be of an infinite length; and l, the ſpace through which it was bent before the body ſtruck it, muſt alſo be of an infinite length; and the ſpace BD, through which the ſpring will be further bent, muſt be equal to the height the body can aſcend to with the velocity v, or $\alpha = s$. *Coroll.* 40.

For, by the laſt, when $p = M$, $\alpha : s :: 2l + s : 2l$; and the reſiſtances of the ſpring at D and B being reſpectively as CD and CB, that is, as $l + s$ and l; ſince thoſe reſiſtances are now ſuppoſed equal to one another, we muſt, upon that ſuppoſition, conſider $l + s$ as equal to l; and adding l to each, $2l + s = 2l$, that is, l muſt be infinitely greater than s; and then $\alpha : s :: 2l : 2l$, or $\alpha = s$.

In this propoſition, and all it's corollaries, except the 4 laſt, we have conſidered the ſpring as being, at firſt, wholly unbent, and then acted upon by a body moving with the velocity V, which bends it through ſome certain ſpace: But, as we ſuppoſe the ſpring to be perfectly elaſtic, the propoſition and corollaries will equally hold, if the ſpring be ſuppoſed SCHOLIUM IV.

to

to have been, at first, bent through that same space, and, by unbending itself, to press upon a body at rest, and thereby to drive that Body before it, during the time of it's expansion: Only, V, instead of being the initial velocity, with which the body struck the spring, will now be the final velocity, with which the body parts from the spring when wholly expanded.

Scholium V. If the spring, instead of being pressed inwards, be drawn outwards by the action of the body, we need only make L the greatest length to which the spring can be drawn out beyond it's natural situation, without prejudice to it's elasticity, l any lesser length to which the spring is drawn outwards, P and p the forces, which will keep it from restoring itself when drawn out to those lengths respectively, and the proposition will equally hold good: as it will also, if the spring be supposed to have been already drawn outwards to the length l, and, in restoring itself, to draw the body after it: only, in this latter case, V, the initial velocity in the proposition, will now be the final velocity, as in *Scholium* IV.

Scholium VI. Our proposition equally holds good, when the spring is of any form whatsoever, provided L be always understood to be the greatest length it can be bent or drawn to from it's natural situation, l any lesser length, and P, p, the forces which will confine it to these lengths. For Dr *Hook*'s principle extends to springs of any form.

I have been at the trouble of drawing so great a number of corollaries from this proposition, because, in the controversy about the force of bodies in motion, I have observed both parties to support their opinion by arguments taken from the theory of springs; and I was willing impartially to furnish them both with means to examine into the truth or falsehood of one another's reasonings. I had thoughts myself of making use of some of these corollaries for that purpose, being far from thinking that the dispute is about words only; but this letter is already drawn out to too great a length; and before I have leisure to write again, I may possibly be prevented by a better hand, which, I hope, may put an end to a dispute thas has too long pester'd the Learned World.

An inquiry into the Measure *of the Force of Bodies in* Motion: *with a proposal of an* Experimentum Crucis, *to decide the Controversy about it; by the same.* No. 476. p. 423. April, &c. 1741. *Read* May 30. 1745.

II. Mechanical forces may be reduced to two sorts; one of a body at rest, the other of a body in motion. The force of a body at rest, is that of a body lying still upon a table, or hanging by a rope, or supported upon a spring, *&c.* This is called by the name of pressure, tension, force, or *vis mortua.*

The *measure* of this force is the weight with which the table is pressed, or the rope is stretched, or the spring is bent. And that *measure* being acknowledged by all writers, there is no dispute about this sort of force, notwithstanding the diversity of appellations by which it is called.

The force of a body in motion is on all hands agreed to be a power residing in that body, so long as it continues it's motion; by means of which it is able to remove obstacles lying in it's way; to lessen, destroy, or overcome, the force of any other moving body, which meets it in an opposite

Fig. 36.

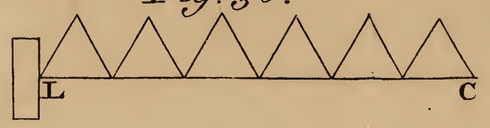


Fig. 37.

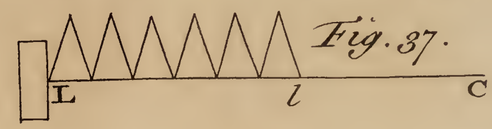


Fig. 38.

Fig. 40.

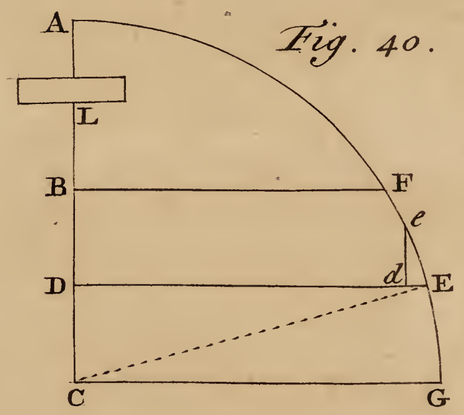


Fig. 39.

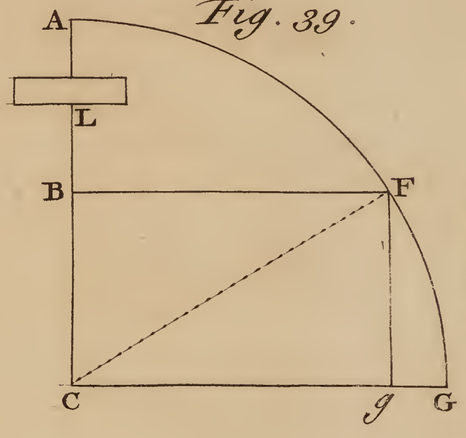


Fig. 41.

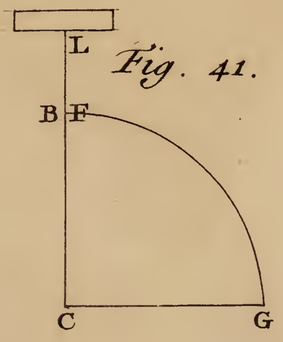


Fig. 42.

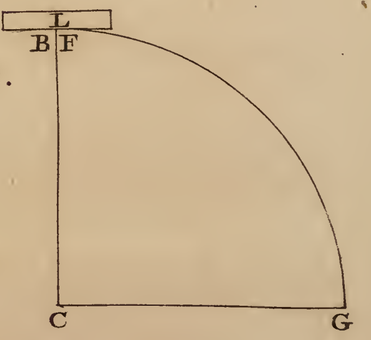
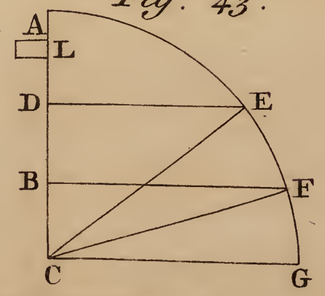


Fig. 43.



oppoſite direction; or to ſurmount any dead preſſure or reſiſtance, as tenſion, gravity, friction, *&c.* for ſome time; but which will be leſſened or deſtroyed by ſuch obſtacles, or by ſuch reſiſtance, as leſſens or deſtroys the motion of the body. This is called moving force, *vis motrix*, and by ſome late writers, *vis viva*, to diſtinguiſh it from the *vis mortua* ſpoken of before: and by theſe appellations, however different, the ſame thing is underſtood by all Mathematicians; namely, That power of diſplacing obſtacles, withſtanding oppoſite moving forces, or overcoming any dead reſiſtance, which reſides in a moving body, and which, in whole or in part, continues to accompany it, ſo long as the body moves.

But about the *meaſure* of this ſort of force, mathematicians are divided into two parties: And, in order to ſtate the caſe fairly between them, it will be neceſſary to ſhew how far the two parties agree, and in what point their diſagreement conſiſts.

Both ſides agree, That the *meaſure* of this force depends partly upon the maſs, or weight, of the body, and partly upon the velocity with which it moves; ſo that, upon any increaſe either of the weight, or of the velocity, the moving force will become greater. They alſo agree, That if the velocity continue the ſame, but the maſs, or weight of the body, be increaſed in any proportion, the moving force is increaſed in the ſame proportion: ſo that, in this caſe, the *meaſure* of the moving force is the ſame with that of the weight: or, when two bodies move with the ſame velocity, if the weight of the ſecond be double, triple, quadruple, of that of the firſt, the moving force of the ſecond will alſo be double, triple, quadruple, of that of the firſt. But, when two bodies are equal, and the velocities with which they move are different, the two parties no longer agree about the *meaſure* of the moving force.

One ſide maintains, That, when the velocity of the ſecond body is double, triple, quadruple of that of the firſt, the *meaſure* of the moving force of the ſecond is alſo double, triple, quadruple, of that of the moving force, being the ſame with that of the velocity.

The other ſide pretend, That, in the ſame caſe, the moving force of the ſecond body is four times, nine times, ſixteen times, as great as that of the firſt; the *meaſure* of the moving force being the ſame with that of the ſquare of the velocity.

In conſequence of the agreement in the firſt of theſe two caſes, and the diſagreement in the ſecond, the one ſide pretends, That the *meaſure* of the moving force is, in all caſes, the product of the weight into the velocity; and the other, That it is the product of the weight into the ſquare of the velocity.

This controverſy was firſt ſtarted by the famous Mr *Leibnitz*, and has been carried on by him and his followers for near 60 years; during which time a great number of pieces have been publiſhed on both ſides of the queſtion, and a great number of experiments have been made, or propoſed to be made, in order to decide it. But though both parties agree in the event of the experiments, whether actually made, or only propoſed; yet

as the writers on each ſide have found a way of deducing from thoſe experiments a concluſion ſuitable to their own opinion, the diſagreement ſtill continues as wide as ever, to the great ſcandal of the learned world.

Now, if we examine carefully into the reaſon of this, and would ſee by what means it happens, that two oppoſite concluſions are ſo often drawn from the ſame experiment, we ſhall find it not ſo much owing to falſe reaſoning on either ſide, (that would be eaſily detected, and ſet right), as to another cauſe; namely, to their diſagreement in the principles upon which the reaſoning is founded.

For, whereas whatever is laid down on either ſide as a principle, ought to be ſomething all the world agrees in, at leaſt what is admitted by the other party; without which, all reaſoning upon it is to no purpoſe; this conduct has been ſo little obſerved in the preſent diſpute, that what has been offered on the one ſide as an undoubted principle or axiom, has commonly been ſomething that the oppoſite party does not admit, nay, even abſolutely denies.

Of this it were eaſy to produce a number of examples; but I ſhall content myſelf with two only, one taken from each ſide.

Thoſe who maintain, That the moving force is as the weight into the velocity, lay down for a principle, or axiom, That when two bodies meet one another in contrary directions, if their moving forces be equal, neither body will prevail over the other: and if their moving forces be unequal, the ſtronger will always prevail over the weaker.

This the *Leibnitian* party deny. They maintain, That one of theſe bodies may prevail over the other, though their moving forces be equal: nay, that, in many caſes, the weaker will prevail over the ſtronger.

It is therefore to no purpoſe to alledge, That the principle above laid down is founded on common ſenſe; or that it was always univerſally received, till this diſpute began: for, ſince the oppoſite party now reject it, all reaſoning upon it can have no weight with them; you muſt have recourſe to ſomething elſe.

On the other hand, thoſe who adhere to Mr *Leibnitz*'s ſentiment, lay down for a principle, That equal effects always ariſe from equal cauſes; provided the cauſes be intirely conſumed in producing thoſe effects.

This their opponents do not admit, unleſs in the caſe where thoſe equal effects are produced in equal times; and therefore, till both ſides ſhall agree in admitting this principle, no argument can be drawn from it by one party, that will be of any ſervice to convince the other.

But as this principle is chiefly made uſe of in reaſoning upon experiments made with ſprings, many of which have been produced by both parties, in ſupport of their opinions, it may be worth while more particularly to conſider, What right there is on the one ſide to impoſe this principle, and what reaſons may be given on the other for rejecting it.

When one end of a ſpring, wholly unbent, leans againſt an immoveable ſupport, and the oppoſite end is ſtruck upon by a body in motion, which, bending the ſpring to ſome certain degree, does thereby loſe it's whole

whole moving force; the moving force of the body may be conſidered as the cauſe of bending the ſpring; and the bending of the ſpring may be looked upon as the effect of that cauſe, which is wholly ſpent and conſumed in producing it.

Now if two unequal bodies, moving with unequal velocities, ſtrike in this manner upon two equal ſprings, and each of them bend the ſpring it ſtrikes upon, exactly to the ſame degree; and by ſo doing, the moving force of each body be intirely conſumed; here, ſay the *Leibnitzian* writers, are two equal effects produced; for the ſprings are equal, and are now equally bent; and the moving forces, which are the cauſes of thoſe effects, are wholly conſumed in producing them; and therefore, by virtue of the principle above laid down, thoſe cauſes muſt be equal; that is, the moving forces of the two bodies muſt be equal.

But their antagoniſts will reply, that this principle is not admitted by them, except the times of producing thoſe effects are equal; and that they are not ſo in the caſe before us: for the greater body will take up a longer time in producing it's effect, or in bending it's ſpring.

If therefore the *Leibnitzian* party pretend, that equal effects, when produced in unequal times, do always ariſe from equal cauſes, they muſt not impoſe this upon their opponents by way of principle or axiom, but muſt demonſtrate it. Till this be done, there will be room to doubt, at leaſt, whether the two bodies have equal moving forces, though they bend equal ſprings to the ſame degree.

For the larger and ſlower of theſe two bodies will bend the one ſpring more ſlowly; and, conſequently, will be reſiſted for a longer time, than the ſmaller and ſwifter body will be reſiſted in bending the other ſpring to the ſame degree. May not therefore the total reſiſtance of a ſpring be greater, if that reſiſtance continues for a longer time? and if the total reſiſtance be greater, muſt not the moving force, which is deſtroyed and conſumed by that reſiſtance, be alſo greater? is there not reaſon then to doubt, whether the moving forces of theſe two bodies be equal, though they bend equal ſprings to the ſame degree?

In like manner, when a ſpring, already bent to ſome certain degree does, by unbending, drive before it a body which gives way to it's preſſure, is there not room to doubt, whether the preſſure of the ſpring may not produce a greater effect, when that preſſure continues for a longer time?

That preſſure may be ſaid to produce three effects, all of which may if we pleaſe, be conſidered as different from one another.

1. The preſſure carries the body thro' a certain ſpace; by which ſpace the length of the bent ſpring is increaſed, in returning to it's natural ſituation.

2. The preſſure gives to the body a certain quantity of motion.

3. It gives the body a certain moving force.

Now, the firſt of theſe effects is greater, when the preſſure acts for a longer time. For, if the preſſure of a bent ſpring, by acting for one ſe-

cond upon the body 1, carry that body 1 thro' the ſpace 1 ; the preſſure of the ſame, or of an equal ſpring equally bent, by acting for two ſeconds upon the body 4, will carry that body 4 thro' the ſame ſpace 1.

Likewiſe the ſecond effect is greater, when the preſſure continues for a longer time.

For, in the caſe juſt now mentioned, the body 4 will have twice the quantity of motion that the body 1 has ; though theſe two quantities of motion ariſe from the preſſure of the ſame, or, which is all one, of equal ſprings equally bent. Why therefore are we to take it for granted, or to have it impoſed upon us by way of principle or axiom, that the third effect is not greater, when the time, in which it is produced by the preſſure of the ſame, or equal ſprings, is longer, nay, infinitely longer ?

But we are told, that all the force, which reſided in the ſpring, while bent, is now, upon the unbending of the ſpring communicated to the body moved. I aſk therefore, what was that force, or what kind of force was that, which reſided in the ſpring, while bent, and without motion ? was it a bare preſſure, or a moving force ? A *Vis mortua*, or a *Vis viva ?* you muſt acknowledge, it was a *Vis mortua*, a bare preſſure, and nothing more. But the force communicated to the body, and which now reſides in the body in motion, is a *Vis viva*, a moving force. This therefore is not the ſame force, nor a force of the ſame kind, as that which reſided in the bent ſpring.

It will be ſaid, however, that the force of the bent ſpring is intirely exhauſted in giving the body it's moving force. I aſk therefore again, what is it I am to underſtand by theſe words, the force of the ſpring is intirely exhauſted ? If the meaning be, that the ſpring could not poſſibly give that ſame body any greater moving force, than what it has already given, I allow it : but this does not prove, that the ſame ſpring, bent afreſh to the ſame degree, or an equal ſpring equally bent, cannot give a greater force to a greater body.

But if the meaning of theſe words be, that the ſpring cannot give a greater moving force to any body whatſoever, I muſt anſwer, that this is taking for granted the very point which is in diſpute. For the oppoſite party pretend, that a body of four times the bulk, will receive twice the moving force in twice the time, from the preſſure of the ſame ſpring in unbending itſelf, or, if you pleaſe, in exhauſting all it's force.

It is plain, therefore, that the followers of Mr *Leibnitz* have no right to ſay, a body has ſuch or ſuch a force, becauſe ſuch or ſuch a ſpring has put it in motion by unbending itſelf, or can be bent by it. This is not a poſition to be taken for granted, but ſtands in need of a demonſtration, which nobody has as yet attempted to give, at leaſt from any uncontroverted principle ; and, till this be done, the laying down any ſuch poſition can have no other effect, than to perplex the controverſy more and more, without hopes of ever coming to an end of it.

For which reaſon I propoſe to take a quite different method in what follows, and to lay down nothing, by way of principle or axiom, but

what

what is allowed of by all the world, or, at leaſt, has never yet been contradicted *a priori*.

When a bent ſpring does, by unbending itſelf, puſh a body before it, the greater the body is, the more ſlowly will the ſpring unbend itſelf. *Axiom* I.

The more any ſpring is bent, the greater is it's preſſure. *Axiom* II.

A greater preſſure produces a greater moving force, if the time be given. *Axiom* III.

Moving forces are not proportional to the maſſes of the bodies, and the ſquares of their velocities. *Propoſition* I.

Let there be two ſprings, equal, and equally bent, *A* and *B*, which, by unbending themſelves, puſh before them two unequal bodies; the ſpring *A* puſhing before it the greater body. Now, by *Axiom* I. the ſpring *A* will unbend more ſlowly than the other: from which it follows, that, at every inſtant of the time which the ſpring *B* takes up in unbending itſelf, the ſpring *A* will have unbent itſelf leſs than *B*, or will be more bent than *B*. *Demonſtration*

Therefore, by *Axiom* II. the preſſure of the ſpring *A* will, at any inſtant of that time, be greater than the preſſure of the ſpring *B* at that ſame inſtant. Hence, by *Axiom* III. the naſcent, or infinitely ſmall moving force, which is produced by the preſſure of the ſpring *A* in every infinitely ſmall part of that time, will be greater than that produced by the preſſure of the ſpring *B* in the ſame infinitely ſmall part of the time. Therefore, the ſum of the infinitely ſmall moving forces; that is to ſay, the whole moving force, which is produced by the ſpring *A*, during that time, will be greater than the moving force produced by the ſpring *B* in that ſame time: or the moving force of the greater body will be greater than that of the leſſer, at the inſtant that the ſpring *B*, being now wholly unbent, ceaſes to act any longer upon the body it has puſhed before it: and as, after that inſtant the ſpring *A*, not being yet wholly unbent, continues to act upon the greater body, the moving force of the greater body will ſtill continue to increaſe, and conſequently will more and more exceed the moving force of the ſmaller body.

But every one knows, that the products of the maſſes and ſquares of the velocities are equal in the two bodies. Therefore the moving forces which we have proved to be unequal, are not proportional to the products of the maſſes and ſquares of the velocities. Which was to be demonſtrated.

To conſider this in a particular example, let us ſuppoſe the maſſes of the two bodies expoſed to the preſſure of the ſprings *A* and *B*, to be 4 and 1 reſpectively; and let the ſpring *B* unbend itſelf, and thereby give the body 1 it's whole moving force in one ſecond of time. Then, at the end of that ſecond, the moving force of the body 4 will already exceed that of the body 1, and will ſtill grow greater during another ſecond of time. For the times are as the ſquare roots of the maſſes.

Alſo if the maſſes be 100 and 1, the moving force of the body 100, will, at the end of the firſt ſecond of time, be greater than that of the

body 1, and will continue to increaſe during the ſpace of nine other ſeconds.

Corollary. When a bent ſpring does, by unbending itſelf, drive a body before it, the larger that body is, the greater will be the moving force which it receives from the ſpring.

Having now clearly proved, that the moving forces are not proportional to the ſquares of the velocities, I proceed next to demonſtrate, that they are proportional to the velocities themſelves: and, in order thereto, I ſhall, as I have hitherto done, make uſe of no other principles or axioms than ſuch as are admitted on both ſides, or, at leaſt, have never yet been controverted *a priori* by either party.

Axiom IV. Springs of unequal lengths, when bent alike, have equal preſſures.

We ſpeak here of ſprings equal in all reſpects, except the length only; and, by being bent alike, we mean, that they are ſo compreſſed, as that the lengths they are now reduced to, are exactly proportional to their natural lengths, or to the lengths they are of when no way compreſſed. In this condition, if one be directly oppoſed to the other, they will mutually ſuſtain each other's preſſure, ſo as to maintain a perfect *æquilibrium:* or, if each be placed ſeparately in a vertical ſituation, they will ſuſtain equal weights. And in one or the other of theſe caſes, it is evident, that they muſt exerciſe equal preſſures.

Axiom V. Equal preſſures in equal times produce equal moving forces.

Propoſition II. Moving forces are proportional to the maſſes and velocities jointly.

Demonſtration. Let there be two ſprings, of the lengths 1 and 2, but equal in all other reſpects, and bent alike: and, in unbending themſelves, let the ſpring 1 drive before it a body whoſe maſs is 2; and the ſpring 2 another body of the maſs 1. Now, by *Coroll.* 11. of my general theorem concerning the action of ſprings, theſe two ſprings will unbend themſelves exactly in the ſame time; and, conſequently, the ſpring 2 will unbend itſelf with a velocity double of that of the ſpring 1: and by *Coroll.* 12. of the ſame thorem, it will give to the body 1 a velocity double of that, which the body 2 will receive from the ſpring 1. Alſo, as the two ſprings were ſuppoſed to be bent alike at firſt, and the ſpring 2 unbends itſelf with a velocity double to that of the ſpring 1, it is manifeſt, that, during the whole time of their expanſion, they will be always bent alike, one to the other. Therefore, by *Axiom* IV. their preſſures will be conſtantly equal one to the other: and hence, by *Axiom* V. the infinitely ſmall moving forces produced by each of theſe ſprings, in every infinitely ſmall part of time, will be equal one to the other. Conſequently, the ſums of thoſe infinitely ſmall moving forces, that is, the whole moving forces, produced by the two ſprings, will be equal one to the other. And the maſſes of the two bodies being 2 and 1, and their velocities being 1 and 2 reſpectively, it is plain, that the moving forces are proportional to the maſſes and velocities jointly. Which was to be demonſtrated.

For

For the greater facility of examining this demonſtration, we have ſuited it to a ſingle caſe only, and that the moſt ſimple that can be ſuppoſed: but every body will ſee, how eaſy it is to form a general one upon the ſame principles.

As we do not think, that any flaw can be found in either of the demonſtrations above laid down; and the axioms, upon which they are founded, have never yet been diſputed, as far as we know; we preſume, that the *Leibnitzian* opinion about the meaſure of moving forces, is inconteſtably overthrown by the firſt propoſition, and the oppoſite ſentiment is as evidently eſtabliſhed by the ſecond.

But, if any reader ſhall be of a different opinion, we muſt beg leave to propoſe to his conſideration the following experiment, which we hope may juſtly deſerve the name of an *experimentum crucis*; and, as ſuch, may put a final period to this controverſy.

It is not new indeed, having been propoſed before by myſelf and others; but, as the manner, in which it was formerly offered, has given occaſion to ſome objections, which, tho' not affecting the ſubſtance of the argument drawn from it, may yet have amuſed and embarraſſed the leſs attentive readers, I ſhall now propoſe it in ſuch a manner, as may obviate all thoſe difficulties, and, I think, will render it abſolutely deciſive. To me, I am ſure, it will be ſo; ſince I ſhall immediately embrace the *Leibnitzian* doctrine, if my argument drawn from it can receive a clear and ſatisfactory anſwer.

Experiment. Upon an horizontal plane, at reſt, but moveable with the leaſt force, ſuppoſe upon a boat in a ſtagnant water, let there be placed, between two equal bodies, a bent ſpring, by the unbending of which the two bodies may be puſhed contrary ways.

In this caſe it is evident, that the velocities, which the two bodies receive from the ſpring, will be exactly equal, and their moving forces will alſo be exactly equal; and that the plane they move upon, and alſo the boat upon which it lies, will have no motion given them either way. Let us call the velocity of each body 1, and the moving force alſo 1.

Now, let us ſuppoſe the ſpring to be bent afreſh to the ſame degree as before, and to be again placed between the two bodies lying at reſt; then let the plane, upon which the ſpring and the bodies lie, be carried uniformly forwards, in the direction of the length of the ſpring, with this ſame velocity 1. In this caſe it is manifeſt, that each of the bodies will have the velocity 1, and the moving force 1, both in the direction of the *axis* of the ſpring.

During this motion, let the ſpring again unbend, and puſh the two bodies contrary ways, as before, the one forwards, the other backwards: then the ſpring will give to each of theſe bodies the velocity 1, as before, when the plane was at reſt.

By this means the hindmoſt body, or that which is puſhed backwards, will have it's velocity 1, which it had before by the motion of the plane, now intirely deſtroy'd, and will be abſolutely at reſt.

But

But the body, which is puſhed forwards, will now have the velocity 2, namely 1 from the motion of the plane, and 1 from the action of the ſpring.

Thus far every body agrees in what will be the event of this experiment.

But the queſtion is, what will be the moving force of the foremoſt body, or of that which is puſhed forwards, and which has the velocity 2; *viz.* 1 from the motion of the plane, and 1 from the action of the ſpring.

By the *Leibnitzian* doctrine, it's moving force muſt be 4: and, if ſo, it muſt have received the moving force 3 from the action of the ſpring; for it had only the moving force 1 from the motion of the plane.

Let us examine, whether this be poſſible, or reconcileable to their own doctrine.

Their doctrine is, that equal ſprings, equally bent, will, by unbending themſelves, give equal moving forces to the bodies they act upon, whatever thoſe bodies are.

We agree to this, not generally indeed; but in the caſe before us, where the bodies are of equal maſſes or weights, we agree to it.

Let us therefore imagine the bent ſpring, which is placed between the two bodies, to be divided tranſverſly into two equal parts. In this caſe it is plain, that the two halves of the ſpring, may be conſidered as two intire ſprings, equal, and equally bent, each of which reſts at one end in *æquilibrio* againſt the other ſpring, and at the oppoſite end, preſſes againſt the body it is to move.

Conſequently, by the *Leibnitzian* doctrine, to which, in this particular caſe, where the bodies are equal, we alſo agree, the two ſprings will give equal moving forces to the two bodies.

But the moving force received by the hindmoſt body from the hinder ſpring, was undoubtedly the moving force 1: for by that force given it in the direction backwards, the moving force 1, which it had before from the motion of the plane in the direction forwards, is exactly balanced and deſtroyed, the body remaining, as was obſerved before, in abſolute reſt.

Therefore the moving force received by the foremoſt body from the foremoſt ſpring, was alſo the moving force 1. And this, added to the other moving force 1, which it had before from the motion of the plane, makes the moving force 2, and not the moving force 4, as the *Leibnitzian* philoſophers pretend.

Conſequently, that body, which had before the velocity 1, and the moving force 1, and now has the velocity 2, has alſo the moving force 2: that is, the moving forces are proportional to the velocities.

Dynamical Principles, or Metaphyſical Principles of

III. When the famous *Leibnitz* publiſhed * his new doctrine, by which he determines, that the force of a body in motion is to be meaſured by the ſquare of the velocity, it raiſed a great controverſy in the Mathematical World. The ſame author, in *April* 1695, publiſhed his

* *Act. Erud. Lipſ.* 1686.

Specimen

Specimen Dynamicum, in confirmation of this doctrine: and in one place makes uſe of the following expreſſion;

" I arrived at the ſame true eſtimation of forces by different ways: " one *a priori*, by a moſt ſimple conſideration of ſpace, time, and ac- " tion, which I ſhall explain in another place. The other *a poſteriori*, by " eſtimating the force by the effect which it produces in exhauſting it- " ſelf."

Mechanicks; *by the ſame.* N°. 479. p. 103. Mar. &c. 1746. *Preſented* Mar. 13. 1746.

He ſeemed to intend the publication of his *a priori*, which he promiſed to *explain in another place*, in *May* following: for towards the end of his *Specimen Dynamicum* he adds the following words;

" And now, having diſpelled error, we ſhall produce the true and " really admirable laws of Nature, ſomewhat more diſtinctly, in the ſe- " cond part of this eſſay, to be publiſhed in the month of *May*."

But, to our great misfortune, this ſecond part never made it's appearance in publick, either in the month of *May*, or in any ſubſequent month, or year, either in the *Leipſick Acts*, or any where elſe, tho' the author ſurvived above 20 years.

However, to clear this great man from the imputation of not having performed his promiſe, the world has lately been favoured with it in the *Commercium Literarum* between himſelf and another famous Mathematician, *John Bernoulli*.

Bernoulli, it ſeems, upon ſeeing the *Specimen Dynamicum*, wrote to *Leibnitz*, in *June* following, applauding ſome things, but at the ſame time being ſo far from approving his eſtimation of forces, that he even endeavoured to demonſtrate, that the forces of moved bodies are not in proportion to the ſquares of their velocities, but to the velocities themſelves. But at laſt, after ſeveral letters had paſſed between them, *Bernoulli* came over to *Leibnitz*, who, being willing to reward the docility of ſo eminent a diſciple, communicates to him his argument *a priori*, which he had hitherto kept to himſelf, and at the ſame time aſſigns the reaſon why he did not divulge it ſooner;

" I would not honour, ſays he *, with this clear light of truth, thoſe " who did not receive as they ought thoſe arguments drawn from the " affections of heavy, or other ſenſible bodies; wherefore I would not " make them publick; but reſerved them to be communicated to " thoſe, who had ſhewn themſelves to be equal judges."

Bernoulli therefore, *having ſhewn himſelf to be an equal judge*, and having *received as he ought* thoſe arguments *a poſteriori*, that is, having come over entirely to the opinion of *Leibnitz*, was thought worthy of the *honour* to be admitted into theſe ſecret receſſes of Science.

" Becauſe, ſays the author †, I ſee you are on our ſide, I will " freely communicate to you my principle of demonſtrating *a priori* " the true eſtimation of forces; which I have ſometimes mentioned as " being in my hands, but have never yet produced. For communi-

* *Jan.* 1696. † *Jan.* 1696.

cating

" cating to you is committing a ſeed to a moſt fruitful ſoil, that it " may grow up into a large plant."

I cannot but commend the good gentleman for *committing* his *ſeed* to ſo *fruitful a ſoil*: and yet I cannot think him wholly free from deſerving ſome cenſure. For though he could work no effect on the *Papins*, *Catelans*, and other oppoſers of his doctrine, who *ſeemed to be incapable of converſion*, *by any demonſtrations*, *how ſtrong ſoever*, though he might think them unworthy of *this clear light of truth*, yet why did he envy it to the reſt of the learned world? I will not ſay, that it was the part of a good, and humane man, and of one who was deſirous to increaſe knowledge, to lay open to all an affair of ſuch moment, but if he had only ſtudied his own glory, preferably to every thing elſe, he ſhould have acted in this manner, that thoſe detractors might either have been immediately ſilenced, or condemned by all the world. Laſtly, as great men are not born for themſelves alone, or for a friend or two; but for all; is it not a little unjuſt, that *Bernoulli* and his diſciples ſhould alone enjoy *this clear light*, when we poor wretches are condemned to live in more than Cimmerian darkneſs. But it is well for us, that after 50 years of darkneſs, that light at laſt ſhines forth upon all. But behold the argument!

" 1. An action making duple, in a ſimple time, is duple, virtually " of an action making the ſame duple in a duple time; or the walking " of 2 miles in 1 hour, is duple, virtually, of the walking of 2 miles " in 2 hours.

" 2. An action making duple in a duple time, is duple, formally of " an action making ſimple in a ſimple time; or the walking of 2 miles " in two hours is duple, formally, of the walking of 1 mile in 1 hour.

" 3. Therefore an action making duple in a ſimple time is quadruple " of an action making ſimple in a ſimple time; or the walking of 2 " miles in 1 hour is quadruple of the walking of 1 mile in 1 hour.

" 4. If for duple we had ſubſtituted triple, quadruple, quintuple, &c. " the action would have come out noncuple, ſedecuple, 25ple; and " generally it appears, that equable, equitemporaneous, moving ac- " tions, are to equal moveable, as the ſquares of the velocities; or, " which is the ſame thing, that in the ſame or an equal body, the for- " ces are in a duplicate ratio of the velocities." *Q. E. D.*

Having read this argument, and out of regard to the great fame of the author, having conſidered it with much attention, I muſt confeſs, I could not diſcover the leaſt ſpark of truth in it, or even of common ſenſe. I ſhould have ſuſpected, that this had been owing to the weakneſs of my own eyes, which perhaps were dazzled by the too great brightneſs of the light, if a doubt of *Bernoulli* himſelf had not raiſed my ſpirits.

This ingenious perſon was ſo far from acquieſcing in *this clear light of truth*, that he not only made an objection, but even produced a double demonſtration.

" I do

" I do not ſee, ſays he *, what can be ſaid by an adverſary to the con-
" trary; unleſs perhaps, that the virtual action ſeems to be confounded
" with the formal action; denying the conſequence, that *A* is the qua-
" druple of *C*, becauſe *A* is the duple of *B*, virtually, and *B* the duple
" of *C* formally.

Having propoſed this objection, he adds his demonſtrations.

" 1. An action making duple in a ſimple time is virtually duple of
" an action making the ſame duple in a duple time."

" 2. An action making duple in a duple time is ſimple virtually of
" an action making ſimple in a ſimple time.

" 3. Therefore an action making duple in a ſimple time, is duple of
" an action making ſimple in a ſimple time. Or,

" 1. An action making duple in a ſimple time is ſimple formally of
" an action making the ſame duple in a duple time.

" 2. An action making duple in a duple time, is duple formally of
" an action making ſimple in a ſimple time.

" 3. Therefore an action making duple in a ſimple time, is duple of
" an action making ſimple in a ſimple time.

" You ſee the 2 arguments, which plainly conclude the ſame thing,
" but are quite contrary to your concluſion, and depend on that com-
" mon axiom, that thoſe things which are equal to the ſame are equal
" amongſt themſelves, which indeed holds only in homogeneous quan-
" ties, as here in comparing a virtual action with a virtual, and a for-
" mal one with a formal, but not one with the other."

Thus *Bernoulli* with no leſs acuteneſs than modeſty. But *Leibnitz*, in his letter dated in *March*, in the firſt place endeavours to take off *Bernoulli*'s objection.

" I do not well underſtand, ſays he, what you mean, when you ſay
" a virtual action is confounded with a formal one. For I do not here
" treat of an action as being either virtual or formal; but one action is
" duple of another, either virtually or formally. Virtually, when it is
" duple in eſtimation, tho' it is not duple in bulk, or congruence, as
" a ducat is the duple of a dollar: but formally, as a dollar is the du-
" ple of a half dollar. And you muſt know, that what is duple for-
" mally is duple alſo in virtue or eſtimation. Therefore as the inquiry
" here is only concerning virtue or eſtimation, there is no confuſion of
" the different kind of quantities or eſtimations; for by virtually duple,
" I underſtand that which is ſo only virtually; but I call that formally
" duple, which is duple both formally and virtually."

It is not to be denied, that *Leibnitz* has a right to aſſign what ſenſe he pleaſes to the words made uſe of by him, and that by this means he plainly takes off *Bernoulli*'s objection. But I could wiſh he had explained one thing, either of his own accord, or at *Bernoull*'s requeſt, by what virtue or by what eſtimation an action making duple in a ſimple

* *Feb.* 1696.

time, is duple of an action making the ſame duple in a duple time. For if I am not greatly miſtaken nothing can be more falſe.

He proceeds, " I might abſtain from words uſed only for the ſake of " a certain harmony, for as becauſe a ducat is the duple of a dollar, " and a dollar of a half dollar, I conclude that a ducat is the quadru- " ple of a half dollar; ſo becauſe the walking of 2 miles in one hour " is the duple of walking 2 miles in 2 hours, and the walking of 2 " miles in 2 hours is the duple of walking 1 mile in 1 hour, it will fol- " low that the walking of 2 miles in 1 hour is the quadruple of walk- " ing 1 mile in 1 hour."

Theſe troubling words, *virtually* and *formally*, being new removed which had hitherto fouled *this clear fountain of truth*, Leibnitz not only took off *Bernoulli*'s objection, but brought him over entirely to his ſide. " Your anſwer, ſays he in his next letter, quite ſatisfies me; for I ſee " what you mean by thoſe 2 terms: but your argumentation appears to " me very elegant, and that it ought no longer to be detained from " the publick; for it will give great weight to the arguments *a poſte-* " *riori*."

Thus *Bernoulli* in his letter dated in *April*, and I would likewiſe acquieſce in the ſame argument, if any one will ſhew me, that it is as plain that the walking of 2 miles in 1 hour is the duple of walking 2 miles in 2 hours, as that a ducat is the duple of a dollar. For I ſee that walking 2 miles in 1 hour has duple the velocity of walking 2 miles in 2 hours; but I do not find it to be duple, but equal, ſince the ſame ſpace is gone over in each walk.

But perhaps *Bernoulli* would not urge the matter any farther, as *Leibnitz* ſeemed to be in a more than ordinary commotion, " I, ſays he *, " dare not promiſe any thing great; but I hoped to be not guilty of a " moſt open paralogiſm, in an argumentation, which did not ſlip from " me on a ſudden, but had been conſidered by me for ſeveral years, " and was vaunted by me as a thing of ſome moment." However, that *Leibnitz* was *guilty of a moſt open paralogiſm*, will be ſhewn preſently, if I am not greatly miſtaken.

I need not dwell upon *Leibnitz*'s examination of both *Bernoulli*'s demonſtrations, becauſe they depend upon the ſenſe of the words *virtually* and *formally*, underſtood differently from the meaning of *Leibnitz*. " I " took the terms, ſays *Bernoulli* †, in a different ſenſe from that in " which you now explain them."

But *Leibnitz*, being ſtill in doubt what weight his firſt demonſtration would have with *Bernoulli*, adds another to it. " I add another, ſays " he ‖, which, if you examine it to the bottom, comes to the ſame as " the former, and yet it has it's own proper weight. Moving actions, " I mean equable ones, of the ſame moveable are in a ratio compounded " of the immediate effects, namely the lengths run thro' and the velo-

* *Mar.* 1696. † *Apr.* 1696. ‖ *Mar.* 1696.

" cities.

" cities. Now the lengths, equably run thro' are in a ratio compound-
" ed of the times and the velocities. Therefore moving actions are in
" a ratio compounded of a ſimple ratio of the times, and a duplicate
" one of the velocities; and ſo, in the ſame times, or elements of times,
" the moving actions of the ſame moveable are in a duplicate ratio of
" the velocities, or if the moveables are different, in a ratio compounded
" of a ſimple ratio of the moveables, and duplicate one of the velo-
" cities.

As *Bernoulli* had ſaid, in his letter dated in *April*, that he acquieſced in the former demonſtration, but did not ſay a word of the latter, *Leibnitz* aſked him in *May*, " what he thought of the other demonſtration " of the ſame propoſition, which (ſays he) is a little more according to " the received form, tho' they both agree in the root."

Bernoulli therefore, when he could no longer avoid opening his mind, in his letter of *June*, thus expreſſes himſelf.

" Your other demonſtration of the propoſition concerning the ratio of " moving actions, which you had alledged in the former, ſeems to me " to be contrived no leſs ingeniouſly than the former, and, as you " expreſs it, more according to form, tho' in the bottom of the thing " they both coincide. For nothing is more evident to me, than that " moving actions ought to be meaſured by their immediate effects; if " therefore the lengths gone thro' and the velocities, unleſs any one will " obſtinately have the velocity to be rather the cauſe, are the effects of " an immediate action, and indeed the only ones, of which one does " not depend on the other, or is not included in the other, the mov-" ing actions will neceſſarily be in a ratio compounded of the lengths " and the velocities; and ſo in equal times in a duplicate ratio of the " velocities."

It is plain that *Bernoulli* in this anſwer approves of the ſecond demonſtration in appearance, but in reality condemns it, tho' with the greateſt caution and modeſty. For he not only hints that the velocity is rather the cauſe than the effect of an action, but he reſtrains his aſſent to this condition, that *one* of the effects mentioned by *Leibnitz*, namely of the length gone thro' and the velocity, *does not depend on the other, or is not included in the other*. Now therefore as it is very evident, that the length gone thro' does depend on the velocity, and is included therein, it is plain that the demonſtration is faulty in the opinion of *Bernoulli*.

Leibnitz, in his next letter dated in *June*, gave a copious and diſtinct anſwer to many other things, but to theſe tacit objections of *Bernoulli*, he anſwers lightly, diſſembling their force, and as if he was treating of ſomething elſe, only juſt ſays,

" But as I now eſtimate an action by the compound ratio of it's prin-" ciples, power and time; ſo I had eſtimated it a little before by the " compound ratio of what it performs; an extenſive or material effect, " namely of the length, which I uſually call an effect κατ' ἐξοχήν, and

" an intenſive or formal effect. For it is required that much ſhould be " performed and ſoon. You ſee now that both the eſtimations agree " together."

By the obſcurity of this anſwer, whether affected, or natural to *Leibnitz*, it is eaſily ſeen that he would have the velocity to be taken for the effect of an action, which *Bernoulli* had hinted was rather the cauſe, but that he did not dare to name it openly, tho' he underſtands it under the name of an intenſive or formal effect, which the action *performs*. Beſides as to the other objection of *Bernoulli*, that tho' the velocity is in the higheſt degree the effect of an action, as well as the length gone thro'; yet as one of theſe effects depends on the other, or is included in the other, and certainly the length gone thro' depends on the velocity, an action ought not to be meaſured by thoſe effects; as to this, I ſay, he obſerves a profound ſilence.

The ſecond demonſtration therefore ſeems to be given up by *Leibnitz* as well as *Bernoulli*; and indeed in all their ſubſequent letters, I do not find the leſt mention of it.

Moreover, that firſt demonſtration, which comes to the ſame with the other, that is, a true one with a falſe one does not ſeem to be wholly free from exception, either with *Bernoulli*, or with *Leibnitz* himſelf.

For *Bernoulli*, tho' he had declared in *April*, that *it quite ſatisfied* him, that *he acquieſced* in it, and that it was *very elegant, and ought no longer to be denied to the publick*, in *Auguſt* however did not know what *Leibnitz* meant by the word *action*, on which that whole demonſtration depends. " You ought, ſays he, to define what you mean by action; " otherwiſe nothing can ever be demonſtrated." This was a juſt admonition, but to no purpoſe; for in the letter which *Leibnitz* wrote in anſwer to this, you will not find a tittle of that definition ſo highly neceſſary.

But *Leibnitz* himſelf, in his letter dated in *June*, expreſſes himſelf thus; " my demonſtration *a priori*, for our eſtimation of forces, de- " pends upon a certain ſuppoſition. Namely, *that an action which does* " *any thing uniformly, in a ſimple* time, is duple of an action doing the " ſame thing uniformly, *in a duple time.* This ſuppoſition ought to " have been granted by *Catelan* and the reſt, with whom I had diſputed." But what if they will not grant it? why then the demonſtration, which depends upon this ſuppoſition, falls to the ground, at leaſt till you demonſtrate that ſuppoſition.

But, " I have not yet indeed found out a way of demonſtrating this " propoſition *a priori* by the way of congruency, nay not even this, " that *an action doing the ſame thing, in a ſhorter time, is greater*; which " ought to have been the beginning."

Therefore ſince that ſo much boaſted demonſtration *a priori* ſtood in need of another demonſtration, which *Leibnitz* had not yet diſcovered, nor ever after did diſcover, nor any mortal ever will diſcover, it is no wonder

wonder that this *ſeed*, tho' *committed to a moſt fruitful ſoil*, did not *grow up into a large plant*. For *Bernoulli* took a final leave of this *clear light of truth*, when he ſaw it dwindle a way to a meer ſnuff.

But a gentleman of much higher courage, the learned *Chr. Wolfius*, having attempted to treat the theory of forces after a geometrical manner, communicated to the publick, in the firſt volume of the *Comment. Acad. Imp. Petropol.* under the title of *Principia Dynamica*. " When he had " communicated," part of this " *in* 1710 *to the moſt illuſtrious* Count " de Herberſtein, *to the moſt illuſtrious* Leibnitz *and others*, Leibnitz, " *in a letter* 1711, *ſaid that it agreed with his, which he had communi-* " *cated to the famous* John Bernoulli, Jacob Herman, *and others, con-* " *firming it in theſe words:* I lay down this calculus of pure forces or " actions. Let the ſpace be *s*, the time *t*, the velocity *v*, the body *c*, " the effect *e*, the power *p*, the action *a*. Then *tv* will be in equal " motion as *s*, *e* as *cs*, *tp* as *a*. And theſe may be aſſumed without a " demonſtration. Add, what is to be demonſtrated, *ev* as *a*. Hence " many other theorems may be demonſtrated; for inſtance *p* as cv^2. " For *tp* as *ev*: but *e* as *cs*, and *s* as *tv*. Therefore *tp* as ctv^2, or *p* " as cv^2. And in theſe is contained part of my *Dynamick*, abſtracted " from ſenſible things, tho' it is afterwards verified by experiments." " I do not doubt therefore, ſays *Wolfius*, but I have here propoſed *Dy-* " *namical* principles, which are conformable to the ſentiment of *Leib-* " *nitz*."

And this indeed is manifeſt of itſelf, as the theorems of *Wolfius* exactly agree with the algebraical notations of *Leibnitz*; but whether theſe principles are as conformable to truth as they are to the ſentiment of *Leibnitz*, is worth the while to examine. But I find one thing particularly worthy of obſervation in theſe notations, *a* as *ev*, *which is to be demonſtrated*: whence *Leibnitz* ſeems, not even then, after 16 years, to have found out a demonſtration of the ſuppoſition formerly put off to *Bernoulli*; that an action doing any thing, in a ſimple time, is duple of an action doing the ſame thing in a duple time; ſince an action doing any thing in a ſimple time, does it with twice the velocity of an action doing the ſame thing in a duple time. But how *Wolfius* demonſtrates this, we ſhall examine preſently.

For the moſt illuſtrious Imperial Academy of Sciences at *Peterſburg* was pleaſed to make me a preſent of the 3 firſt volumes of their Commentaries, and at the ſame time to ſignify that it would not be diſagreeable to them, if I would ſend them any obſervations of mine to be inſerted in their Commentaries. In conſequence of this, having ſent a paper relating to my theory of the action of capillary tubes, which was well received by the moſt illuſtrious Academy, and publiſhed in their Commentaries, I ſoon after took the liberty of ſending another under the title of *Principia Dynamica*.

For as I ſaw that the celebrated *Wolfius* propoſed to explain clearly and diſtinctly, and after a geometrical manner, thoſe things which had been

been leſs perſpicuouſly handled by *Leibnitz*, ſo that it was not eaſy to diſcover truth from falſehood; and yet that they agreed exactly together in the main; I was willing to take the opportunity of bringing that theory to an accurate examination.

With this view I tranſcribed exactly all that I thought was rightly delivered by *Wolfius*, and inſerted them in my *Dynamical Principles*; what was wanting I ſupplied; and what ſeemed to be falſe I corrected. When I had done this, I ſent it 12 years ago with all due reſpect to the Imperial Academy. That it was read in their publick aſſembly, and that thanks were ordered for the communication, I was informed by the learned *Muller*, who was then at *Peterſburg*, and ſetting out for the expedition to *Kamkatſchi*.

But afterwards, when after ſo many years I found no mention of that paper in the Commentaries, I inquired laſt year of a friend, what was become of it. He anſwered me at firſt, that no ſuch paper had ever been preſented to the Academy. I anſwered, that it had certainly been preſented, that it was read in a publick aſſembly in *June* 1733, and that thanks were returned me for it. At laſt, on examining their regiſters, it was found to be true; but the paper itſelf could no where be found, nor could any one imagine by what accident it was loſt. However the moſt illuſtrious Academy were pleaſed to give me my choice either of ſending another copy to *Peterſburg*, to be inſerted in their Commentaries, or of publiſhing it in our Philoſophical Tranſactions.

When I had examined my own papers, I could not find a perfect copy of it any where, whether it had been loſt by ſome accident in moving twice from one houſe to another, or whether I had written only that copy which I had ſent to *Peterſburg*. I found however an imperfect copy, which I ſupplied as well as I could, and now preſent and dedicate it to the Royal Society of *London*, and I hope with better ſucceſs.

Dynamical Principles.

We often ſee, when perſons are engaged in law ſuits, that a thing which was at firſt eaſy and plain, has by the ill management of the advocates been carried thro' all the turnings and windings of the law, till it has ended in a difficult and almoſt inextricable cauſe. In ſuch a caſe, if any lawyer ſhall ſhew a ſhort and plain way of coming to a concluſion, I ſhall think he deſerves very well of both parties, on which ſide ſoever the queſtion is decided.

In this light I conſider the behaviour of the famous *Wolfius* with regard to the controverſy concerning moving forces, which has now for many years engaged the learned world. For if he has not attained to the truth, he has certainly ſhewn the way by which others may with ſafety and eaſe arrive at the truth.

Treading therefore in his ſteps and thoſe of the illuſtrious *Leibnitz*, whom he profeſſes to follow. I ſhall endeavour to explain the *Dynami-*

cal

cal Principles, to uſe their own term, with as much perſpicuity as is poſſible.

To which end I reſolved to conſider only one very ſimple caſe, of a body endued with a *Vis viva*, which is moved with an uniform motion, that is, without any impediment, either of a reſiſting medium, or of any oppoſite bodies whatſoever, plainly according to the poſitions of *Wolfius*. And if the candid reader ſhall obſerve, that I have taken this learned gentleman's definitions and axioms, nay and the ſubſequent propoſitions, excepting 1 or 2, and their demonſtrations, without changing a word, I muſt give him to underſtand, that I did this profeſſedly, becauſe I think they can neither be more clearly expreſſed, nor more certainly demonſtrated.

" I call that *Vis viva* with *Leibnitz*, or merely *vis* or *force*, which " adheres to a local motion." *Definition* 1.

" A *pure force* is that which is not reſiſted in acting by any con- " trary force." *Def.* 2.

" Therefore a pure force remains unvaried in the whole time of " action. *Corollary*

" Such a force exerts itſelf in an equable motion, if it be con- " ceived to be made in an unreſiſting medium. For in whatſoever " interval the moveable body is moved forwards, the ſame celerity al- " ways ſubſiſts, conſequently the moving force is the ſame. Therefore " the effect, which it produces, does not in the leſt exhauſt it. *Scholium.*

" A *pure action* is that which is exerciſed by a pure moving force." *Def.* 3.

" Such is the action of a moveable carried with an equable motion " in an unreſiſting medium. *Schol.*

" An uniform action is that, which is duple in a duple time, triple " in a triple, *&c.* or in general, which is as the time. *Def.* 4.

" Such an action has place in an equable motion, when a moveable " continues to be moved with the ſame celerity, namely if the motion " is conceived to be made in an unreſiſting medium. *Schol.*

" *The effect of a moving force beyond the conflict* is the tranſlation of a " moveable thro' a ſpace." *Def.* 5.

" If 2 or more equal moveables are moved with equal celerity, " the force of them is the ſame." *Axiom* 1.

" The ſame action is performed by the ſame force in the ſame " time." *Axiom* 2.

" That a greater action is performed by the ſame force in a longer " time than in a ſhorter, and that a greater action is performed in the " ſame time by a greater force than by a leſs, no one doubts. There- " fore the quantity of an action depends on the quantity of forces and " time. Wherefore if the forces are equal, and the time the ſame, the " action alſo muſt be the ſame." *Schol.*

" If the ſame moveable is transferred thro' the ſame ſpace, the effect " is the ſame." *Ax.* 3.

" We

Schol. " We ſuppoſe the motion to be made in an unreſiſting medium, or " at leaſt abſtract it from the action, which is ſpent in overcoming the " reſiſtance of the medium: which may be, whilſt we take no account " of the time in which the effect is produced."

Theorem 1. " If unequal bodies are moved with the ſame celerity, the forces are " as the maſſes."

The demonſtrations of this and the 8 following theorems, about which we have no controverſy with the *Leibnitzians*, were ſet down in *Wolfius*'s own words in the paper ſent to *Peterſburg*: but here we thought proper to omit them, to avoid prolixity.

Theorem 2. " Uniform actions performed in the ſame time are to each other as " their forces."

Theorem 3. " Uniform actions, performed with equal forces, are to each other, " as the times in which they are performed.

Theorem 4. " Uniform actions are in a ratio compounded of the times and " forces."

Theorom 5. " Unequal forces perform the ſame action in times reciprocally pro- " portional to each other."

Theorem 6. " If 2 equal moveables are transferred thro' unequal ſpaces, the effects " are as the ſpaces."

Theorem 7. " If any 2 moveables are transferred thro' the ſame ſpace, the effects " are as the maſſes."

Theorem 8. " If any 2 moveables are transferred thro' any ſpaces, the effects are " in a ratio compounded of the maſſes and ſpaces."

Theorem 9. " In an equable motion, the effects are in a ratio compounded of the " maſſes, celerities, and times.

Theorem 10. " Actions, by which the ſame effect is produced, are as the celeri- " ties."

We are now come to that theorem, on which the whole affair turns. If this is true, the *Leibnitzian* doctrine is to be embraced, if not, it is to be rejected. Therefore the demonſtration of this theorem muſt be diligently examined.

It is divided by *Wolfius* into 3 caſes; but as the ſecond and third depend on the firſt, we ſhall conſider only this one.

Demonſtration of the firſt caſe. " If moveables are equal, and the ſame effect is produced in a differ- " ent time, the velocities will be as the times reciprocally in which it is " produced; that is, a body, which produces an effect in the time $\frac{1}{2}T$, " is moved with the velocity $2C$, when another, which produces the " effect in the time T, is moved with the ſimple velocity C, and ſo " on. Now it is evident, that an uniform action is duple, which pro- " duces the effect in $\frac{1}{2}$ the time, triple, which in ſubtriple, and ſo on."

But do you ſay, Mr *Wolfius*, that this is evident? what if I ſhould deny it? what if I ſhould ſay that any action, which produces the ſame effect, is the ſame in what time ſoever it produces it. This is the very ſuppoſition of *Leibnitz*, of which, in his letter to *Bernoulli* dated in 1696, he ſays he has not diſcovered a method of demonſtrating *a priori*,

a priori, and in his letter to your ſelf dated 1711, ſays is ſtill to be demonſtrated. And yet you do not endeavour to demonſtrate it, but ſay it is evident, I deny it's being evident, and ſo your demonſtration falls to the ground, and the ſuppoſition itſelf therewith.

But before we ſubſtitute a new one, let us ſee a little, what is underſtood by action, and what by effect.

Wolfius, after the example of *Leibnitz*, has omitted the definition of action. He has only ſhewn what is a *pure* action, namely that which is free from all impediment; and what is an *uniform* action, namely that which increaſes in proportion to the time: but what he means by action itſelf he has no where determined. But till this is done, *nothing can ever be demonſtrated*, as *Bernoulli* adviſed *Leibnitz* long ago, but in vain.

If I might venture to ſupply this defect, I would aſcribe the ſame definition to action, which *Wolfius* has given of effect; ſince there ſeems to be no other difference between action and effect, than that action, if I may ſo ſpeak, is an effect *in fieri*; and effect an abſolute action, or one that is perfected. For in *Wolfius*'s example, a *Vis viva* is that which transfers a moveable thro' a ſpace; therefore the action of a *Vis viva* is the *tranſlation of a moveable thro' a ſpace*; and the effect of a *Vis viva* is alſo the *tranſlation of a moveable thro' a ſpace*; or rather, an effect is a moveable already transferred thro' the ſame ſpace.

But generally, an action is the preceder of an effect; or rather, an action is that by which any thing is effected, but an effect is the thing itſelf which is effected.

I do not boaſt of theſe definitions as being perfect; but yet I think they are without any danger of being miſtaken, eſpecially if I make the thing a little plainer by ſome examples.

If I write a page, my action will be the writing of a page, and the effect will be a page written.

If a workman whitens a wall, his action will be the whitening of a wall, and the effect will be a wall whitened.

If a labourer digs a garden, his action is the digging of a garden; and the effect is a garden digged.

Any one may eaſily conceive an infinite number of examples; and indeed I ſhould have been aſhamed to dwell ſo long on things ſo plain, and in a manner frivolous, if theſe very things falſly conceived had not thrown ſo many great men into the moſt grievous errors. For

— — — *Hæ nugæ ſeria ducunt*
In mala.

Of equal actions the effects are equal. *Our 10th Theorem.*

Let any *Vis viva A* perform any action, and let there be ſuppoſed any other *Vis viva B*. Now that the *Vis viva B* may perform an action equal to the *Vis viva A*, it is neceſſary that the *Vis viva B* ſhould act exactly as much as the *Vis viva A* has acted. Therefore after the com-

pletion of the action of *B*, as much will be acted by the force *B*, as has been acted by the force *A*; that is, the effect of the *Vis viva B* will be equal to the effect of the *Vis viva A*, the actions of which were equal. *Q. E. D.*

Our 11th *Theorem. Demonſtration.*

Actions are in proportion to the effects.

Let the effect *e* be produced by the action *a*. Therefore another effect *e* equal to the firſt will, by *Theor.* 10, be produced by another equal action *a*: conſequently, the effect *b* will be produced twice by the action *a*. In like manner it appears that the effect thrice *e* muſt be produced by the action thrice *a*, &c. Nay *in genere*, that the effect *n e* (= *E*) muſt be produced by the action *n a* (= *A*). Therefore *A* : *a* :: *E* : *e*, that is, the actions are in proportion to the effects.

Our 12th *Theorem.*

Forces are in a ratio compounded of the maſſes and velocities.

Demonſtration.

By *Theor.* 4. actions are in a ratio compounded of the times and forces.

By *Theor.* 11. actions are in proportion to the effects. Therefore effects are in a ratio compounded of the times and forces. But by *Theor.* 8. effects are in a ratio compounded of the maſſes and ſpaces. Therefore a ratio compounded of the times and forces, is equal to a ratio compounded of the maſſes and ſpaces. Wherefore forces are in a ratio compounded of the maſſes and ſpaces directly, and of the times reciprocally, that is, in a ratio compounded of the maſſes and velocities. *Q. E. D.*

Part of a letter from Mr Turberville Needham *to* James Parſons, *M. D. F. R. S. of a new* Mirror, *which burns at* 66 *feet diſtance, invented by M.* de Buffon, *F. R. S. and Member of the* R. Acad. *of* Sciences *at* Paris. No. 483. P. 493. Mar. *&c.* 1747. *Read* April 30. 1747.

IV. 1. I have been at the king's garden, and am juſt returned: I there learned, that this morning they have been trying ſome experiments with a new-conſtructed reflecting mirror or mirrors with ſucceſs: I knew indeed ſome time ago, that they had been upon the deſign; and M. *de Buffon* had acquainted me with the theoretical part of the whole. I had even ſeen a part of it executed; but as they had not then eſſayed it, I would take no notice of it: In one word, it is *Archimedes* revived; and the credit of antiquity, in this point, is in ſome meaſure re-eſtabliſhed. This machine, for ſo I muſt call it, conſiſts of 140 ſmall plain mirrors, each of about 4 by 3 Inches ſquare; they are fixed at about $\frac{1}{4}$ of an Inch diſtance from each other, upon a large wooden frame about 6 feet ſquare, ſtrengthened with many croſs bars of wood for the mounting of theſe mirrors. Each of them has three moveable ſcrews, which the operator commands from behind, ſo contrived, that the mirror can be inclined to any angle in any direction that meets the ſun; and by this means the ſolar image of each mirror is made to coincide with all the reſt.

There are in all, as I told you, 140 mirrors; but they tried the experiment this morning with 24 only; for ſo many, and no more, were then ready for the purpoſe: the effect was, that, in very few ſeconds of time, a combuſtible matter they had prepared with pitch and tow, daubed upon a deal-board, was ſet on fire, and burn'd vigorouſly at the diſtance of 66 *French* feet. Judge now of the effect 140 will produce; and

and whether the invention may not be improved to the height of all that has been advanced of *Archimedes* by the Ancients. The only difficulty they found was, to make the ſolar images of the mirrors coincide; but this is owing to the yet imperfection of their method of mounting, which may be eaſily improved.

The dimenſions I have given in of the mirrors and frame were only gueſſed at from view, for I have not meaſur'd them; ſo you muſt not expect they will ſquare or tally mathematically in the utmoſt rigour. Nor indeed did I think it neceſſary to do any more; for the dimenſions of themſelves are purely arbitrary.

Extract of a letter from the Marquis Nicolini, *F. R. S. to the* Pref. *concerning the ſame* Mirror *burning at* 150 *Feet Diſtance.* ibid. p. 495. *Read* April 30. 1747.

2. You know that the affair of *Archimedes* ſetting the *Roman* fleet on fire by means of burning-glaſſes, has been look'd upon as a thing impoſſible and romantic. *Deſcartes* poſitively denied the fact, which had been believed for ſo many ages; and our modern philoſophers, after many trials, and various reaſonings, have been of the ſame opinion. But M. *de Buffon*, being aſked if it might be poſſible to invent a *Phaometer*, or *machine* for meaſuring the *intenſity* of *Light*, hath diſcovered by trial, that light was able to produce great effects in a *focus* at a great diſtance, if one made uſe of a great numbers of diſks, which would reflect ſo many images of the ſun and fling them all into one place. He put together therefore a ſort of *Polyedron*, conſiſting of 168 ſmall mirrors, or flat pieces of looking-glaſs, each 6 inches ſquare; by means of which, with the faint rays of the ſun, in *March*, he ſet on fire ſome boards of beech wood at 150 feet diſtance. By increaſing the numbers of mirrors, he hopes to be able to do the ſame 900 feet off.

His machine has beſides, the conveniency of burning downwards or horizontally, as one pleaſes; and it burns either in it's greater *focus*, or in any nearer interval, which our commonly known burning-glaſſes have not, their *focus* being fix'd and determined.

Perhaps this machine may afford a manner of meaſuring either light, or the different degrees of heat of burning bodies. The difficulty is to find the method of marking the degrees, and of fixing a point of compariſon; for the point of kindling will not determine it; becauſe that chiefly depends upon the greater or leſs degree of inflammability of different combuſtible bodies*.

Abſtract from a letter ſent by M. Buffon, *Memb. of the* R. Acad. *of* S. *at* Paris, *&c. to* M. Folkes, *Eſq; Pr. R. S. concerning his Re-invention of* Archimedes's burning

3. As what I read ſome time ſince to our *Royal Academy* upon the ſubject of my re-invention of *Archimedes*'s burning *Specula*, cannot appear in our Memoirs before the year 1747, I think of publiſhing by themſelves my obſervations upon theſe mirrors, as ſoon as I ſhall ſatisfy myſelf upon certain particulars, by ſome new experiments I am now prepar-

* Mr *Maupertuis*, in a letter to the *Preſident*, dated at *Potzdam*, *May* 20. 1747. ſays, that his friend *Buffon* has recovered the burning-glaſſes of *Archimedes*; that with 168 plane glaſſes, each 6 inches ſquare, he has melted a ſilver plate, at the diſtance of 60 feet, and fired pitch'd boards at 150. Each *ſpeculum* is moveable, ſo as, by the help of 3 ſcrews, to be ſet to a proper inclination for directing the rays towards any given point.

Specula. N°. 489. p. 504. Oct. *and* Nov. 1748. *Read* Oct. 27. 1748.

ing to make. The *speculum* I have already constructed, and which is but 6 feet broad and as many high, burns wood at the distance of 200 feet, it melts tin and lead at the distance of above 120 feet, and silver at 50. The theory which led me to this discovery is founded upon two important remarks; the one, that the heat is not proportional to the quantity of light; and the other, that the rays do not come parallel from the sun. The first of those, which appears to be a paradox, is nevertheless a truth of which one may easily satisfy one's self, by reflecting that heat propagates itself even within bodies; and that when one heats at the same time a large superficies, the firing is much quicker than when one only heats a small portion of the same.

The motion of Projectiles *near the Earth's Surface consider'd, independent of the properties of the Conic Sections; in a letter to* M. Folkes, *Esq*; *Pr. R. S. by Mr* Tho. Simpson, *F. R. S.* N°. 486. p. 137. Feb. *and* Mar. 1748. *Read* Feb. 4. 1747.

V. After so much as has been already said upon the motion of projectiles *in vacuo*, it may seem needless to attempt any thing further on that head; nevertheless, as a thorough knowledge in the art of Gunnery is become more than ever necessary, and as gentlemen employ'd in the practice of that art are (I am sensible) too often deterr'd from applying themselves to the theory, by the difficulties they imagine they shall meet with in the conic sections, you will, I hope, pardon the liberty I have taken, in troubling you with my thoughts on a subject, in which little or nothing new is to be expected besides the method.

When I first drew up this paper (which was about two years ago) I did intend, had health permitted me to make the proper experiments, to have also attempted something with respect to the resistance of the atmosphere, whereof the effects are indeed too considerable to be intirely disregarded: but if the amplitude of the projection, answering to one given elevation, be first determined by experiment (which our method supposes) the amplitudes in all other cases, where the elevations and velocities do not very much differ from the first, may be determined, by the proportions here laid down, to a sufficient degree of exactness. Because, in all such cases, the effects of the resistance will be nearly as the amplitudes themselves; and were they accurately so, the proportions of the amplitudes, at different elevations, would be exactly the same as *in vacuo*; which proportions I now proceed to determine.

Prob. I. *Let two balls be projected with the same celerity at different, but given elevations, 'tis proposed to determine the ratio of the times of their flight, of their greatest altitudes, and of their horizontal amplitudes.*

Fig. 44. Let Pq, Fig. 44. represent the plane of the horizon, PEQ and peq the paths of the projectiles, described in the flight; moreover let QPT and qpt be the given angles of elevation, and let PQ and pq be bisected in H and h; drawing HE, QT he and qt, all perpendicular to Pq: and making the sine of $QPT = S$, it's co-sine $= C$, the sine $qpt = s$, it's co-sine $= c$, and radius $= r$.

Therefore,

Therefore, ſince the diſtances deſcended by heavy bodies (whether from a point at reſt, or from the right lines in which they *would* move, if not acted upon by gravity) are known to be as the ſquares of the times, $\mathcal{Q}\mathcal{T}$ will be to qt, as the ſquare of the time of deſcribing $PE\mathcal{Q}$ (or of that wherein the ball would move uniformly over the ſpace $P\mathcal{T}$ with it's firſt velocity at P) is to the ſquare of the time of deſcribing peq (or of that wherein the other ball would move uniformly thro' the length pt). But the celerities at P and p being equal, by hypotheſis, the times in which the ſaid lines $P\mathcal{T}$ and pt would be uniformly deſcribed, are manifeſtly, as the lines themſelves: whence the ſquares of thoſe lines muſt, alſo, be as the ſquares of the times, and, conſequently, as the diſtances deſcended: that is, $P\mathcal{T}^2 : pt^2 :: \mathcal{T}\mathcal{Q} : tq$.

Now, by plane trigonometry $\mathcal{T}\mathcal{Q} = \frac{S \times P\mathcal{T}}{r}$ and $tq = \frac{s \times pt}{r}$; therefore $P\mathcal{T}^2 : pt^2 \left(:: \frac{S \times P\mathcal{T}}{r} : \frac{s \times pt}{r} \right) :: S \times P\mathcal{T} : s \times pt$; whence, by dividing the antecedents by $P\mathcal{T}$, and the conſequents by pt, we have $P\mathcal{T} : pt :: S : s$; from which it appears, that the times of flight are directly as the ſines of elevation.

Again, the times of deſcribing $E\mathcal{Q}$ and eq (which are the halves of the wholes) being alſo to one another as $S : s$, and the diſtances EH, eh deſcended in them, as the ſquares of the times, it likewiſe follows, that $S^2 : s^2 :: EH : eh$; or that the greateſt altitudes are as the ſquares of the ſines of elevation.

Moreover, becauſe (by Trigonometry) $P\mathcal{T} = \frac{r \times P\mathcal{Q}}{C}$ and $pt = \frac{r \times pq}{c}$, and it has been already proved, that, $S : s :: P\mathcal{T} : pt$, it follows, that $S : s :: \frac{r \times P\mathcal{Q}}{C} : \frac{r \times pq}{c}$; whence, by multiplying the antecedents by $\frac{2C}{r}$ and the conſequents by $\frac{2c}{r}$, it will be $\frac{2SC}{r} : \frac{2cs}{r}$ $(:: 2P\mathcal{Q} : 2pq)$ $:: P\mathcal{Q} : pq$. But $\frac{2SC}{r}$ is known to be the ſine of double the angle whoſe ſine is S, and co-ſine C, &c. Therefore the horizontal amplitudes are to one another, as the ſines of the double elevations.

Hence it follows, that the greateſt amplitude poſſible will be, when the elevation is $\frac{1}{2}$ a right angle, or 45° (becauſe the ſine of 90° is the greateſt of all others). *Coroll.* 1.

Therefore, if the greateſt amplitude be given (from experiment) the amplitude anſwering to any propoſed elevation, above, or below, 45°, may from hence be found: for it will be as the radius, to the ſine of double *Corol.* 2.

double the given elevation, ſo is the greateſt, to the required, amplitude.

Corol. 3. Hence, alſo, the altitude of the projection may be known; for $\mathcal{Q}T$, when the angle $\mathcal{Q}PT$ is half a right angle, will be $= P\mathcal{Q}$; and therefore $HE\ (\frac{1}{4}\mathcal{T}\mathcal{Q}) = \frac{1}{4}P\mathcal{Q}$; alſo, in this caſe, $S^2 = \frac{1}{2}r^2$; whence our proportion $S^2 : s^2 :: HE : he$ will here become $\frac{1}{2}r^2 : s^2 :: \frac{1}{4}P\mathcal{Q} : he$; from whence it appears, that, as the ſquare of the radius is to the ſquare of the ſine of any given elevation, ſo is half the greateſt horizontal amplitude, to the altitude of the projection. Hence it alſo follows, that the height to which the ball would aſcend, if projected directly upwards, is juſt half the greateſt amplitude.

Corol. 4. Therefore, ſince it is well known, that a body *in vacuo* aſcends and deſcends with the ſame velocity; and that the diſtances deſcended are as the ſquares of the velocities; it follows, that the amplitudes, at the ſame elevation, with different velocities, will alſo be to one another as the ſquares of the velocities; becauſe they are as the greateſt amplitudes, with the ſame velocities (by *Corol.* 2.) and theſe are as the diſtances perpendicularly deſcended (*by the precedent*). Whence, *univerſally*, if both the elevations and the velocities differ, the amplitudes will be to each other in a *ratio* compounded of the *ratio's* of the ſines of double the angles of elevation, and of the duplicate *ratio's* of the velocities, or impelling forces.

PROB. II. *The angle of elevation, and the greateſt horizontal amplitude, being given, to find at what diſtance the piece ought to be planted, to hit an object, whoſe diſtance, above or below the plane of the horizon, is alſo given.*

Fig. 45, 46. Let AB, *Fig.* 45, 46. be the plane of the horizon, BC the perpendicular height or depreſſion of the object, and AB the required diſtance: alſo let BC be produced to meet the line of direction AD in D, and let P be the place where the path of the projectile would meet the horizon; moreover, let $P\mathcal{Q}$ be perpendicular to AP, and CN parallel to AD. Then, by the preceding problem, it will be as radius : the ſine of $2BAD$: : the given (or greateſt) amplitude : AP; which therefore, is known.

Moreover, the areas of ſimilar triangles being as the ſquares of their homologous ſides, we have $AP \times P\mathcal{Q} : AB \times BD :: A\mathcal{Q}^2 : AD^2$. But $A\mathcal{Q}^2 : AD^2 :: AB \times BD :: \mathcal{Q}P : DC$ (from principles already explained) therefore, by equality, $AP \times P\mathcal{Q} : AB \times BD :: \mathcal{Q}P : DC$; and conſequently $AP : AB :: BD : CD$; but (becauſe of the parallel lines CN and AD) $BD : CD :: AB : AN$; whence, again by equality, $AP : AB :: AB : AN$; therefore, by diviſion, $AP : BP :: AB : BN$; and, conſequently $AP \times BN = BP \times AB$.

Let AP be now biſected in O; then $BP \times AB$ being $= AO^2 - OB^2$ (in the firſt caſe) and $= OB^2 - AO^2$ (in the ſecond caſe), we ſhall therefore

therefore have $OB^2 = AO^2 \mp AP \times BN = AO \times \overline{AO \mp 2BN}$: whence the distance AB is likewise known. *Q. E. I.*

Hence, if the elevation, and the greatest amplitude, together with the distance AB of the object be given, the height or depression of the ball in the perpendicular BCD will be known: for it is proved, that $AP : BP :: BA : BN$; whence BN is known: but, as the radius to the tangent of BNC (BAD): so is BN to BC. *Corol.*

The greatest horizontal amplitudes of the piece, together with the distance and height (or depression) of the object being given, to find the direction or angle of elevation. PROB. III.

Let BC, *Fig.* 47, 48. be the perpendicular height or depression of the object, AB it's given horizontal distance, and AH the required direction; also let PQ *Fig.* 49. be the greatest amplitude (answering to 45° of elevation); draw AC, in which produced (if need be) take $AG = PQ$; make MGO perpendicular to AG, meeting AB produced (if need be) in O; and from the centre O, with the interval OA, let a circle be described, intersecting AG, produced in E, and the line of direction AD in H; join E, H, and let HI, AN and QR, be perpendicular to AE, AO, and PQ respectively, and let BC, produced, meet AH in D. Fig. 47, 48. Fig. 49.

It will appear, from what has been said above, that $AD : PR^2 :: DC : RQ$; therefore PR being $= 2PQ = 2AG = \frac{1}{2}AE$, and $RQ = PQ = \frac{1}{2}AE$ (by construction), we have $AD^2 : \frac{1}{2}AE :: DC : \frac{1}{2}AE$, and therefore $AD^2 = AE \times DC$.

Now, the triangles ADC, AEH, being equiangular (because $ADC = DAN = AEH$, and DAC common to both) we likewise have $AD : DC :: AE : EH$, and consequently $AE \times DC = AD \times EH = AD^2$ (*per* above); whence $EH = AD$. Therefore, as the triangles ADB and EHI are equiangular, they are equal in all respects; and so $HI = AB$: whence follows this easy construction.

Having described the circle AEF, as above directed, and drawn MG perpendicular to AE, take Gn equal to AB, and thro' n, parallel to AE, draw Hh, cutting the circle in H and h; join A, H, and A, h; then either of the directions AH or Ah, will answer the conditions of the problem. From this construction we have the following calculation, *viz.* *Construction.*

As AB is to BC, so is AG to OG; which added to, or subtracted from, Gn (AB) gives On: then, it will be, as $AG : On ::$ the co-sine of OAG : co-sine of HOn ($= HAh$) the difference of the two required elevations; whence the elevations themselves are known. *Q. E. I.*

Hence, if the elevation of the piece, with the distance and the height (or depression) of the object be given, the greatest horizontal amplitude may be found: for it will be $AB : BC ::$ radius : tang. of BAC; whence CAD is also known. *Corol.*

Then, $S.CAD : S.ACD$ (AHE) $:: AD$ (HE) $: AE$.

And, $S.ADC$: radius $:: AB : AD$.

Therefore,

Therefore, by compounding theſe proportions, we have $S.CAD \times S.ADC$: radius $\times$ $S.ACD$: : $AB : AE$; which is equal to twice the required amplitude, by conſtruction.

Corol. 2. Moreover, if the elevation, and the greateſt horizontal amplitude be given, the amplitude of the projection on any aſcending or deſcending plane AE, whoſe inclination FAE is alſo given, may from hence be derived. For, $S.AHE$ $(ACD) : S : EAH$ (CAD) : : AE $(2PQ) : EH$ (AD) and $S.ACD : S.ADC$: : $AD : AC$; whence, by compounding the two proportions, *Sq*. $S.ACD : S.CAD \times S.ADC$: : $2PQ : AC$; from which AC is known.

Corol. 3. Since it appears, that the triangles ADB and EHI are equal and alike in all reſpects, and therefore, the horizontal diſtance AB, *univerſally*, equal to the perpendicular HI, it is manifeſt, that, when HI is the greateſt poſſible, AB will alſo be the greateſt poſſible; in which circumſtance AC (if the angle FAE be given) will likewiſe be the greateſt poſſible: and this, it is evident, muſt be, when HI coincides with MG, or when the angles HEA and HAE are equal, *Fig.* 50, 51. at which time the point D coincides with H; becauſe AD and EH are always equal to each other. Therefore, ſince, in this caſe, HAE (HEA) is $= NAH$, it follows, that the amplitude, on any inclined plane AE, will be the greateſt poſſible, when the line of direction AH biſects the angle made by the plane and zenith.

Fig. 50, 51.

Corol. 4. Hence the greateſt amplitude on any inclined plane may alſo be known; for the right-angled triangles AOG and HOB, having $AO = HO$ and the angle O common, are equal in all reſpects; and therefore, as tang. of AHG (BAH the angle of elevation) : tang. of CHG (CAB the plane's inclination) : : $AG : GC$; whence $AC = AG \mp GC$ is alſo known.

Corol. 5. Hence, alſo, if the greateſt amplitude on an inclin'd plane be given, the greateſt horizontal amplitude may be determined: for, radius : $S.BAC$: : $AC : BC = CG =$ the difference of the given, and the required, amplitudes.

Corol. 6. But if, inſtead of the plane's inclination, the perpendicular height, or depreſſion, of the object be given; then, AC $(AG \mp BC)$ being to BC, as radius to the ſine of BAC, and radius : co-tang. BAC : : $BC : AB$; the greateſt diſtance AB, at which the ball can poſſibly hit the object, will from hence be given: which diſtance (becauſe $AC = AG \mp BC$, and $AB^2 = \overline{AC + CB} \times \overline{AC - BC}$) will alſo be expreſſed by $\sqrt{AG \times \overline{AG \mp 2BC}}$. Hence the greateſt horizontal amplitude of a ball, projected from a given height above the plane of the horizon is known: for ST, *Fig.* 51. may here be ſuppoſed to repreſent, the plane of the horizon, and SA the given height; and then SC, being equal to AB, is given from above $= \sqrt{AG \times \overline{AG + 2BC}}$.

Fig. 50.

But,

But, if the horizontal diſtance AB be given, and it be required to find the greateſt height the ball can poſſibly reach in the perpendicular BCD; we ſhall have $HG\ (AB) : AG ::$ radius : tang. of the elevation (BAH or AHG); and radius : tang. $BAC\ (2BAH \backsim 90^\circ) ::$ $AB : BC$; which therefore is known. But (becauſe $AC \pm BC = AG$, and $\overline{AC + CB} \times \overline{AC - CB} = AB^2$) the ſame will alſo be truly exhibited by $\frac{AG^2 \backsim AB^2}{2AG}$. *Corol.* 7.

Laſtly, let the height, or depreſſion, of the object be given, together with it's diſtance AB, to determine the direction, and the leaſt *impetus* poſſible, to hit the object: then $AB : BC ::$ radius : tang. BAC; whence the elevation BAH is known; and as radius : tang. $AHG\ (BAH) :: MG\ (AB) : AG$; whence the *impetus* is alſo known. *Corol.* 8.

VI. 1. The uſe of rockets is, or may be, ſo conſiderable in determining the poſition of diſtant places to each other, and in giving ſignals for naval or military purpoſes, that I thought it worth while to examine what height they uſually riſe to, the better to determine the extent of the country, thro' which they can be ſeen. I therefore, at the exhibition of the late fire-works, deſir'd a friend of mine, who I knew intended to be only a diſtant ſpectator, to obſerve the angle of elevation to which the greateſt part of them roſe, and likewiſe the angle made by the rocket or rockets, which ſhould riſe the higheſt of all. *Obſervations on the height to which rockets aſcend; by Mr* Benj. Robins, *F. R. S.* N°. 492. p. 131. Apr. &c. 1749. *Read* May 4. 1749.

My friend was provided with an inſtrument, whoſe *radius* was 38 inches; and, to avoid all uncertainty in it's motion, it was fixed in an invariable poſition; and it's field, which took in ten degrees of altitude was divided by horizontal threads. The ſtation my friend choſe was on the top of Dr *Niſbett*'s houſe in *Kingſtreet* near *Cheapſide*, where he had a fair view of the upper part of the building erected in the *Green Park*. There he obſerved that the ſingle rockets which roſe the moſt erect, were uſually elevated at their greateſt height about $6^\circ \frac{1}{4}$ above his level; and that amongſt theſe there were 3 which roſe to $7^\circ \frac{1}{2}$; and that in the laſt great flight of rockets, ſaid to be of 6000, the creſt of the arch, formed by their general figure, was elevated about $8^\circ \frac{3}{4}$. From the care and dexterity of my friend, and the nature of the inſtrument, I doubt not but theſe obſervations are true within a few minutes.

The diſtance of this ſtation from the building in the *Green Park* is 4000 yards, according to the laſt great map of *London*: and hence it appears, that the cuſtomary height, to which the ſingle, or honorary rockets, as they are ſtyled, aſcended, was near 440 yards: that three of theſe roſe 526 yards; and that the greateſt height of any of thoſe fired in the grand girandole was about 615 yards: all reckon'd above the level of the place of obſervation, which I eſteem to be near 25 yards higher than the *Green Park*, and little leſs than 15 yards below the cheſts whence the great flight of rockets was diſcharged.

It ſeems then there are rockets which riſe 600 yards from the place whence they are diſcharged: and this being more than a third part of a mile, it follows, that if their light be ſufficiently ſtrong, and the air be not hazy, they may be ſeen in a level country at above 50 miles diſtance.

The obſervations on the ſingle rockets are ſufficiently conſonant to ſome experiments I made myſelf about a fortnight ſince: for then I found that ſeveral ſingle pound rockets went to various heights between 450 and 500 yards, the altitude of the higheſt being extremly near this laſt number, and the time of their aſcent uſually ſhort of 7″.

But though from all theſe trials it ſhould ſeem as if good rockets of all ſizes had their heights limited between 400 and 600 yards; yet I am diſpoſed to believe, that they may be made to reach much greater diſtances. This I in ſome degree collect from the nature of their compoſition, and the uſual imperfect manner of forming them.

Nor is this merely matter of ſpeculation; for I lately ſaw a dozen of of four pound rockets fired; the greateſt part of which took up near 14″ in their aſcent, and were totally obſcured in a cloud near 9 or 10″ of the time; ſo that the moment of their burſting was only obſervable by a ſudden glimmering through the clouds: and as theſe rockets, during the time they were viſible, were far from moving with a languid motion, I cannot but conceive, that the extraordinary time of their aſcent muſt have been attended by a very unuſual riſe.

An account of ſome Experiments, made by Benj. Robins, *Eſq; F. R. S. Mr* Samuel Da Coſta, *and ſeveral other Gentlemen, in order to diſcover the height to which* Rockets *may be made to aſcend, and to what diſtance their* Light *may be ſeen; by Mr* John Ellicott, *F. R. S.* N°. 496. p. 578. Nov. &c. 1750. *Read* Dec. 13. 1750.

2. Mr *Robins* not having been able to obtain any certain account to what diſtance any of the rockets mentioned in the preceding paper were actually ſeen, reſolved to order ſome rockets to be fired at an appointed time, and to deſire ſome of his friends to look out for them at ſeveral very diſtant places.

The places fix'd upon for this purpoſe, were *Godmarſham* in *Kent*, about 50 Miles diſtant from *London*; *Beacon-Hill* on *Tiptery-Heath* in *Eſſex*, at about 40 miles; and *Barkway*, on the borders of *Hertfordſhire*, about 38 miles from *London*.

Mr *Robins* accordingly order'd ſome rockets to be made by a perſon many years employ'd in the Royal Laboratory at *Woolwich*; to which ſome gentlemen, who had been inform'd of his intentions, added ſome others of their own making. *Sept.* 27, 1749. at 8 in the evening, was the time appointed for the firing of them; but, thro' the negligence of the engineer, they were not let off till above ½ an hour after the time agreed upon. There were in all a dozen rockets fired from *London Field* at *Hackney*; and the heights were meaſur'd by Mr *Canton*, Mr *Robins* being preſent, at the diſtance of about 1200 yards from the poſt from whence the rockets were fir'd. The greateſt part of them did not riſe to above 400 yards; one to about 500, and one to 600 yards nearly.

By a letter I receiv'd the next day from the Rev. Dr *Maſon*, of *Trin. Coll. Cambridge*, who had undertaken to look out for them from *Barkway* on the borders of *Hertfordſhire*, I was informed, that, having waited upon

upon a hill near the town with ſome of his friends till about half an hour paſt the time appointed, without perceiving any rockets, as they were returning to the town, ſome of the company ſeeing thro' the trees what they took to be a rocket, they immediately haſten'd back out of the cloſes into the open fields, and plainly ſaw 4 riſe, turn and ſpread: he judged they roſe about 1° above the horizon, and that their lights were ſtrong enough to have been ſeen much farther.

From *Eſſex* I was inform'd, that the perſons on *Tiptery-Heath* ſaw 8 or 9 rockets very diſtinctly, at about ½ an hour paſt 8; and likewiſe greatly to the eaſtward of theſe 5 or 6 more. The gentlemen from *Godmarſham* in *Kent* having waited till above half an hour paſt 8, without being able to diſcern any rockets they fir'd half a dozen; which, from the bearings of the places were moſt probably thoſe ſeen to the eaſtward by the perſons upon *Tiptery-Heath*; and if the ſituations, as laid down in the common maps, are to be depended upon, at about 35 miles diſtance.

The engineer being of opinion that he could make ſome rockets, of the ſame ſize as the former, that ſhould riſe much higher, Mr *Robins* order'd him to make half a dozen. Theſe laſt were fired *Oct.* 12. following, from the ſame place, and in general they roſe nearly to the ſame heights with the foregoing; excepting one, which was obſerved to riſe 690 yards. The evening prov'd very hazy, which render'd it impoſſible for them to be ſeen to any conſiderable diſtance.

It being obſerv'd in theſe trials, that the largeſt of the rockets, which were about 2 inches and a half in diameter, roſe the higheſt, Mr *Robins* intended to have made ſome more experiments, in order to a farther diſcovery what ſiz'd rockets would riſe higheſt: but his engagements with the *Eaſt-India* company preventing him, Mr *Samuel Da Coſta*, late of *Devonſhire-Square*, a gentleman of an extraordinary genius in Mechanicks, and indefatigable in the application; Mr *Banks*, a gentleman who had for many years practis'd making rockets, and two other perſons, undertook the proſecuting theſe enquiries; and having made ſeveral experiments as well with regard to the compoſition, as the length which rockets might be made to bear, in proportion to their diameters, and of different-ſiz'd rockets from 1 ½ to 4 inches diameter, they intended this winter to have made trial of ſome of a yet greater diameter, had not the death of Mr *Da Coſta* prevented it.

I ſhall therefore beg leave to give ſome account of the ſucceſs which has hitherto attended their undertaking, ſo far as they went: and as it has been much beyond what was expected, I am in hopes this ſhort relation will not prove unacceptable.

Amongſt ſome rockets fired in the laſt ſpring, there were two made by Mr *Da Coſta* of about 3 ½ inches diameter, which were obſerved to riſe, the one to about 833, the other 915 yards. At a ſecond trial, made ſome time after, there was one made by Mr *Da Coſta*, of 4 inches diameter, which roſe to 1190 yards. The laſt trial was made the latter end of *April* 1750, where 28 rockets were fir'd in all, made by different perſons, and of different

ferent ſizes, from 1 ½ to 4 inches diameter; the moſt remarkable of each ſize were as follows; one of 1 ½ inch roſe to 743 yards; one of 2 to 659; one of 2 ½ to 880; another of the ſame ſize, which roſe to 1071; one of 3 to 1254; one of 3 ½ to 1109; and one of 4 inches; which, after having riſen to near 700 yards, turned, and fell very near the ground before it went out. Theſe were all made by Mr *Da Coſta*. Beſides theſe, there was one of the rockets of 2 ½ inches in diameter, which roſe to 784 yards, and another made by Mr *Banks* of the ſame ſize to 833.

As the making of large rockets is not only very expenſive, but likewiſe more uncertain than thoſe of a leſſer ſize; ſo from the laſt experiments it is evident, that rockets from 2 ½ to 3 ½ inches diameter, are ſufficient to anſwer all the purpoſes they are intended for; and I doubt not may be made to riſe to an height, and to afford a light capable of being ſeen to conſiderably greater diſtances than thoſe before-mention'd.

Before I conclude this account, it may not be improper to take notice, that, tho' the heights of the rockets are ſet down to a ſingle yard, it is not pretended the method made uſe of (tho' ſufficient for all the purpoſes of theſe experiments) is capable of determining the heights to ſo great an exactneſs: for as they were meaſur'd by only one obſerver, it is evident that, if any of the rockets deviated from the perpendicular, ſo as either to incline towards the place of obſervation, or to decline from it, the height would be given either greater or leſs than the truth; but as the the baſe upon which they were meaſur'd was 1190 yards, the greateſt error that can ariſe on this account will be but very inconſiderable. If we ſhould ſuppoſe there might be an error of 30 or even 50 yards, which is very highly improbable, it muſt then be allowed, that ſeveral of theſe rockets roſe to 1000 yards, one to 1100, and another to 1200 yards, or double to any of thoſe fired in the *Green Park*.

I have been informed that the relation of this affair has appeared ſo very extrodinary to ſome gentlemen converſant in ſuch matters, that they have mention'd it as their opinion, that there muſt certainly have been ſome miſtake, either in placing the inſtrument, taking the heights, or otherwiſe. In anſwer to which I would obſerve, that, in all the experiments mentioned in this paper, the heights were all taken by the ſame perſon, *viz.* Mr *John Canton*, and that the laſt trial was made in the preſence of ſeveral very worthy members of this *Society*. That the inſtrument, being firſt fixed to a proper angle was not alter'd during the whole time of trial; and therefore, if there had been any miſtake in fixing it, that miſtake would have varied the height of all the rockets as much as thoſe of Mr *Da Coſta*'s; but it was thoſe of Mr *Da Coſta*'s only, and that at 3 different trials, which roſe to ſuch extraordinary heights; and therefore I think we have ſufficient reaſon to conclude that their meaſures were certainly taken very near the truth.

Fig. 52. *Deſcription of a Machine to*

VII. *ABCD* is a pit dug in the ground, whoſe ſurface is higher at *D* than on the other ſide at *A*. The bottom *BC* is ſtrongly ramm'd with clay, upon which are laid thin ſawen deals.

In

blow fire *by the fall of* water; *by* James Stirling, *F. R. S.* N°. 475. p. 315. Jan. *&c.* 1745. *Read* March 21. 1744-5.

In this pit is fixed a tub *GHKI* without a bottom, having a hole *I* at the lower part of the ſide, and all round the tub is ramm'd with clay, except at the hole *I*.

In the middle of the upper end of the tub is fixed a pipe *PQRS*; at the higher end of which are four holes pointing downwards, whereof two are repreſented by *S* and *R*,

SRTU is a funnel fixed on the top of the pipe, with a throat *XZ* narrower than the bore of the pipe. In the upper end of the tub towards one ſide is fixed a crooked pipe at *LM*, tapering to the end at *N*. It is made of wood ſo far as *O*, but from *O* to *N* of iron, the fire being ſuppoſed at *N*. *EF* is the ſurface of a plain ſtone, raiſed up in the middle of the tub, directly under the pipe *PQRS*.

The running water, being let in at the top of the funnel, falls thro' the pipe upon *EF* the ſtone in the tub; it runs out at the hole *I*, but cannot get off till it riſes as high as *A*.

This raiſes it in the tub almoſt up to the ſurface of the ſtone, and it muſt not riſe higher.

So much water muſt run in at the top of the funnel, as will keep it always full, or nearly ſo.

This height of water ſqueezes it into the pipe with a great velocity; but, ſince it paſſes thro' the throat of the funnel, which is of a ſmaller bore than the pipe, room is left all round the vein of water for the air to enter at the air-holes.

It no ſooner enters but it mixes with the water, on the account of the rapidity of the motion; and both together make a white froth, and intirely fill the bore of the pipe. When this froth falls on the ſtone in the tub, it is daſhed into ſmall particles, which diſengages the air from the water. The air cannot get out at *PQ*, the end of the pipe, becauſe it is fill'd with the froth, which falls with a great force; neither can it get out at the hole *I*, becauſe the ſurface of the water is kept ſo high above it; and for that reaſon it ruſhes out at *N*; and if the hole *N* be ſtopped, the air will ſoon force all the water in the tub out at *I*, and then follow it.

The moſt convenient way of regulating the blaſt, is to bore a ſmall hole in the Blaſt-pipe; and, by the help of a pin in it, to let out what air there may be more than is wanted.

The dimenſions of ſuch an engine ſufficiently big to ſmelt harder ore than any in lead-hills, are ſet down at the Bottom.

	Feet.
Height of the funnel	5
Length of the pipe	14, 15, or 16
Height of the tub	6
Diameter of the tub	5½
Height of the ſtone in the tub	2

Diameter

	Inches.
Diameter of the throat of the funnel - - - - - - - - -	$3\frac{1}{2}$
Diameter of the bore of the pipe - - - - - - - - - -	$5\frac{1}{2}$
Diameter of the blaſt-hole at *N* - - - - - - - - - -	$1\frac{1}{2}$
Hole at *I* about 5 inches ſquare,	
Diameter of the air-holes - - - - - - - - - - - -	$1\frac{1}{4}$

This engine is likewiſe of admirable uſe to convey freſh air into the works; which ſaves the double drifts and ſhafts, and cutting communications between them.

A ſmall one will do very well for a Black-ſmith.

Tables of Specific Gravities, *extracted from various authors, with ſome obſervations upon the ſame; in a letter to* M. Folkes, *Eſq;* P. R. S. *by* Rich. Davis, *M. D.* N°. 488. p. 416. June 1748. *Preſented* Feb. 18. 1747.

VIII. The manifold applications which may be made, for the purpoſes of Natural Philoſophy, of the relations which bodies bear to each other, by their reſpective ſpecific gravities, engaged me ſome years ſince to collect all the experiments of this ſort I could meet with in the courſe of my ſtudies, and alſo to make ſeveral new ones of my own with the ſame deſign.

When my collection began to be ſomewhat conſiderable, I diſpoſed the ſeveral bodies in tables according to their ſpecies, which I found to be the moſt convenient method, as my tables were by this means capable of receiving additions in any part, without deſtroying the form of the whole: and as they were thereby eaſy and ready to be conſulted, and well diſpoſed for the forming of immediate compariſons between the ſeveral bodies of the ſame ſpecies.

But having now no farther opportunities of enlarging my collection, I hereby beg leave to recommend the proſecution of my deſign to others, as a ſubject well deſerving the attention of ſome of the members of the *Royal Society*, to whom I therefore preſent theſe my tables: wiſhing they may prove of ſome uſe and ſervice to the inquiſitive and philoſophical part of the world. As I perſuade myſelf they really will, when they ſhall be further rectified by the omiſſion of the erroneous or uncertain experiments; when they ſhall be enlarged by the addition of ſuch others, as may ſtill be found in good authors, or which yet remain unpubliſhed in the cloſets of the Curious: and eſpecially if ſome ſuch gentlemen as have ſkill, leiſure, and opportunities, ſhall pleaſe to ſupply their remaining defects, by the communication of their own obſervations, made upon thoſe bodies, whoſe ſpecific gravities have not as yet been carefully recorded.

Denique cur alias aliis præſtare videmus
Pondere res rebus, nihilo majore figura?
Nam, ſi tantundem eſt in lanæ glomere, quantum
Corporis in plumbo'ſt tantundem pendere par eſt. Lucret.

The

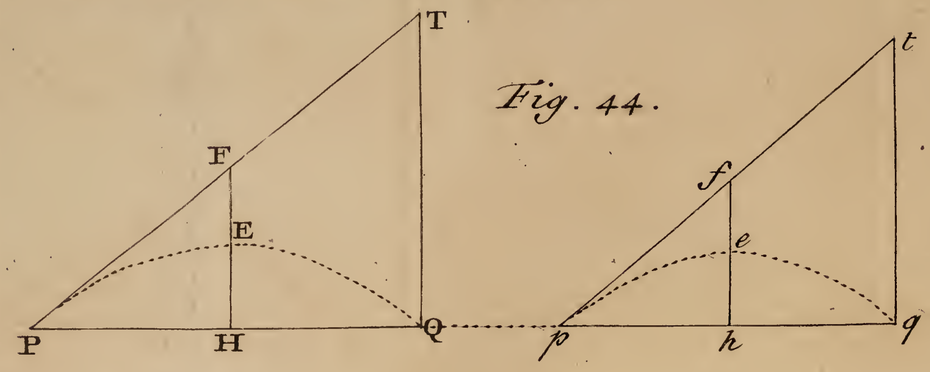


Fig. 44.

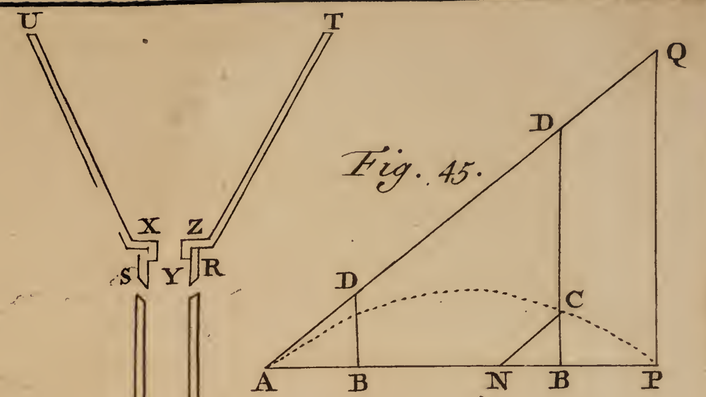


Fig. 45.

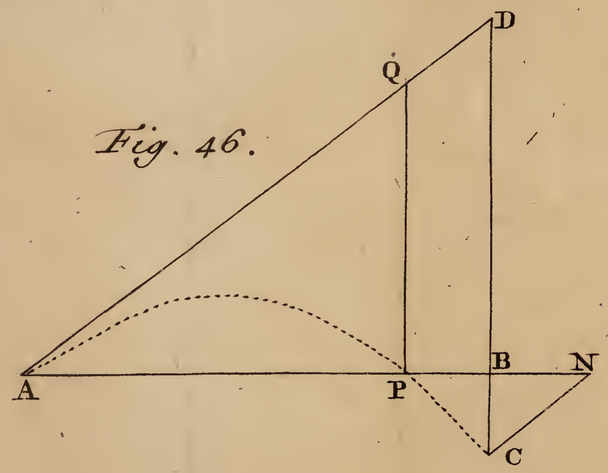


Fig. 46.

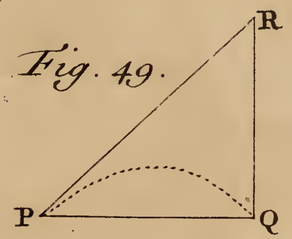


Fig. 49.

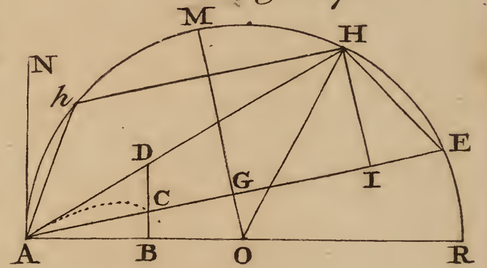


Fig. 47.

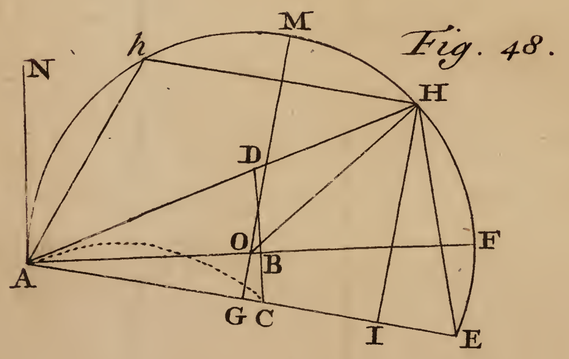
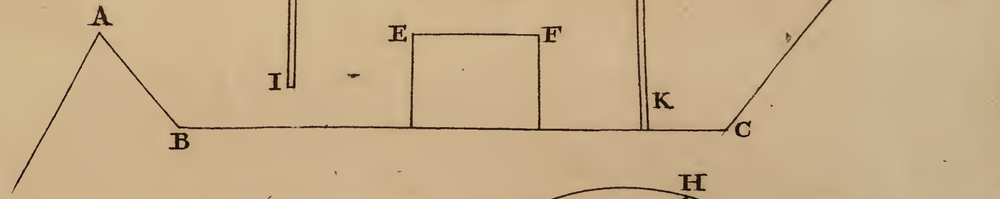


Fig. 48.



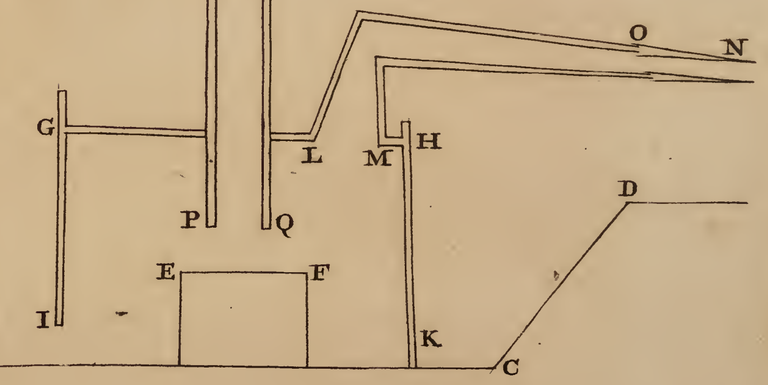


Fig. 52.

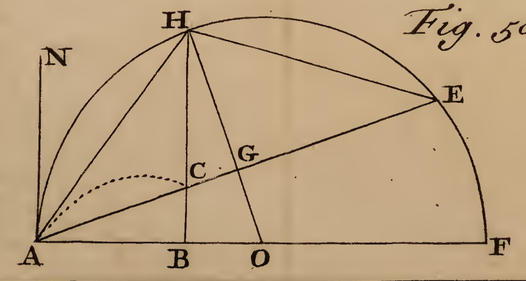


Fig. 50.

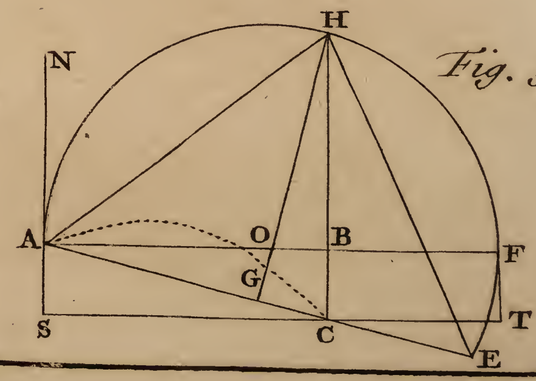
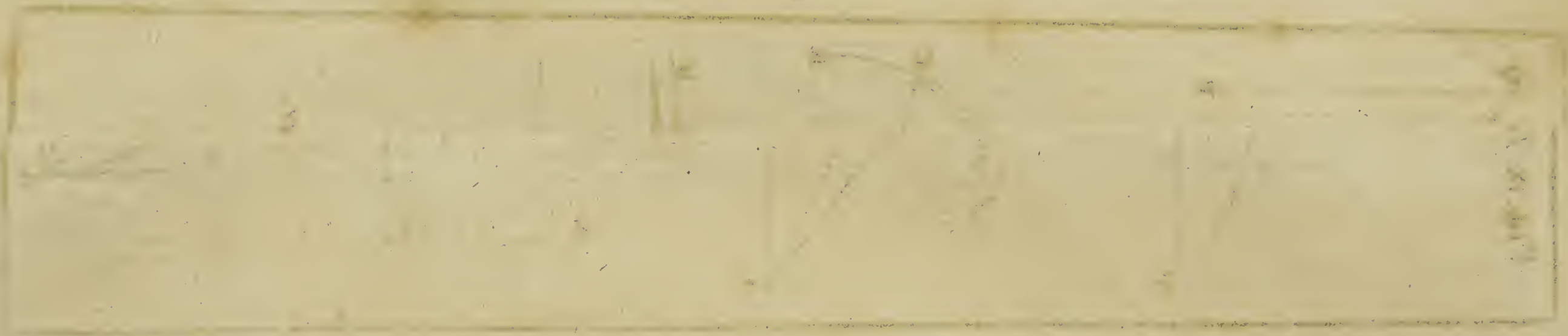


Fig. 51.



A ſhort account of the Authors, from whoſe writings and expements, the following tables have been collected, with ſome remarks upon the experiments themſelves, and the manner in which they appear to have been made.

The Antients have left but few particulars concerning the different ſpecific gravities of bodies, tho' it is plain they were in the general ſufficiently acquainted with them. It was by the knowledge of the various weights of gold and ſilver, that *Archimedes* is recorded to have detected the famous fraud committed in *Hiero*'s crown, as *Vitruvius* has at large related in his *Architecture*, l. ix. c. 13. and it is from the ſame great philoſopher, that we have derived the demonſtration of thoſe hydroſtatical rules, by which the proportions are beſt to be known, of the ſeveral weights or denſities of different bodies, having the ſame bulk or magnitude: as may be ſeen in his tract *De inſidentibus humido*, loſt in the *Greek* original, but retrieved in great meaſure, as it is ſaid, from an *Arabic* tranſlation. It was publiſhed in *Latin*, with a Commentary by *Federicus Commandinus* at *Bononia* 1565, 4^to^, and the ſubſtance of it by Dr *Barrow* in his *Archimedes*, printed likewiſe in 4^to^ at *London* 1675.

Pliny, in the xviii. book of his *Natural Hiſtory*, has ſet down the proportional weights of ſome ſorts of grain, among which he ſays that barley is the lighteſt. *Leviſſimum ex his hordeum, raro excedit, [in ſingulos nimirum modios]* xv *libras, et faba* xxii. *Ponderoſius far magiſque etiamnum triticum.* And a little further on, *ex his generibus [frumenti ſcilicet] quæ Romam invehuntur, leviſſimun eſt Gallicum, atque è Cherſoneſo advectum: quippe non excedunt in modium vicenas libras, ſi quis granum ipſum ponderet. Adjicit Sardum ſelibra, Alexandrinum et trientes: hoc et Siculi pondus. Bœoticum totam libram addit: Africum et dodrantes. In Tranſpadanâ Italiâ ſcio vicenas quinas libras farris modios pendere: circa Cluſium et ſenas.* And the ſame author in his xxxiii. book, ſpeaking of quickſilver, obſerves that it is the heavieſt of all ſubſtances, gold only excepted. *Omnia ei innatant, præter aurum: id unum ad ſe trahit.* Which *Vitruvius* had alſo taken notice of, and had mentioned beſides the weight of a known meaſure of it, that of four *Roman ſextarii. Eæ autem [guttæ nempe argenti vivi quæ inter ſe congruunt et una confunduntur] cum ſint quatuor ſextariorum menſuræ, cum expenduntur, inveniuntur eſſe pondo centum. Cum in aliquo vaſe eſt confuſum, ſi ſupra id lapidis centenarii pondus imponitur, natat in ſummo: neque eum liquorem poteſt onere ſuo premere, nec elidere, nec diſſipare: centenario ſublato, ſi ibi auri ſcrupulum imponatur, non natabit, ſed ad imum per ſe deprimetur. Ita non amplitudine ponderis, ſed genere ſingularum rerum gravitatem eſſe, non eſt negandum.* Archit. l. vii. c. 8.

Again, *Q. Rhemnius Fannius Palæmon*, in his fragment *De ponderibus et menſuris*, has given us an obſervation, of the proportional gravities of water, oil, and honey.

——Libræ, ut memorant, beſſem ſextarius addet,
Seu puros pendas latices, ſeu dona Lyæi,
Addunt ſemiſſem libræ labentis olivi,
Selibramque ferunt mellis ſupereſſe bilibri.

That

That is to ſay, that the *ſextarius* of either water or wine weighed 20 ounces, the ſame meaſure of oil 18, and of honey 30. Their ſpecific weights were therefore in proportion as 1.0, 0.9 and 1.5, exactly agreeable to what *Villalpandus* determined about the beginning of the laſt century: yet was this author himſelf ſenſible, that theſe were not to be look'd upon as very nice experiments.

Hæc tamen aſſenſu facili ſunt credita nobis.
Namque nec errantes undis labentibus amnes,
Nec merſi puteis latices, aut fonte perenni
Manantes, par pondus habent: non denique vina,
Quæ campi aut colles nuperve aut ante tulere,
Quod tibi mechanica promptum eſt depromere Muſa.

After which he proceeds to deſcribe a good pretty inſtrument for the ready finding of the different ſpecific gravities of fluids, and ſhews how thoſe of ſolids alſo may be hydroſtatically diſcovered. And ſo much ſhall ſuffice for what I had to mention from the Antients relating to this ſubject: I now come to thoſe who have written within theſe laſt hundred and fifty years.

Francis Bacon, Lord Verulam, &c. in his *Hiſt. denſi et rari*, printed in vol. ii of his works in *folio*, *Lond.* 1741. *p.* 69. has given a table, which he calls, *Tabula coitionis et expanſionis materiæ per ſpatia in tangibilibus (quæ ſcilicet dotantur pondere) cum ſupputatione rationum in corporibus deverſis.* This tract does not appear to have been publiſhed till after his death, which happened in the year 1626, but was probably written ſeveral years before; and the experiments were even as he tells us made long before that. *Hanc tabulam multis abhinc annis confeci, atque ut memini, bona uſus diligentia.* I therefore apprehend it to be the oldeſt table of ſpecific gravities now extant. The experiments therein mentioned were not made hydroſtatically, but with a cube of an ounce weight of pure gold, as he ſays, to which he cauſed cubes of other materials to be made equal in ſize: as he did alſo two hollow ones of ſilver, and of equal weights, the one to be weighed empty, and the other filled with ſuch liquid as he wanted to examine. He was himſelf ſenſible that his experiments of this ſort were, notwithſtanding his care, very defective, *poſſit proculdubio tabula multo exactior componi, videlicet tum ex pluribus, tum ex ampliore menſura: id quod ad exactas rationes plurimum facit, et omnino paranda eſt, cum res ſit ex fundamentalibus.* From among theſe, notwithſtanding their imperfection, as they appear to have been ſome of the firſt experiments of the ſort regularly digeſted, and as they were beſides made by ſo great a man, I have extracted the ſpecific gravities of the fixed metals, which I have inſerted as examples in the following tables: after reducing them to the common form, upon the ſuppoſition that pure gold was, according to *Ghetaldus*, juſt 19 times as heavy as water. And this I have rather choſen to do, than to make uſe of his Lordſhip's own weight

of

of water given in the table, which in the manner he took it could not be very exact, and which beſides would not have brought out the ſpecific gravity of pure gold more than 18 times as much; and that of the other metals in proportion. This table contains in all 78 articles.

There are alſo in the third volume of the ſame edition of his works, *p.* 223. *Certain experiments made by the Lord Bacon about weight in air and water.* Theſe are truly *hydroſtatical* but very imperfect, I have not therefore inſerted any of them in the following collection.

Marinus Ghetaldus, a nobleman of *Raguſa*, publiſhed in *quarto* at *Rome*, in 1603, his treatiſe entitled, *Promotus Archimedes, ſeu de variis corporum generibus gravitate et magnitudine comparatis*, wherein he has given a compariſon between the ſpecific gravities of water and eleven other different ſubſtances, from his own hydroſtatical experiments made with care and exactneſs. Theſe I have inſerted: expreſſing the numbers as they ſtand in his own book, but I have afterwards alſo for uniformity reduced them to the decimal form. I have beſides at the end tranſcribed at large the two tables of this author, in which every one of the 12 ſorts of bodies he treats about is ſucceſſively compared with all the others, both in weight and magnitude.

F. *Joh. Baptiſta Villalpandus*, a Jeſuit of *Cordoua* in *Spain*, in his *Apparatus Urbis et Templi Hieroſolymitani*, printed in *folio* at *Rome* in 1604, exhibited a table of the proportional weights of the 7 metals and ſome other ſubſtances, from his own experiments, made with great care as he tells us, by the means of 6 equal ſolid cubes of the fixed metals, and a hollow cubical veſſel 8 times as large, for the comparing mercury, honey, water, and oil with the ſame. His numbers, which are inſerted under his name in the following tables, were alſo again publiſhed afterwards by *Joh. Henr. Alſtedius* in his *Encyclopædia univerſa*, printed in 2 vols. in *folio*, at *Herborn* 1630, and by *Hen. Van Etten*, in his *Mathematical Recreations*, from whence they have been often tranſcribed into other books. *Villalpandus*'s book, which is only the third volume of a work begun to be publiſhed ſeveral years before, was itſelf printed ſo ſoon after *Ghetaldus*'s, that it is probable he either never ſaw that author, or not at leaſt till after his own experiments were made.

Mr *Edm. Gunter*, in his *Deſcription and Uſe of the Sector*, printed after his death by Mr. *Sam. Foſter*, in 1626, having occaſion to make mention of the ſpecific weights of the ſeveral fixed metals, quoted *Ghetaldus*, and made uſe of his proportions, and ſo did alſo Mr *Will. Oughtred*, in his *Circles of Proportion*, firſt publiſhed in 4to 1633, with this only difference, as to the form, that he changed *Ghetaldus*'s unit into 210, whereby he expreſſed all his relations in whole numbers. It is likewiſe probable that *D. Henrion* took from the ſame place the numbers he applied in his *Uſage du Compas de Proportion*, printed at *Paris* in 1631, 8vo. although he has not given them all with exactneſs, for the ſake as it ſeems of uſing ſimpler vulgar fractions.

F. *Marinus Merſennus*, a *French* Minim, in his *Cogitata Phyſico-Mathematica*, printed at *Paris* in 1644, 4^to^, has given from the obſervations of his accurate friend *Petrus Petitus*, a table of the ſpecific gravities of the metals and ſome other bodies, making gold 100, water $5\frac{1}{3}$, and the reſt in proportion. Theſe I have reduced to the common form, and inſerted under his name in the following tables. The ſame were afterwards made uſe of by F. *Francis Milliet de Chales*, Jeſuit, in his *Curſus Mathematicus*, Monſieur *Ozanam*, Profeſſor *Wolfius*, and ſeveral others. I have not ſeen *Petitus*'s own book, but it was entitled *L'Uſage ou le moyen de pratiquer par une Regle toutes les Operations du Compas de Proportion —— augmentées des Tables de la Peſanteur et Grandeur des Metaux*, &c. had a privilege dated in 1625. tho' it is ſaid not to have been printed till ſome years after. The ſame Father *Merſennus* has alſo taken notice, in his general preface, of a table of 20 ſpecific gravities, ſome time before publiſhed by M. *Aleaume*, which he there ſets down, but which he alſo obſeves to be very incorrect. I have not therefore inſerted any of them in this collection.

Mr *Smethwick*, one of the earlieſt members of the *Royal Society*, communicated to the ſame in *July* 1670, the weights of a cubic inch of ſeveral different ſubſtances; ſaid to have been formerly taken by Mr *Reynolds* in the *Tower* of *London*. This gentleman was the ſame who compoſed ſeveral tables relating to the price of gold and ſilver, which were publiſhed in a book entitled *The Secrets of the Goldſmith's Art*, at *London* 1676 *in* 8^vo^. Theſe weights are expreſſed in decimals of an *Averdupois* pound, are carried to 8 places of figures, and ſeem to have been carefully and accurately collected. I have therefore in the following tables reduced them to the common form, in order to give them their proper authority with the reſt. I am ignorant whether theſe weights were ever before printed or not, neither can I give any account, after what particular manner the experiments were made, from which they were taken. They were communicated to me from the regiſter-books of the *Royal Society*; and I ſhall only obſerve, that the abſolute weight here aſſigned of a cubic inch of common water, does not differ more than a ſmall fraction of a grain, from the weight of the ſame afterwards determined by Mr *Ward* of *Cheſter*.

The *Philoſophical Society*, meeting at *Oxford*, directed ſeveral experiments to be made hydroſtatically by their members, concerning the ſpecific gravities of various bodies; which being digeſted into a table, were by Dr *Muſgrave* communicated to the *Royal Society*, *March* 21, 1684. ſoon after which they were printed in the 169^th^ number of the *Philoſ. Tranſ.* Theſe experiments were, according to Dr *Muſgrave*, made by Mr *Caſwell* and Mr *Walker*; they are all originals, and eſteemed ſome of the moſt accurate that are extant.

The honourable *Robert Boyle*, at the end of his *Medicina hydroſtatica*, firſt publiſhed at *London* in 1690, 8^vo^. ſubjoined a table of the ſpecific gravities of ſeveral bodies, accurately taken from his own hydroſtatical experiments.

experiments. Beſides which, there are alſo in the ſame tract, and in other parts of his works, ſeveral experiments of this excellent author's, which he has given occaſionally, together with the uſes reſulting from them. To ſuch of theſe in the following collection, as were taken from the table juſt mentioned, I have barely annexed his name, but to ſuch of the others as occurred, I have alſo added the volume, page, and column, of the late *folio* edition of his works in 1744, where the ſame are to be found. It may be noted, that in the firſt edition of the *Medicina hydroſtatica*, there were ſeveral errors of the preſs. Such of them as I could diſcover by calculation, I have corrected in the following pages.

There is a table publiſhed under the name of *J. C.* in the 199th number of the *Philoſ. Tranſ.* An. 1693: and this is evidently a ſupplement to that abovementioned of the *Philoſophical Society* meeting at *Oxford*. The experiments were, according to the initials *J. C.* made by the ſame curious perſon Mr *John Caſwell*, and are therefore of the ſame eſtimation as the others.

M. *Homberg*, of the *R. Acad. of Sc.* at *Paris*, read a memoir in 1699, wherein he took notice of the expanſion of all ſubſtances by heat, and the contraction of the ſame by cold: from whence it muſt follow, that the ſpecific gravities of the ſame bodies would conſtantly be found leſs in the ſummer and greater in the winter. And this he ſhew'd from the experiments he had made upon ſeveral fluids, both in the ſummer and the winter-ſeaſons, by means of an inſtrument he had contrived and called an *Araeometer*, being a large phial, to which he had adjuſted a long and ſlender ſtem, whereby he could to good exactneſs determine, when it was filled with equal bulks or quantities of the ſeveral fluids he propoſed to examine. The reſult of his trials with this inſtrument he digeſted into a ſhort table, which was printed in the Memoirs of the *Academy* for the ſame year 1699. This table *J. Caſpar Eiſenſchmid* afterwards republiſhed with ſeveral additions, in his tract *De Ponderibus et Menſuris*, printed at *Straſburg* in 1708, 8vo. changing it to a more convenient form for his purpoſe, by reducing the different fluids therein named to the known bulk of a cubical *Paris* inch. So much of this table as I thought might be of ſervice, I have here ſubjoined to the others in the following collection, but I have alſo made an alteration in the form, the better to fit it for general uſe, by omitting the abſolute weights of the ſeveral bodies in ſummer and winter, and placing inſtead of them, after the name of each body a decimal number, expreſſing the proportion of its weight in winter to its weight in ſummer, ſuppoſed to be every-where repreſented by unity.

Sir *Iſ Newton*, in his *Optics*, printed in 4to. at *London* 1704, gave a table of the ſpecific gravities of ſeveral *diaphanous* bodies. The experiments were made by him with a view chiefly to optical enquiries, and to enable him to compare their denſities with their ſeveral refractive powers: we may therefore be well aſſured, that they were made by the great

author with the moſt ſcrupulous care and exactneſs. The table conſiſts of 22 articles.

John Harris, D. D. in his *Lexicon Technicum*, firſt printed at *London* 1704, *fol.* republiſhed at large the ſeveral tables of ſpecific gravities of the *Oxford Society* and *J. C.* from the *Philoſ. Tranſ.* and that of the Hon. *Rob. Boyle* from his *Medicina hydroſtatica*, to which laſt he alſo added ſome experiments of his own, made as it ſeems with good accuracy. Theſe are here extracted, and placed under his name in the following tables.

Mr *John Ward* of *Cheſter*, in his *Young Mathematician's Guide*, firſt printed, as I take it, in 1706, acquaints us, that he had himſelf for his own ſatisfaction, made ſeveral experiments upon the different ſpecific gravities of various bodies; and that he was of opinion, that he had obtained the proportion of the weight that one body bears to another of the ſame bulk and magnitude, as nicely as the nature of ſuch matter, as might be contracted or brought into a leſſer body (*viz.* either by drying, hammering, or otherwiſe) would admit of. And he has accordingly given us in the ſaid book the weight of a cubic inch of 24 different ſubſtances, both in *Troy* and *Averdupois* ounces and decimal parts of an ounce; which he further aſſures us required more charge, care, and trouble, to find out nicely, than he was at firſt aware of. This table appears to have been well eſteemed, and to have had the ſanction of Mr *Cotes*'s approbation, by his taking it, when reduced to the common form, into that collection which he drew up for his own hydroſtatical lectures.

Roger Cotes, M. A. and *Plumian Prof. Aſtron. and Exp. Philoſ.* at *Cambridge*, firſt giving about the year 1707 a *Courſe of Hydroſtatical and Pneumatical Experiments*, in conjunction with Mr *Whiſton* in that Univerſity, drew up, for the uſe of that courſe, a very accurate table of ſpecific gravities, collecting from ſeveral places ſuch experiments as he took to be moſt exact, and the beſt to be depended upon. And as the judgment of ſo great a man cannot but give a general reputation to ſuch experiments as he had ſo ſelected, I have thought proper, in the following tables, to diſtinguiſh all ſuch by the addition of the letter *C*, after the names of ſuch perſons from whom they firſt appear to have been taken, adding alſo the name of *Cotes* at length, to ſuch others as I have not met with elſewhere, and which I therefore take to have been tranſcribed from the *memoranda* of his own experiments. This table of Mr *Cotes*'s uſed firſt to be given in *M. S.* to thoſe who attended his lectures; but it was afterwards printed in a ſingle ſheet, relating to a *Courſe of Experiments* at *Cambridge* in 1720, and ſince in Mr *Cotes's Hydroſtatical and Pneumatical Lectures*, when they were publiſhed at large in 8vo by his ſucceſſor Dr *Smith*, now the worthy maſter of *Trinity College*. In theſe printed lectures were inſerted the gravities of human blood, its *ſerum*, &c. from Dr *Jurin*, inſtead of thoſe that had before been made uſe of from Mr *Boyle*.

Mr *Francis Hauksbee*, now Clerk to the *Royal Society*, did, about the the year 1710, begin, in conjunction with Mr *Whiſton*, who had then newly left the Univerſiy, to give hydroſtatical lectures, *&c.* in *London*; for

for the purpoſe of which he reprinted in a thin volume in 4^to^, in which are the ſchemes of his experiments, Mr *Cotes*'s table of ſpecific gravities abovementioned. To which he added, from tryals of his own, the weights of ſteel, ſoft, hard, and temper'd, which are printed with his name in the following tables, as are alſo ſome other experiments, which he has ſince occaſionally made, and communicated to me. Mr *Cotes*'s table, with the abovemention'd additions of Mr *Haukſbee*, was afterwards again publiſhed by Dr *Shaw*, in his *Abridg.* of Mr *Boyle*'s *Philoſ. Works*, at *Lond.* 1725, 4^to^. vol. ii. p. 345.

John Freind, M. D. at the end of his *Prælect. Chem.* printed at *Lond.* in 1709, 8^vo^. has publiſhed ſome new tables of the ſpecific gravities both of ſolid and fluid bodies, entirely taken from his own original experiments. And as theſe tables contain an account of a very uſeful ſet of bodies, upon which few or no other experiments have been made: it is great pity that this truly learned and elegant writer was not more accurate in his tryals than he appears to have been. Many of his experiments having indeed been made in ſo lax and improper a manner, and ſo many errors having been committed in them, that one cannot with ſecurity depend upon theſe tables, tho' containing otherwiſe facts one would ſo much deſire to be truly informed about. I have however here inſerted the ſeveral particulars of his two laſt tables, which immediately concern ſpecific gravities, after correcting ſuch errors in calculation as I could certainly come at: and I hope that I ſhall be excuſed for this free cenſure upon part of the works of a gentleman, who has ſo well deſerved of the learned world, and acquired ſo juſt a reputation in it.

James Jurin, M. D. and ſeveral years Secretary of the *Royal Society*, gave, in N°. 361 of the *Philoſ. Tranſ.* An. 1719, ſome original and very accurate experiments made by himſelf, upon the ſpecific gravity of human blood, at ſeveral times during the ſix preceding years. Theſe were accompanied with a very curious Diſcourſe, which has ſince been tranſlated by himſelf, into *Latin*, and reprinted in his *Diſſert. Phyſico Math. Lond.* 1732, 8^vo^.

This gentleman has alſo, in N°. 369 of the ſame *Tranſ.* obliged us with ſome very judicious and uſeful remarks, relating to the *caution to be uſed in examining the ſpecific gravity of ſolids, by weighing them in water*; for want of attending to which, ſeveral ſorts of bodies, ſuch as human *calculi*, the ſubſtance of all woods, *&c.* have appeared, from their pores and ſmall cavities filled up with air, to be conſiderably lighter than they really are.

John Woodward, M. D. and Profeſſor of Phyſic in *Greſham College*, had, as he acquaints us in ſeveral places of his works, made a great number of experiments upon the ſpecific weights, of mineral and other foſſil bodies, but which being probably contained in thoſe of his papers which he ordered to be ſuppreſſed at his death, are thereby loſt to the world, to which they would without all doubt have been very acceptable. All I have been able to pick up are a very few mentioned in the

Catalogue

Catalogue of the Engliſh Foſſils in his Collection, publiſhed ſince his deceaſe, in 8vo. *Lond.* 1729.

Mr *Gabriel Fahrenheit*, F. R. S. communicated, in N°. 383. of the *Philoſ. Tranſ. a table of the ſpecific gravities of* 28 *ſeveral ſubſtances*, from hydroſtatical experiments of his own, made with great care and exactneſs; to which he ſubjoined ſome obſervations upon the manner in which his trials were performed, together with a deſcription of the inſtruments in particular which he made uſe of to examine the gravities of fluids. To ſome of his experiments which he thought required a greater nicety, he has affixed an aſteriſk in his table, ſignifying ſuch to have been adjuſted to the temperature of the air, when his Thermometers ſtood at the height of 48 degrees. This gentleman, who is well known by the reputation of his Mercurial Thermometers, which he made with great curioſity, and which are now generally uſed, was in *England* in the year 1724.

Profeſſor *Peter van Muſſchenbroek*, of *Utrecht*, publiſhed in his *Elementa Phyſicæ* at *Leyden* in 8vo. 1734. a large table of ſpecific gravities, which he afterwards yet ſomewhat further enlarged in his *Eſſai de Phyſique* in *French*, at *Leyden* 1739. 4to. This table contains almoſt all the preceding ones, but without the names of the authors from whom they were collected. I have among thoſe which follow inſerted, under this author's name, ſuch experiments as I had not before met with elſewhere: making uſe of the *Latin* edition as the more correct, except in ſuch articles which are only to be found in the *French*.

Mr *John Ellicott*, F. R. S. having an opportunity in the year 1745, to examine the weight of ſome large diamonds, he accordingly, with the utmoſt care, and with exquiſite aſſay-ſcales which very ſenſibly turned with the 200th part of a grain, took the ſpecific gravities of 14 of thoſe diamonds, 4 of which came from the *Braſils*, and the other 10 from the *Eaſt-Indies*. Theſe experiments he communicated to the Preſ. of the Royal Society, who cauſed them to be read at one of their meetings, and afterwards publiſhed them in N°. 476. of the *Philoſ. Tranſ.* Among theſe *Braſilian* diamonds, one was of the abſolute weight of 92,425, another of 88,21; and among the *Eaſt-Indian* ones, one of 29,525 *Troy* grains. And as the ſize of theſe ſtones made them much fitter for theſe enquiries, than any others which had probably ever before been uſed for the ſame purpoſe, ſo the known accuracy of the author, the goodneſs of his inſtruments, and the conſiſtency of all his experiments, ſufficiently ſhew the ſpecific gravities he has delivered in his paper, may entirely be depended upon.

The ſame curious perſon alſo communicated the ſpecific gravities of fine and ſtandard gold, publiſhed under his name in the following tables, and which were deduced from experiments he was ſo kind as to make on purpoſe at my requeſt.

As I have juſt had occaſion to mention diamonds, it may poſſibly not be foreign to the purpoſe here to take ſome notice of the diamond carat weight,

weight, uſed among Jewellers, which weight was originally the carat or 144th part of the *Venetian* ounce, equal to 3,2 *Troy* grains, but which is now, for want of an acknowledged ſtandard, ſomewhat degenerated from its firſt weight. I have myſelf found it, upon a medium of ſeveral experiments, equal to 3,17 *Troy* grains; and I have the rather taken notice of this weight here, becauſe there happens to be a miſtake about it, both in Dr *Arbuthnot*'s and Mr *Dodſon*'s tables, who have ſet down as it ſeems the number of diamond carats in a *Troy* ounce, inſtead of the weight of the diamond carat itſelf. This carat is again divided into four of its own grains, and thoſe into halves and quarters, commonly called the eighths and ſixteenths of a carat: and thus the largeſt of the diamonds juſt abovementioned, weighed, in the Jewellers phraſe, better than 29 carats and almoſt half a grain.

Mr *James Dodſon*, in his book called *The Calculator*, printed in 8vo. *Lond.* 1747, has inſerted a uſeful table of ſpecific gravities, in which he has by the firſt initial letter of their names diſtinguiſhed the ſeveral authors he has quoted: and amongſt theſe are ſeveral new experiments marked with an *L*, which I am told were communicated from his own trials, by Mr *Charles Labelye*, engineer, and which concern particularly the weights of ſeveral ſorts of ſtone and other materials uſed in building. Theſe I have alſo diſtinguiſhed by an *L.* as they ſtand in Mr *Dodſon*'s book.

Mr *Geo. Graham*, F. R. S. made for me, at the requeſt of a friend, ſome accurate trials upon the weight of gold and ſilver, both when reported fine, and when reduced to the *Engliſh* ſtandard: all which I have inſerted under his name in the following tables. Wherein I have beſides reported, ſome other ſingle experiments, which I occaſionally met with, from *Fred. Slare*, M. D. *John Keill* of *Oxford*, M. D. *Steph. Hales*, D. D. and *Edward Bayley* of *Havant* in *Hampſhire*, M. D.

Richard Davies, M. D. I have laſtly to this collection of experiments added ſome of my own, which I endeavoured to make with as much accuracy, as the inſtruments I was provided with would allow of. My hydroſtatical balance was one conſtructed ſeveral years ſince by Mr *Francis Haukſbee*, which I have conſtantly found to turn ſenſibly with half a grain: and the bodies upon which I made moſt of my trials, were taken from a collection of the *Materia Medica* formerly made by Signor *Vigani*, and ſtill preſerved in the library of *Queen's-College* in *Cambridge*.

☉ GOLD, fine. *Ward, C.*	19.640	Tab. I. *Of*
A medal eſteemed to be near fine gold. *J. C.*	19.636	Metals.
Or d'eſſai, ou de coupelle. *Muſſchenbr.*	19.238	
Fine gold hammered. *Ellicot.*	19.207	
D°. an ingot, ſo accounted, and again refined with antimony. *Ellicot.*	19.184	
D°. the ingot itſelf juſt mentioned. *Ellicot.*	19.161	
A medal of the Royal Society, reported fine gold. *Graham.*	19.158	

A gold

A gold medal of Queen *Elizabeth*. *J. C.*	19.125
D°. of Queen *Mary*. *J. C.*	19.100
Aurum. *Fahrenheit*.	19.081
Id. *Ghetaldus*. Aurum purum. *Bacon* (ex hyp.)	19.000
A gold Coin of *Alexander*'s. *J. C.*	18.893
Gold. *Reynolds*.	18.806
Aurum. *Villalpandus*. *Petitus*.	18.750
Standard gold (by which is underſtood gold of 22 carats, or ſuch of which our guineas are intended to be coined). *J. C. Ward. C.*	18.888
An old *Jacobus*. I ſuppoſe the ſceptered broad piece. *Harris*.	18.375
A *Mentz* gold ducat. *J. C.*	18.261
Aureus Ludovicus. *Muſſchenbr.*	18.166
A five guinea piece of K. *James* II. 1687, with an elephant. *Graham*.	17.933
A *Portugal* piece of 3*l.* 12*s.* 1731, ſuppoſed to be nearly the ſame as ſtandard. *Graham*.	17.854
Guineas, ten weighed together. *Davies*.	17.800
D°. on a mean of 7 trials upon thoſe of different reigns. *Ellicot*.	17.726
A piece of gold coin of the Commonwealth. *Harris*.	17.625
Guineas, two new ones. *Haukſbee*.	17.414
A grain of *Scotch* gold, ſuch as nature had made it. *Boyle* V. 30. *b*. $12\frac{2}{7}$	12.286
Electrum, a *Britiſh* coin. *J. C.*	12.071
☿ QUICKSILVER. Mercurius crudus. *Freind*.	14.117
Mercury, *Spaniſh*. *Boyle* V. 10. *b*. Mercure ſublimé 511 fois. *Muſſchenb*.	14.110
Quickſilver. *Oxford Soc*.	14.019
D°. *Ward. C.* revived from the ore. *Boyle*.	14.000
Fine mercury. *L*.	13.943
Quickſilver, another parcel. *Oxf. Soc*.	13.593
Mercure amalgamé avec de l'argent, affiné et ſublimé 100 fois. *Muſſchenb*.	13.580
Mercurius. *Fahrenheit*.	13.575*
Argentum vivum. *Ghetaldus*. $13\frac{4}{7}$	13.571
Mercure amalgamé avec de l'or affiné, et ſublimé 100 fois; le même meſlé avec du plomb, enſuite converti en poudre et revivifié. *Muſſch*.	13.550
Coarſe mercury. *L*.	13.512
Mercurius. *Petitus*.	13.406
Quickſilver. *Reynolds*.	13.147
♄ LEAD. *Reynolds*.	11.856
Plumbum. *Villalpand*.	11.650
Id. *Ghetaldus* $11\frac{1}{2}$.	11.500

Id.

Id. *Bacon.* 11.459
Lead. *Harris.* 11.420
Hardeſt lead. *L.* 11.356
Plumbum. *Fahrenheit.* 11.350
Lead. *Oxford Soc. Ward.* 11.345
Plumbum. *Petitus.* 11.343
Lead. *Harris.* (an ordinary piece) . . . 11.330
D°. *Cotes.* 11.325
Plumbum Germanicum. *Muſſchenb.* . . . 11.310
Caſt lead. *L.* 11.260
☾ SILVER, fine. *Ward. C.* 11.091
A medal of the Royal Society, reported fine ſilver. *Graham.* 10.484
Argentum. *Fahrenheit.* 10.481
Silver. *Reynolds.* 10.432
Argentum. *Villalpandus.* 10.400
Id. *Ghetaldus.* $10\frac{1}{3}$. 10.333
Id. *Bacon.* 10.331
Id. *Petitus.* 10.219
Sterling, or ſtandard ſilver (that is, ſilver 11 oz. 2d^{wt}. in the pound fine). A half crown of K. *William*'s coin. *Harris.* 10.750
D°. ſtruck into money. *L.* 10.629
D°. *J. C. Ward. C.* 10.535
D°. caſt. *L.* 10.520
A new crown-piece, 1746. LIMA under the head. *Graham.* 10.284
♀ COPPER. *Reynolds.* 9.127
Cuprum. *Villalpandus.* 9.100
Æs. *Ghetaldus.* Roſe copper. *Ward. C.* Fine copper. *L.* An old copper halfpenny, *Charles* II's coin. *Harris.* 9.000
Copper, in half-pence. *L.* 8.915
Æs; cuivre. *Petitus.* 8.875
Cuprum. *Bacon.* 8.866
Copper. *Oxf. Soc.* 8.843
Cuprum Suecicum. *Fahrenheit.* 8.834
Id. Japonenſe. *Fahrenheit.* 8.799
Id. Suecicum. *Muſſchenbr.* 8.784
Common copper. *L.* 8.478
BRASS. An old braſs gold weight, marked XXXIII. *Harris.* . 8.830
Aurichalcum. *Bacon.* 8.747
A piece of hammered braſs. *Harris.* . . . 8.660
Æs, airin, calaminæ mixtum. *Petitus.* . . . 8.437
Aurichalcum. *Fahrenheit.* 8.412
Braſs hammered. *J. C.* Plate braſs. *Ward.* . . 8.349

Wrought braſs. *J. C.*	8.280
Caſt braſs. *L.*	8.208
D°. *J. C. Ward.*	8.100
D°. *Cotes.*	8.000
Braſs hammered. *Reynolds.*	7.950
D°. caſt. *Reynolds.*	7.905
A piece of caſt braſs. *Harris.*	7.666
♂ IRON. Ferrum. *Villalpandus.*	8.086
Id. *Ghetaldus.*	8.000
Iron, forged. *Reynolds.*	7.906
Ferrum. *Petitus.*	7.875
Id. *Bacon.*	7.837
Spaniſh bar iron. *L.*	7.827
Swediſh D°. *L.*	7.818
Ferrum. *Fahrenheit.*	7.817
Iron. *Cotes.*	7.645
D°. of a key. *J. C.* Common iron. *Ward.*	7.643
A piece of hammered iron, perhaps part ſteel. *Harris.*	7.600
Iron caſt. *Reynolds.*	7.520
D°. caſt. *L.*	7.135
Softeſt caſt iron, or *Dutch* Plates. *L.*	6.960
STEEL. *J. C. Ward.*	7.852
D°. *Cotes.*	7.850
D°. ſpring temper. *Hawkſbee.*	7.809
D°. nealed ſoft. *L.*	7.792
D°. ſoft. *Haukſbee.*	7.738
D°. hard. *Haukſbee.*	7.704
D°. hardened. *L.*	7.696
♃ TIN. *Reynolds.*	7.617
Stannum. *Bacon.*	7.520
Id. *Villalpandus. Freind.*	7.500
Etain d'Angleterre. *Muſſchenbr.*	7.471
Stannum. *Ghetaldus.* $7\frac{2}{3}$	7.400
Id. Provinciæ Indiæ Or Malacca. *Fahren.*	7.364
Block tin. *Oxf. Soc. Ward. C.*	7.321
Stannum Anglicanum. *Fahrenheit.*	7.313
Id. commune. *Petitus.*	7.312
Id. purum. *Petitus.*	7.170
Block or grain tin. *L.*	7.156

Notes and obſervations.

As I thought the uſes that might be made of theſe tables, either in buſineſs or in philoſophy, would beſt be illuſtrated by a few ſhort notes, I have therefore here occaſionally inſerted ſuch obſervations as occurred to me, whilſt I was reviſing them for the preſs: and, as many of theſe related chiefly to the preſent defects of my tables, thoſe I thought would

would probably be of ſervice, to ſuch as might hereafter take the trouble of improving or correcting them.

As the particulars contained in the tables were extracted from different books, at different times, and at firſt only intended for my own private uſe, I was not ſollicitous to preſerve one uniform language, but generally ſet down every experiment in my common place, in the words of the author I took it from: and as I have ſince found, that by a tranſlation I might ſometimes happen not ſo juſtly to repreſent the body intended, I have upon the whole judged it beſt, here alſo to tranſcribe them in the ſame languages in which they were at firſt delivered.

To make experiments of this ſort, with a ſufficient degree of accuracy, requires a pretty deal of care and pains: and, as in ſuch as I have made myſelf, I have found great conveniency in the uſe of decimal weights, preferably to thoſe of the common form, I would alſo recommend the uſe of ſuch to others, who ſhall pleaſe to employ themſelves in the like enquiries. Thoſe I have provided for myſelf have a *Troy* ounce for their *integer*, and my leaſt weight is the thouſandth part of that quantity, differing conſequently from the half of a *Troy* grain only as 24 does from 25, which is inconſiderable ſo far as thoſe ſmall weights are concerned. My four ſmalleſt are reſpectively of 1, 2, 3, and 4 of thoſe thouſandth parts, and together make ten, or an unit of the next denomination, that of the 100th part of an ounce. I then have four others, making 1, 2, 3, and 4 100ths, and together the unit of the next denomination, or one tenth of an ounce, and ſo on. By theſe I ſave the trouble of reducing the common weights to their loweſt denomination in every experiment, and ſometimes perhaps avoid making miſtakes in that very trifling work.

Whenever two or more original writers nearly concur in their experiments upon any ſubject, the gravity ſo deduced may be well depended upon. But where they differ remarkably, it muſt either be imputed to the unequal gravity of the ſubject itſelf, or to ſome error in the trials, which may eaſily happen in matters that depend on the obſervation of ſo many minute particulars. All thoſe caſes that ſo ſenſibly differ would well deſerve to be re-examined.

The firſt table above, that of metals, as it is compoſed of the moſt perfect and uniform bodies in nature, ſeems capable of being adjuſted with the greateſt preciſion, both with relation to the pure metals themſelves, and to the ſeveral degrees of their mixtures one with another, if experiments in all theſe caſes were but made with a ſufficient degree of accuracy.

Gold, in the experiments I have made myſelf, I could never find to come up to the weight aſſigned it in ſome of the former tables, and particularly thoſe I have made upon our own coin, and ſome others have always remarkably fallen ſhort of the weight aſſigned to the ſtandard in thoſe ſame tables. I have inſerted that trial in which I found guineas to come out beſt; and I may venture to affirm, that that experiment, in

particular, was made with as much accuracy as my inſtrument was capable of, the pieces were all waſhed in ſoap and water, cleaned with a bruſh, and the air-bubbles well freed, and the like. That experiment is beſides abundantly confirmed ſince, by the exact trials lately made by Mr *Graham* and Mr *Ellicot*, which were performed with the greateſt care; and the fine gold alſo mentioned by the laſt was choſen and prepared with the greateſt curioſity.

It may be obſerved, that the gold medals of Q. *Eliz.* and Q. *Mary*, quoted by *J. C.* were, without doubt, the large ſovereigns of thoſe queens, which were of the old ſtandard of *England*, or of gold appointed to be 23 carats, 3 grains and a half fine: that the *Mentz* ducat, mentioned by the ſame, if it was one of thoſe *ad legem imperii*, which are always in their own mints affirmed to be fine, come out conſiderably too light: and that the gold coin of the Commonwealth, and the piſtoles of *France*, were like our preſent gold money of the goodneſs of 22 carats.

Mercury is placed in this table among the metals, by reaſon of its near agreement with thoſe bodies in its ſpecific gravity; though it otherwiſe ſo widely differs from them in moſt of its properties.

Braſs is conſiderably condenſed by hammering; whether gold, ſilver, and the other metals are alſo condenſed in like manner, hardly appears yet to have been ſufficiently tried.

Of the mixed metals, hardly any except braſs, appear to have had their ſpecific gravities very carefully aſcertained: bell-metal, princes metal, however, and ſome others, might deſerve to be examined in that particular.

It might poſſibly be queried alſo, whether ſeveral mixed metals do not either rarify or condenſe upon mixture, ſo as thereby to acquire a more different ſpecific gravity, than the natural law of their compoſition, at firſt ſeems to require.

It may laſtly be obſerved, that the ſpecific gravities of all the known metals are ſuch, as that none of them come up to 20 times the weight of common water, or fall ſenſibly below 7 times the ſame weight.

Tab. II. *Of minerals, ſemi-metals, ores, preparations, and recrements of metals, &c.*

BISMUTH. *J. C.*	9,859
D°. *Cotes.*	9,700
D°. or tynglaſs. *Boyle.*	9,550
Tynglaſs. *Reynolds.*	7,951
Marcaſita alba. *Fahrenheit.*	9,850
Mineral, Corniſh, ſhining like a marcaſite. *Boyle.*	9,060
Calx of lead. *Boyle.*	8,940
Spelter ſolder. *J. C.*	8,362
Spelter. *J. C.*	7,065
Cinnabar common. *Boyle.*	8,020
Cinnabaris factitia. *Muſſchenbr.* (if not a miſtake for the laſt experiment)	8,200

Cinnabar

Cinnabar native, breaking in poliſhed ſurfaces like Talc. *Davies.*	7,710
D°. *Perſian*, breaking rough. *Davies.*	7,600
D°. native. *Boyle.*	7,576
Cinnabaris nativa. *Muſſchenbr.*	7,300
Cinnabar native, very ſparkling. *Boyle.*	7,060
D°. native, from *Guinea*. *Davies.*	6,280
Cinnabar of antimony. *Harris.*	7,060
D°. another piece. *Harris.*	7,043
D°. *Boyle.*	7,030
Cinnabar antimonii. *Freind.*	6,666
Cinnabre d'antimoine. *Muſſchenb.*	6,044
Lead ore, rich, from *Cumberland*. *Boyle.*	7,540
D°. *Boyle.*	7,140
The reputed ſilver ore of *Wales*. *J. C.*	7,464
The metal thence extracted. *J. C.*	11.087
Regulus antimonii. Item Martis et Veneris. *Freind.*	7,500
Id. *Fahrenheit.*	6,622
Id. *Harris.*	6,600
Id. per ſe. *Davies.*	4,500
Silver ore, choice. *Boyle.*	7,000
D°. another piece from *Saxony*. *Boyle.*	4,970
Lithargyrus argenti. *Freind.*	6,666
Lithargyrium argenti. *Muſſchenbr.*	6,044
Id. Auri. *Freind.*	6,316
Id. Auri. *Muſſchenb.*	6,000
Minera antimonii. *Davies.*	5,810
Cuprum calcinatum. *Freind.*	5,454
Glaſs of antimony. *Newton. C.*	5,280
Vitrum antimonii. *Freind.*	5,000
Id. per ſe. *Boyle.*	4,760
Tin ore, choice. *Boyle.*	5,000
D°. black, rich. *Boyle.*	4,180
New *Engliſh* tin ore, Mr *Hubert*'s. *Boyle.*	4,080
Tutty, a piece. *Boyle.*	5,000
Tutia. *Muſſchenb.*	4,615
Lapis calaminaris. *Freind.* Lapis cæruleus Namurcenſis. *Muſſchenb.*	5,000
Id. *Boyle.*	4,920
Loadſtone. *Boyle* V. 6. *b.*	4,930
Magnes. *Petitus.*	4,875
A good loadſtone. *Harris.*	4,750
Marcaſites, one more ſhining than ordinary. *Boyle.*	4,780
A golden marcaſite. *J. C.*	4,589
Marcaſites, from *Stalbridge*. *Boyle.*	4,500
D°. *Boyle.*	4,450

Antimonium Hungaricum. *Muſſchenbr.*		4,700
Antimony, good, and ſuppoſed to be *Hungarian.* *Boyle.*		4,070
D°. crude, which ſeemed to be very good. *Harris.*		4,058
Antimonium crudum. *Freind.*		4,000
Id. *Davies.*		3,960
Black Sand, commonly uſed on writing. *Boyle.* V. 33. *b.*		4,600
Crocus Metallorum. *Muſſchenb.*		4,500
Id. *Freind.*		4,444
Hæmatites. *Muſſchenbr.*		4,360
Id. *Boyle.* V. 6. *a.*		4,150
D°. *Engliſh.* *Boyle.*		3,760
Copper ore, rich. *Boyle.*		4,170
D°. *Boyle.*		4,150
Copper-ſtone. *Boyle.*		4,090
Emeri. *Boyle.* V. 26. *b.*		4,000
Manganeſe. *Boyle.*		3,530
A blew ſlate with ſhining particles. *J. C.*		3,500
Iron ore, a piece burnt or roaſted. *Harris.*		3,333
Ceruſſa. *Item* Chalybs cum ſulphure. pp. *Freind.*		3,158
Lapis lazuli. *J. C.*		3,054
D°. *Boyle.* V. 6. *b.*		3,000
D°. *Boyle.*		2,980
Gold ore. *Boyle.* V. 29. *b.*		2,910
D°. not rich, brought from the *Eaſt Indies.*	*Boyle.*	2,652
Another lump of the ſame. *Boyle.*		2,634
A mineral ſtone, yielding 1 part in 160 metal.	*J. C.*	2,650
The metal thence extracted. *J. C.*		8,500
Pyrites homogenea. *Fahrenheit.*		2,584
Black Lead. *Boyle.* V. 27. *a.*		1,860
Æs viride. *Freind.*		1,714
Plumbum uſtum. *Freind.*		1,666

The ſecond table is impoſed of ſubjects no way ſtrictly allied to each other, either by their gravities, or their other eſſential properties; and perhaps they might better, on that account, have been divided into different tables.

The bodies themſelves are chiefly of an uncertain and heterogeneous nature; being ſo far as appears compoſed of different elements, and thoſe alſo combined in various proportions, ſuch as ſulphur and arſenic, joined with ſtone, metal, and the like: and from theſe ſeveral degrees of mixture it muſt follow, that moſt of theſe kinds of bodies, tho' ſo far ſimilar as to be called by the ſame names, yet muſt neceſſarily admit of a conſiderable latitude in their ſpecific gravities. Many uſefull deductions may nevertheleſs be drawn from thoſe conſiderations, relalating to the comparative goodneſs, *&c.* of ſuch bodies.

Cinnabar

Cinnabar native, appears to be a compound of mercury and ſulphur, with a portion of earthy or ſtony matter; and that which is heavieſt muſt abound moſt with the mercury. The different appearances which this body makes, would alſo give us a ſuſpicion that there are other varieties in its compoſition, beſides thoſe juſt taken notice of: ſome ſorts of cinnabar, ſuch as the *Hungarian*, breaking into poliſhed planes and ſquares like talc, whilſt others like the *Perſian* of this table, break rough and with ſhining *granulæ* or *micæ*; and that without any conſiderable difference in their gravities.

By the factitious cinnabar it may be determined, what proportion of mercury will ſo incorporate with ſulphur, as to make up an uniform body.

Antimony may in like manner be conſidered as a compoſition of its regulus and ſulphur.

The black ſand uſed on writing is ſaid by Mr *Boyle* to be a rich iron ore: he alſo ſays that emery, loadſtone, and all ſuch ponderous ſtones, contain ſome kind of metal, which he had himſelf ſeparated from them. IV. 120. *a.*

The great variety of ores of all kinds well deſerve to be accurately examined, for the ſake of the many concluſions that may be drawn from thence, concerning the natures of concrete bodies, and for many other purpoſes in metallurgy. But I have as yet met with a very ſmall number of experiments upon theſe ſubſtances. Dr *Woodward* has indeed mentioned a great many obſervations of this ſort which he had made, and kept exact regiſters of: but as they were probably among thoſe paper which he ordered to be deſtroy'd at his death we muſt look upon them as now loſt to the world.

The marcaſites and pyrites are very uncertain and ſtrange kinds of bodies, their gravities are often very great: a marcaſite here taken from *Fahrenheit* was found nearly to equal the heavieſt mineral biſmuth itſelf; and yet it is very ſeldom that any metal or ſemimetal can be obtained in any quantity from theſe ſubſtances, all that is in them being uſually deſtroyed, and carried away by their ſulphur.

Black lead is alſo a very odd kind of mineral, having all the appearance of a ſemimetal, and yet falling ſhort even of the weight of common earth.

The ſemimetals generally exceed in their ſpecific gravities even the baſer metals themſelves.

It may be obſerved, that it appears by this table, that the ſpecific gravities of ores, including the metallic ſtones, are uſually found to lie between 7 and 3 times the weight of water. Lead and ſilver ores are the heavieſt, thoſe of copper, tin, and iron being conſiderably lighter. The gold ore we have an account of muſt be ſo poor as hardly to be worth taking any notice of: but we have in general too few of theſe experiments, to draw any certain concluſions from them.

GRANATE, Bohemian. *Boyle.* 4,360 Tab. III.
Granate. *J. C.* 3,978 *Of Gems,*
Granati *Chryſtals, Glaſs, and*

transparent Stones.

Granati minera. *Boyle.*	3,100
A Pſeudo-Topazius, being a natural pellucid, brittle, hairy ſtone, of a yellow colour. *Newton. C.*	4,270
Sapphires. *Davies.*	4,090
A Sapphire very perfect, but rather pale. *Hauksbee.*	4,068
Glaſs, blue in ſticks from Mr Seale. *Hauksbee.*	3,885
D°. whiteſt, from Mr Seale. *Hauksbee*	3,380
D°. clear cryſtal. *Cotes.*	3,150
D°. blue plate, old. *Hauksbee.*	3,102
D°. plate. *L.*	2,942
D°. old looking-glaſs plate of a light colour. *Hauksbee.*	2,888
D°. green. *Freind.*	2,857
D°. green bottle. *Hauksbee.*	2,746
D°. of a bottle. *Oxf. Soc.* It. a blue paſte *Hauksbee.*	2,666
D°. common green. *Hauksbee.*	2,620
D°. deep green old. *Hauksbee.*	2,587
D°. vulgar. *Newton. Ward.*	2,580
Vitrum Venetum. *Freind.*	1,791
An oriental cat's-eye, very perfect. *Hauksbee.*	3,703
A diamond, yellow, of a fine water, ſomewhat paler than the joinquille. *Hauksbee.*	3,666
D°. white of the ſecond water. eau celeſte. *Hauksbee.*	3,540
D°. Eaſt Indian, the heavieſt of many. *Ellicot.*	3,525
D°. the lighteſt of many. *Ellicot.*	3,512
D°. Braſilian, the heavieſt of many. *Ellicot.*	3,521
D°. the lighteſt of many. *Ellicot.*	3,501
D°. the mean of all his experiments. *Ellicot.*	3,517
D°. *Newton. C.*	3,400
Diamond bort, of a bluiſh black, with ſome little adhering foulneſs. *Hauksbee.*	3,495
A Jacinth of a fine colour, but ſomewhat foul. *Hauksbee.*	3,637
A Chryſolite. *Hauksbee.*	3,360
Chryſtal cubic, ſuppoſed to contain lead. *Woodward.*	3,100
Chryſtal from Caſtleton in Derbyſhire having the double refraction. *Hauksbee.*	2,724
Chryſtal of Iſland. *Newton. C.*	2,720
Chryſtallum diſdiaclaſticum. *J. C.*	2,704
Chryſtallus de rupe. *Fahrenheit.*	2,669
Chryſtal rock. *J. C. Boyle* III. 229. *b.*	2,659
D°. a large ſhoot. *Hauksbee.*	2,658
D°. of the rock. *Newton. C.* It. chryſtal in the lead-mines near Workſworth. *Woodward.*	2,650
D°. *Hauksbee.*	2,646
D°. pure pyramidal, ſuppoſed to contain tin. *Woodward.*	2 5 or 2.400
Chryſtallus. *Petitus.*	2,287

Chryſtal

Chryſtal. *Boyle.*	2,210
Talc. Jamaican. *Boyle.*	3,000
D°. Venetian. *Boyle.*	2,730
D°. *J. C.*	2,657
D°. Engliſh. *Woodward.*	2,600
D°. a piece like lapis amianthus. *Boyle.*	2,280
A red paſte. *J. C.*	2,842
A Braſil-pebble, foul and feather'd. *Hauksbee.*	2,755
D°. a fragment uncut. *Hauksbee.*	2,676
D°. cut. *Hauksbee.*	2,591
Jaſper, ſpurious. *J. C.*	2,666
A Corniſh diamond cut. *Hauksbee.*	2,658
A water topaz, very perfect, but ſaid not to be oriental. *Hauksbee.*	2,653
Pebble pellucid. *J. C.*	2,641
Briſtol ſtone. *Davies.*	2,640
Hyacinth, ſpurious. *J. C.*	2,631
Selenites. *J. C.*	2,322
D°. *Newton.*	2,252

As the mean gravity of chryſtal appears, by the foregoing table, to be little more to that of water than as two and a half to one; it may well be ſuſpected, that the granate, pſeudo-topazius, ſapphire, and ſuch other gemms which greatly exceed chryſtal in weight, do contain a conſiderable portion of ſome ſort of metal in their compoſition: as was obſerved of theſe bodies by Dr *Woodward*, in his Method of Foſſils, p. 24.

As to the white ſapphire, which is reputed by Dr *Woodward* to be a ſpecies of gemm intermediate between chryſtals and the diamond in hardneſs, I have not yet obtained any good account of its ſpecific gravity.

The weight of the diamond is aſcertained in N°. 476 of the *Philoſ. Tranſ.* where it appears, that by experiments made with the greateſt care by Mr *John Ellicot*, F. R. S. with moſt exact inſtruments, and upon 14 different diamonds, ſome of them very large, brought from different places, and having the greateſt varieties of colour and ſhape poſſible; they were all found to agree in weight to a ſurpriſing degree of accuracy, being all ſomewhat above $3\frac{1}{2}$ times the weight of common water.

This indeed differs very ſenſibly from what had been found in ſome former experiments, but it is hardly probable that thoſe had been made upon diamonds of ſo large a ſize as theſe: Mr *Boyle* who found their weight leſs than 3 times that of common water, has himſelf told us in the ſame place, V. 83. *b.* that the ſtone he made uſe of, only weighed about 8 grains. And tho' no doubt can be made of the exactneſs of Sir *I. Newton*'s experiment, by which alſo the ſpecific weight of the diamond came out leſs than Mr *Ellicot*'s, yet it may well be queſtion'd whether Sir *Iſaac* had, at the time when he made his trials, either ſo many or ſo perfect and weighty ſtones, as a favourable opportunity offered to this

laſt gentleman. I ſhall therefore only obſerve, that, admitting this laſt to be the true ſpecific weight of the diamond, the refractive power of the ſame, in proportion to its denſity, ſhould in Sir *I. Newton*'s table be leſſened from 14556 to 14071; which would ſtill be greater than what is found in any other body; but is upon the whole more conformable to the general law of that table.

Sir *I. Newton* conjectured a diamond to be an unctuous ſubſtance coagulated, and found it to have its refractive power nearly in the ſame proportion to its denſity as thoſe of camphire, oil-olive, linſeed oil, ſpirit of turpentine, and amber, which are fat ſulphureous unctuous bodies: all which have their refractive powers 2 or 3 times greater in reſpect to their denſities, than the refractive powers of other ſubſtances in reſpect of theirs. Yet muſt it be allowed, that a diamond ſuffers no change by heat in any degree, contrary to the known property of ſulphurs; and as it is moſt reaſonable in our philoſophy to treat ſuch bodies as ſimple, in which we are not able to produce any change or ſeparation of parts, we muſt therefore on that account conſider a diamond as a ſimple body and of the chryſtalline kind.

Glaſs, which is a factitious concrete of ſand and alkaline ſalt, is nearly found to aſſume the mean gravity of ſtones and chryſtals.

If there is no miſtake in the gravity of what Dr *Freind* calls *vitrum Venetum*, it differs very remarkably from all other kinds of glaſs.

I do not know whether the jaſper and hyacinth ſpurious of *J. C.* are to be underſtood as natural or artificial gemms.

Tab. IV. *Of Stones and Earths.*

Sardachates. *J. C.*	3,598
Lapis ſciſſilis cæruleus. *Muſſchenbr.* (qu. if not the ſame experiment mentioned before pag. 222. *a blew ſlate with ſhining particles. J. C.*)	3,500
Cornelian. *Boyle.*	3,290
D°. *J. C.*	2,563
A hone. *J. C.*	3,288
D°. to ſet razors on. *Harris.*	2,960
Marmor. *Petitus.* (probably ſome miſtake in the experiment.)	3,937
Marble. *Reynolds.*	3,026
D°. white. *Hauksbee.*	2,765
D°. white Italian, of a cloſe texture viſibly	2,718
D°. white. *Boyle.* fine. *Ward. C.*	2,710
D°. white Italian, tried twice. *Oxford Soc.*	2,707
D°. black Italian. *Oxford Soc.* veined. *L.*	2,704
D°. black. *Hauksbee.*	2,683
D°. Parian. *L.*	2,560
Lapis amianthus. from Wales. *J. C.*	2,913
Turquoiſe, one of the old rock, very perfect. *Hauksbee.*	2,908
Turcoiſe ſtone. *J. C.*	2,508
Lapis nephriticus. *J. C.*	2,894

Corallium

Corallium rubrum. *Freind.*	3,857
Corall. *J. C*	2,689
D°. red. *Boyle* V. 7. *a.*	2,680
D°. *Boyle.*	2,630
D°. white, a fine piece. *Boyle.*	2,570
D°. white, another piece. *Boyle.*	2,540
Emeril ſtone, a ſolid piece. *Hauksbee*	2,766
Paving ſtone. *Reynolds.*	2,708
D°, a hard ſort from about Blaiden. *Oxf. Soc.*	2,460
A Whetſtone, not fine, ſuch as Cutlers uſe. *Harris.*	2,740
Pellets, vulgarly called alleys, which boys play withal. *Hauksb.*	2,711
Engliſh pebble. *L.*	2,696
Lapis Judaicus, *Boyle.*	2,690
Id. *Freind.*	2,500
Maidſtone rubble. *L.*	2,666
Marbles, vulgarly ſo called, which boys play withal. *Hauksbee.*	2,658
Morr ſtone. *L.*	2,656
Agate. *Boyle.*	2,640
D°. German, for the lock of a gun, *Hauksbee.*	2,628
D°. Engliſh. *J. C.*	2,512
Lapis, *Petitus.*	2,625
Flint, black, from the Thames. *Hauksbee.*	2,623
Flint ſtone. *L.*	2,621
A round pebble-ſtone within a flint. *Harris.*	2,610
Eaſt Indian blackiſh. Item, an Engliſh one. *Boyle* III. 243. *a.*	2,600
D°. *Oxford Soc.*	2,542
Corallachates. *J. C.*	2,605
Purbeck ſtone. *L.*	2,601
Free-ſtone. *Reynolds.*	2,584
Portland ſtone. *L.*	2,570
D°. white for carving. *L.*	2,312
Grammatias lapis. *J. C.*	2,515
Onyx ſtone. *J. C.*	2,510
Slate Iriſh. *Boyle.* Lapis hibernicus. *Davies.*	2,490
Wood petrified in lough Neagh. *J. C.*	2,341
Oſteocolla. *Boyle.*	2,240
Heddington ſtone. *L.*	2,204
Allom ſtone. *Boyle.*	2,180
Bolus Armena. *Freind.*	2,137
Hatton ſtone. *L.*	2,056
Burford ſtone, an old dry piece. *Oxford Soc.*	2,049
Heddington ſtone, that of the ſoft lax kind. *Oxford Soc.*	2,029
Terra Lemnia. *Freind.*	2,000
Brick. *Cotes.*	2,000
D°. *Oxford Soc.*	1,979
A Gallypot. *J. C.*	1,928

Alabaſter. *Ward. C.*	1,874
D°. *Oxford Soc.*	1,872
A ſpotted factitious marble. *J. C.*	1,822
Stone bottle. *Oxford Soc.*	1,777
A piece of a glaſs (perhaps glazed) coffee-diſh of a brown colour. *Harris.*	1,766
Barrel clay. *L.*	1,712
Lapis de Goa. *Davies.*	1,710
Lapis ruffus Bremenſis. *Muſſchenb.*	1,666
An icicle broken from a grotto (I ſuppoſe ſtalactites.) Dr *Slare*, in *Harris.*	1,190
Chalk, as found by Dr *Slare. Harris.*	1,079

The mean gravity of ſtone appears to be to that of water as about $2\frac{1}{2}$ to one, and many ſtones of great hardneſs, ſuch as the onyx, turquoiſe, agate, marble, flint, *&c.* do not much exceed that weight. It may therefore well be doubted whether ſuch ſtones, whoſe ſpecific gravity comes up to near three times that of water, or even beyond it, owe their denſity to metalline additions; or whether they are really formed of a different ſpecies of matter, as the diamond ſeems to be.

Coral by it's denſity appears to be a ſtone, though in a vegetating ſtate: or it may poſſibly from ſome late obſervations, be of an animal nature.

What is called *Lapis Hibernicus*, is a ſoft ſtone containing vitriol.

We have not many obſervations upon earths: by thoſe we have, it ſeems probable that they contain the ſame kind of matter in a lax form, of which ſtones are a more ſolid and denſer concretion.

Lapis de Goa is but a trifling compoſition perhaps hardly worth retaining in the tables.

What ſpecies of body ſhould *Alabaſter* be accounted? which with a ſtone-like hardneſs, yet falls ſo much below other ſtones, or even earths in gravity.

Tab. V. *Of Sulphurs and Bitumens.*

SULPHUR. *Petitus.*	2,344
D°. a piece of roll. *Hauksbee.*	2,010
D°. vive. *Boyle.*	2,000
D°. German, very fine. *Boyle.*	1,980
D°. tranſparent, Perſian. *Davies.*	1,950
Sulphur mineralis. *Freind.*	1,875
Brimſtone, ſuch as is commonly ſold. *J. C.*	1,811
D°. *Cotes.*	1,800
Aſphaltum. *Boyle.* III. 243. *a.*	1,400
Scotch Coal. *Boyle.* III. 243. *a.*	1,300
Coal, of Newcaſtle. *L.*	1,270
D°. pit, of Staffordſhire. *Oxford Soc.*	1,240
Jet. *J. C.*	1,238

D°.

D°. *Davies.*	1,160
D°. *Davies.*	1,020
Succinum citrinum. *Davies.*	1,110
Id. pingue. *J. C.*	1,087
Id. flavum (by 2 experiments). *Davies.*	1,080
Id. pellucidum *J. C.*	1,065
Id. album, item pingue. *Davies.*	1,060
Amber. *Boyle. Newton. C.*	1,040
Fine Gunpowder. *Reynolds.*	0,698

Sulphur is in gravity very nearly the ſame as earth, ſo that it's purity can hardly be aſcertained by it's weight, unleſs the matter it is aſſociated with, is of a ſtony denſity.

The ſemidiaphanous *Sulphur* is a beautiful kind which I have but ſeldom ſeen : it is in lumps of the ſize of a ſmall bean.

Coal, the ſorts here taken notice of are conſiderably lighter than *Sulphur* : but there are many other kinds, and of different weights.

I take the *Gagates* or *Jet* to differ very little from the *Channel Coal.*

The different ſorts of *Amber* may be obſerved not to differ conſiderably in their ſeveral gravities.

Sulphurs ſeem to be the ligheſt of all mineral bodies.

GUM Arabic. *Freind.*	1,430	Tab. VI.
D°. *Newton. C.*	1,375	*Of Gums,*
Opium. *Freind.*	1,360	*Reſins, &c.*
Gum Tragacanth. *Freind.*	1,330	
Myrrh. *Freind.*	1,250	
Gum Guaiac. *Freind.*	1,224	
Reſina Scammonii. *Freind.*	1,200	
Aloes. *J. C.* (qu. whether the reſin or the wood.)	1,177	
Aſa fœtida, a very fine ſample. *Hauksbee.*	1,251	
D°. from Dr *John Keill's Introd. ad veram Phyſicam.*	1,143	
Pitch. *Oxford Soc. C.*	1,150	
Thus. *Freind.*	1,071	
Camphire. *Newton. C.*	0,996	
Bees-wax. *Cotes.*	0,955	
Cera. *Ghetaldus.* (ad aquam ut $95\frac{5}{11}$ ad 100.)	0,954	
Wax well freed from the honey. *Davies.*	0,938	
Cera. *Petitus.*	0,937	
D°. the ſame lump 2 years after. *Davies.*	0,942	
Balſamus de Tolu. *Muſſchenbr.*	0,896	
Maſtic. *J. C.* (qu. whether the gum or the wood.)	0,849	

The bees wax in my own experiments was well freed from honey, by the boiling it in water, which probably made it lighter than it was ſet down in Mr *Cotes*'s table : and the ſecond experiment which I made

two

two years after the firſt, if the difference was not owing to the difference of heat, is an inſtance of what I take to be a pretty general truth, that bodies become more denſe and compact by reſt, and that they would alſo be found heavier in the ſcale, in thoſe caſes where they do not loſe weight by the evaporation of humidity.

The weights of vegetable gums nearly correſpond with thoſe of the ligneous parts.

Tab. VII. *Of Woods, Barks,* &c.

COCO ſhell. *Boyle.*	1,345
Bois de Gayac. *Muſſchenbr.*	1,337
Lignum Guaiacum. *Freind.*	1,333
Lignum vitæ. *Oxf. Soc.*	1,327
Speckled wood of Virginia. *Oxf. Soc.*	1,313
Cortex Guaiaci. *Freind.*	1,250
Lignum nephriticum. *Freind.*	1,200
Lignum aſphaltum. *J. C.*	1,179
Ebony. *J. C.* *Item* Aloes. *J. C.*	1,177
Santalum rubrum. *J. C.*	1,128
Id. album. *J. C.*	1,041
Id. citrinum. *J. C.*	0,809
Lignum Rhodium. *J. C.*	1,125
Radix Chinæ. *Freind.*	1,071
Dry Mahogany. *L.*	1,063
Gallæ. *Freind.*	1,034
Red-wood. *Oxf. Soc.* It. Box-wood. *Oxf. Soc. Ward. C.*	1,031
Log-wood. *Oxf. Soc.*	0,913
Oak, dry, but of a very ſound cloſe texture. *Oxf. Soc.*	0,932
D°. tried another time. *Oxf. Soc.*	0,929
D°. found dry. *Ward.*	0,927
D°. dry. *Cotes.*	0,925
D°. dry, *Engliſh.* *L.*	0,905
Oak of the outſide ſappy part, fell'd a year ſince. *Oxf. Soc.*	0,870
D°. *Reynolds.*	0,801
D°. very dry, almoſt worm-eaten. *Oxf. Soc.*	0,753
Dry Wainſcot. *L.*	0,747
Beech meanly dry. *Oxf. Soc.*	0,854
Maſtic (qu. if the wood or gum). *J. C.*	0,849
Aſh dry about the heart. *Oxf. Soc.*	0,845
D°. dry. *Cotes.*	0,800
D°. meanly dry, and of the outſide lax part of the tree. *Oxf. Soc.*	0,734
Elm dry. *L.*	0,800
D°. *Reynolds.*	0,768
D°. *Oxf. Soc. C.*	0,600
Rad. Gentianæ. *Freind.*	0,300
Cortex Peruvianus. *Freind.*	0,734

Crabtree

Crabtree meanly dry. *Oxf. Soc.*	0,765
Yew, of a knot or root 16 years old. *Oxf. Soc.*	0,760
Maple dry. *Oxf. Soc. C.*	0,755
Plumtree dry. *J. C.*	0,663
Fir, dry yellow. *L.*	0,657
Dry white Deal. *L.*	0,569
Lignum Abietin. *Freind.*	0,555
Fir dry. *Cotes.*	0,550
D°. *Oxf. Soc.*	0,546
Walnut-tree dry. *Oxf. Soc.*	0,631
Cedar dry. *Oxf. Soc.*	0,613
Juniper-wood dry. *J. C.*	0,556
Saſſafras wood. *J. C.*	0,482
Cork. *Cotes.*	0,240
D°. *J. C.*	0,237

Dr *Jurin* has obſerved in the *Phil. Tranſ.* N°. 369. that the ſubſtance of all wood is ſpecifically heavier than water, ſo as to ſink in it, after the air is extracted from the pores and air-veſſels of the wood, by placing it in warm water under the receiver of an air-pump; or if an air-pump cannot be had, by letting the wood continue ſome time in boiling water over a fire. The ſeveral weights therefore above given muſt be looked upon as the weights of the concrete bodies, in the condition they were, before the air was either forcibly got out, or the water driven into the ſmall hollows: and both theſe conſiderations may have their uſe as notwithſtanding that the ſpecific weights of the ſolid particles are truly heavier than water, we ſhall from the weights of the bodies as they are now compounded, be enabled to make ſome judgment of their poroſity, ſo far as they may be penetrable by water or other fluids.

MANATI lapis. *Boyle.*	2,860	Tab. VIII.
D°. another. *Boyle.*	2,330	*Of Animal*
D°. a fragment of. *Boyle.*	2,290	*parts.*
D°. *J. C* another from Jamaica. *Boyle.*	2,270	
Pearl, very fine ſeed oriental. *Boyle* V. 12. *a.*	2,750	
D°. a large one, weighing 206 grains. *Boyle* V. 7. *b.*	2,510	
Murex ſhell. *J. C.*	2,590	
Crabs-eyes artificial. *Boyle.*	2,480	
D°. native. *Boyle.*	1,890	
Os ovinum recens. *Freind.*	2,222	
Oyſter ſhell *J. C.*	2,092	
Calculus humanus, juſt voided. *Davies.*	2,000	
D°. *Boyle* V. 7. *b.*	1,760	
D°. *Boyle.*	1,720	
D°. *Cotes.*	1,700	
D°. *Boyle* V. 7. *b.*	1,690	

D°.

D°. *J. C.*	1,664
D°. *Davies.*	1,650
D°. *Boyle.*	1,470
D°. *J. C.*	1,433
D°. *Davies.*	1,330
D°. *J. C.*	1,240
Rhinoceros horn. *Boyle.*	1,990
The top part of one. *J. C.*	1,242
Ebur. *Freind*	1,935
Ivory. *Boyle.*	1,917
D°. dry. *Oxford Soc. C.*	1,826
D°. *Ward.*	1,823
Unicorn's horn, a piece. *Boyle.*	1,910
Cornu Cervi. *Freind.*	1,875
Ox's horn, the top part of one. *J. C.*	1,840
Blade bone of an Ox. *J. C.*	1,656
A ſtone of the bezoar kind found with four others in the inteſtines of a mare. *Edw. Bailey, M. D.* of *Havant* in *Hampſhire.* See Philoſoph. Tranſact. N°. 481.	1,700
Bezoar ſtone. *Boyle.*	1,640
D°. a large one. *Davies.*	1,570
D°. being the kernel of another. *Boyle* V. 8. *a.*	1,550
D°. a fine oriental one. *Boyle.*	1,530
D°. two weigh'd ſeparately. *Davies.*	1,504
D°. *Cotes.*	1,500
D°. *Boyle.*	1,480
D°. *Boyle.*	1,340
A ſtone from the gall-bladder. *Hales.*	1,220
Blood human, the globules of it. *Jurin by calculation.*	1,126
D°. the Craſſamentum of. *Jurin from experiments.*	1,086
D°. *Davies.*	1,084
D°. from another experiment. *Jurin.*	1,082
Sanguinis humani cuticula alba *Davies.*	1,056
Human blood when grown cold *Jurin.*	1,055
The ſame as running immediately from the vein. *Jurin.*	1,053
The ſerum of human blood. *Jurin.*	1,030
D°. *Davies.*	1,026
Ichthyocolla. *Freind.*	1,111
A Hen's egg. *Davies.*	1,090
Milk. *J. C. C.*	1,031
Lac caprinum. *Muſſchenbr.*	1,009
Lac. *Freind.*	0,960
Urine. *J. C C.*	1,030
Id. *Freind.*	1,012

Manati

Manati lapis is ſaid to be a ſtone, found in the head of the manatee, or ſea-cow of the *Weſt-Indies*. See *Ray's Syn. Meth. Anim. Quad. &c. Lond.* 1693. 8vo. Theſe ſtones and pearls are the heavieſt of all the animal productions we are acquainted with.

Dr *Jurin* has obſerved, *Phil. Tranſ.* N°. 369. that, in examining freſh human *calculi* whilſt they were ſtill impregnated with urine, he had met ſuch as exceeded the weight of ſome ſorts of burnt earthen ware and alabaſter, and approached very near to that of brick, and the ſofter ſort of paving ſtone; which I have myſelf alſo found to be true. Whereas thoſe who have made their experiments upon ſuch *calculi*, as had moſt probably been a conſiderable time taken out of the bladder, and had conſequently loſt much of their weight, by the evaporation of the urine, with which they had at firſt been ſaturated, have found thoſe ſtones commonly to have been but about one half part, and ſome of them no more than a fourth part, heavier than an equal bulk of water. From whence it has been too haſtily concluded, that theſe ſtones have very improperly been called by that name, as not at all approaching to the ſpecific gravity of even the lighteſt real ſtones that we have any account of.

The *Calculus Humanus* and *Animal Bezoar* approach nearly to each other in their ſpecific gravity.

Mr *Boyle* has taken notice of the great difference to be found between the gravity of the true and the factitious crabs-eyes. It is ſtrange that the factitious ſhould be made of ſuch materials as can bring them ſo near to the mean gravity of true ſtones: and this conſideration may deſerve the attention of thoſe who may think that any particular dependance is to be had upon the uſe of theſe bodies in medicine.

Dr *Jurin* was the firſt who carefully examined the ſpecific gravities of the different parts which compoſe human blood; and his experiments were performed with the greateſt accuracy. It may be obſerved, that the blood is, by an eaſy *analyſis* divided into *ſerum* and *craſſamentum*; and the *craſſamentum* again into the glutinous and the red globular parts, whoſe ſpecific gravities are the greateſt. It had before theſe experiments been the general received opinion, that the globules of the blood were lighter than the ſerum; and this indeed ſeemed tofollow from Mr *Boyle*'s experiments in his natural hiſtory of *human blood*; from which he deduced the ſpecific gravity of the maſs itſelf, to be to that of water as 1040 to 1000, and that of the ſerum alone to be to the ſame as 1190. And theſe numbers 1040 and 1190 had accordingly, till Dr *Jurin* re-examined the the affair, been conſtantly taken to repreſent the true gravities of human blood and it's ſerum reſpectively. See Dr *Jurin*'s *Diſſertation* in *Phil. Tranſ.* N°. 361.

Milk is made by Dr *Freind* to fall more ſhort of the gravity of water, than it is made to exceed the ſame by *J. C.* Poſſibly this difference might ariſe from the milk's being taken in one caſe warm from the cow, and in the other after it had ſtood ſome time.

Tab. IX. *Of Salts.*

MERCURIUS dulcis bis ſublim. *Muſſch.*	12,353
Mercurius dulcis. *Freind.*	11,715
Id. ter ſublim. *Muſſchenbr.*	9,882
Id. tertio ſublim. Item Panacea rubra. *Freind.*	9,372
Id. quater ſublim. *Muſſchenbr.* Item turpethum minerale.	8,235
Id. 4*to* ſublim. Item turpeth mineral. *Freind.*	7,810
Sublimat corroſiv. *Muſſchenbr.*	8,000
Id. *Freind*	6,045
Cinis clavellatus, ſordibus ſaleque ſuo neutro quodam (quod fere ſemper magis vel minus in cinere illo reperitur) depurgatus. *Fahrenheit.*	3,112
Sal illud neutrum. *Fahrenheit.*	2,642
Saccharum Saturni. Item ſal nitri fix. *Muſſchenbr.*	2,745
Eadem. *Freind.*	2,600
Magiſterium Coralli. Item pulvis ſympatheticus. *Freind.*	2,231
Tartarum vitriolatum. *Muſſchenbr.*	2,298
Id. *Freind.*	2,186
Sal mirabile Glauberi. *Muſſchenbr.*	2,246
Id. *Freind.*	2,132
Tartarum emeticum. *Muſſchenbr.*	2,246
Id. *Freind.*	2,077
Sal Gemmæ. *Newton. C.*	2,143
Nitrum. *Fahrenheit.*	2,150
Nitre. *Newton. C.*	1,900
Id. *Freind.*	1,671
Sal Guaiaci. Item Sal enixum. Item Sal prunellæ. Item S. Polychreſt. *Muſſchenbr.*	2,148
Eadem omnia. *Freind.*	2,030
Sal maritimum. *Fahrenheit.*	2,125
Cremor Tartari. Item Vitriol. alb. Item Vitriol. rubefact. Item S. Vitriol. *Muſſchenbr.*	1,900
Cremor Tar. Item Vitriol. alb. *Freind.*	1,796
Vitriol Engliſh, a very fine piece. *Boyle.*	1,880
D°. Dantzick. *Newton. C.*	1,715
Alumen. *Fahrenheit.*	1,738
Alum. *Newton.*	1,714
Sal chalybis. *Freind.*	1,733
Borax. *J. C.*	1,720
D°. *Newton. C.*	1,714
Vitriolum viride. Item Calcanth. rubefact. Item S. Vitriol. alb. *Freind.*	1,671
Saccharum albiſſ. *Fahrenheit.*	1,606 $\frac{1}{2}$
Mel. *Villalpandus*	1,500
Id. *Ghetaldus* 1 $\frac{9}{20}$. Honey. *Cotes.*	1,450
Sal volatile Cornu Cervi. *Muſſchenbr.*	1,496
Id. *Freind.*	1,421

Sal

Sal Ammoniac. purum. Item Ens Martis ſemel ſublimat.

Muſſchenb.	1,453
Eadem. *Freind.*	1,374
Ens Martis ter ſublimat. *Muſſchenb.*	1,269
Id. *Freind.*	1,233

Moſt of the experiments in the ninth table are taken from Dr *Freind*, who weighed the ſalts in ſpirits of wine, and regiſtered the proportional gravity of the ſalts to the ſpirits. But the misfortune is, that the gravity of the ſpirits of wine he made uſe of is not regiſtered: ſo that the experiments cannot with certainty be reduced to the common ſtandard of water. He has delivered the gravity of ſpirits of wine to be 0,818, and that of ſpirits of wine rectified to be 0,78. I have ſuppoſed the ſalts to be weighed in the laſt, as being the fitteſt for the purpoſe: but which he really uſed can only be conjectured.

There appears indeed to be a way to diſcover the weight of the ſpirits of wine, in which Dr *Freind* weighed his ſalts: for he weighed 60 grains of mercury, both in water and in ſpirits of wine, and the loſs of it's weight was reſpectively $4\frac{1}{4}$ grains and $2\frac{2}{3}$. Now the gravities of theſe fluids muſt be in the ſame proportion, and this would give for the weight of the ſpirits of wine 0,627, which is much too little for the weight of his own rectified ſpirits, tho' even that is leſs than what is aſſigned by any other author. So that, upon the whole, nothing can really be concluded from this experiment; and it muſt be allowed beſides, that 60 grains of mercury take up too ſmall a bulk in theſe fluids, to have their gravities determined with any exactneſs thereby.

As Prof. *Muſſchenbroke* has given in his table the ſpecific weights of many of the ſame ſalts which are mentioned by Dr *Freind*, but which differ conſiderably from the weights above ſet down, as reſulting from the Doctor's experiments, I have alſo tranſcribed the Profeſſor's numbers from his own table. Theſe do not however appear to me to be derived from new or differing experiments, but from the very ſame related by Dr *Freind*, only computed from the ſuppoſition of a heavier ſort of ſpirits of wine, whoſe ſpecific gravity is ſuppoſed to have been 0,823. The gravity of the *ſublimate corroſive*, ſet down 8,000, I take to be a miſtake, made by the writing down it's comparative weight to that of the ſpirits themſelves, inſtead of the water to which it ſhould have been referred.

It requires great care and attention to take the ſpecific gravities of ſalts with ſufficient accuracy. They diſſolve in water, and in ſome degree in all fluids that partake of the nature of water. If therefore ſpirits of wine are made uſe of for this purpoſe, they ought to be highly rectified, their own gravity accurately aſcertained, and their degree of heat ſhould be preſerved uniform. For as this fluid rarefies much faſter than water does, a ſmall difference of heat would ſenſibly effect the gravities of the ſalts to be determined by it. And perhaps ſpirit of

turpentine were a more proper fluid to be employed on theſe occaſions.

It is remarkable, that *Tartar vitriolat. Sal gem. Sal mirabile, Sal maritimum, Nitre*, &c. being ſalts compoſed of different acids and an alkaline ſalt, ſhould ſo far exceed in gravity the vitriolic ſalts, compoſed of the moſt heavy acid and a metallic earth. Is not this owing to it's forming leſs ſolid chryſtals, and to it's containing large quantities of air concealed in it's pores?

The great difference in the weight of the *Nitre*, in the ſeveral experiments of *Fahrenheit, Newton*, and *Freind*, may poſſibly be owing to the quantity of it's concealed air.

Tab. X. *Of Fluids.*

MERCURY. *Ward. C.* (ſee Tab. I. among the metals)	14,000
Oleum Vitrioli. *Fahrenheit.*	1,8775*
Oil of Vitriol. *Newton. C.*	1,700
Spiritus Nitri Hermeticus. *Freind.*	1,760
Id. *Muſſchenb.*	1,610
Lixivium cineris clavellati, ſale quantum fieri potuit impregnatum. *Fahrenheit.*	1,5713*
Id. alio tempore præparatum. *Fahrenheit.*	1,5634*
Oil of tartar. *Cotes.* Ol. tartari per deliquium. *Muſſchenb.*	1,550
Spiritus Nitri, cum Ol. Vitrioli. *Freind.*	1,440
Id. *Muſſchenb.*	1,338
Spiritus Nitri communis. Item Bezoardicus. *Freind.*	1,410
Spirit of Nitre. *Cotes.* Item Sp. Nit. Bezoardicus. *Muſſchenb.*	1,315
Sp. Nitri. *Fahrenheit.*	1,2935*
Sp. Nitri dulcis. *Muſſchenb.*	1,000
Aqua fortis melioris notæ. *Fahrenheit.*	1,409*
Eadem, duplex. *Freind.*	1,340
Aqua fortis. *Cotes.*	1,300
Eadem, ſimplex. *Freind.*	1,100
Solutio ſalis comm. in aqua ſaturata. *Davies.*	1,244
Eadem, 1 in aquæ 2,7 part. ponderis. *Davies.*	1,240
Eadem, 1 in aquæ 3 part. *Davies.*	1,217
Eadem, 1 in aquæ 3 part. *Freind.*	1,146
Eadem, 1 in aquæ 12 part. *Davies.*	1,060
Soap Lees the ſtrongeſt. *Jurin.*	1,200
D°. Capital. *Jurin.*	1,167
Spirit of Vitriol. *Freind.*	1,200
Spiritus Salis cum Ol. Vitriol. *Muſſchenb.*	1,154
Idem, &c. *Freind.*	1,146
Spirit of ſalt. *Cotes.* Sp. Salis marini. *Muſſchenb.*	1,130
Sp. Salis communis. *Freind.*	1,037
Sp. Salis dulcis. *Muſſchenb.*	0,951
Id. *Freind.*	0,890

Sp.

Sp. Salis Ammoniaci ſuccinat. Item, cum ciner. clavellat. *Freind.*	1,120
Sp. Salis Ammoniac. cum calce. *Muſſchenb.*	0,952
Idem cum calce viva. *Freind.*	0,890
Sp. Cornu Cervi non rectific. *Freind.*	1,073
Sp. Serici. *Muſſchenb.*	1,145
Sp. Urinæ. *Cotes.*	1,120
Solutio Salis enixi, 1 in aquæ 5 part. *Freind.*	1,100
Oleum Saſſafras. *Muſſchenb.*	1,094
Decoctio Gentianæ. *Freind.*	1,080
Sp. Tartari. *Freind. Muſſchenb.*	1,073
Decoctio Biſtortæ. *Freind.*	1,073
Decoctio Sarzæ. It. Chinæ. *Freind.*	1,049
Decoctio ari. It. Sp. Salis comm. *Freind.*	1,037
Oleum Cinnamomi. *Muſſchenb.*	1,035
Ol. Caryophyllorum. *Muſſchenb.*	1,034
Beer-Vinegar. *Oxf. Soc.*	1,034
Acetum Vini. *Muſſchenb.*	1,011
Id. diſtillatum. *Muſſchenb.*	0,994
Acetum. *Freind.*	0,976
Sack. *Oxf. Soc.*	1,033
Sp. Ambræ. *Muſſchenb.*	1,031
Sea-Water. *Cotes.*	1,030
D°. ſettled clear. *Oxf. Soc. Ward.*	1,027
College plain ale. *Oxf. Soc.*	1,028
Solutio Aluminis, 1 in aquæ 5,33 part. Item Solutio Sal. Amm. purif. 1, et vitriol. alb. 1 in aquæ 5 part. *Freind.*	1,024
Laudanum liquidum Sydenhami. It. Panacea Opii. *Freind.*	1,024
Decoctio Cort. Peruv. Item, Granatorum. *Freind.*	1,024
Moil Cyder, not clear. *Oxf. Soc.*	1,017
Aqua fluviatilis. *Muſſchenb.*	1,009
Tinctura Aloes cum aqua. Item, Decoctio Santali rubri *Freind.*	1,000
Rain water. *Newton, Reynolds.* Common water. *Cotes.* Common clear water. *Ward.* Pump water. *Oxf. Soc. J. C.* Aqua. *Ghetaldus.* Aqua pluviatilis. *Fahrenheit, Muſſchenb.* &c.	1,000
Aqua vel vinum. *Villalpandus.*	1,000
Aqua putealis. *Muſſchenb.*	0,999
Oleum Fœniculi. *Muſſchenb.*	0,997
Oleum Anethi. *Muſſchenb.*	0,994
Aqua diſtillata. *Muſſchenb.*	0,993
Wine, Claret. *Oxf. Soc.*	0,993
D°. red. *Ward.*	0,992
Vinum. *Petitus.*	0,984
Id. *Ghetaldus.* (ad aquam ut 98 $\frac{1}{3}$ ad 100)	0,983

Vinum

Vinum Burgundicum. *Muſſchenb.*	0,953
Oleum Sabinæ. It. Hyſſopi. *Muſſchenb.*	0,986
Ol. Ambræ. It. Pulegii. *Muſſchenb.*	0,978
Ol. Menthæ. It. Cumini. *Muſſchenb.*	0,975
Decoctio Sabinæ. *Freind.*	0,960
Infuſio Marrhubii. It. Menthæ. It. Abſynth. *Freind.*	0,950
Ol. Nucis Moſchatæ. *Muſſchenb.*	0,948
Ol. Tanaceti. *Muſſchenb.*	0,946
Ol. Origani. It. Carvi. *Muſſchenb.*	0,940
Elixir propr. cum Sale volat. It. Infuſio Theæ. *Freind.*	0,940
Ol. Spicæ. *Muſſchenb.*	0,936
Ol. Roriſmarini. *Muſſchenb.*	0,934
Linſeed Oil. *Newton. C.*	0,932
D°. *Ward.*	0,931
Spirits of wine proof, or Brandy. *Ward.*	0,927
Sp. of wine well rectified. *Newton. C.*	0,866
Alcohol Vini. *Fahrenheit.*	0,826
Id. magis dephlegmatum. *Fahrenheit.*	0,825
Sp. Vini. *Freind.*	0,818
Id. rectific. *Freind.*	0,781
Eſprit de Vin etheré. *Muſſchenb.*	0,732
Spiritus Croci. *Freind.*	0,925
Lamp Oil. *Reynolds.*	0,924
Oleum. *Ghetaldus.* (ad aquam ut $91\frac{2}{3}$ ad 100.)	0,916
Oil Olive. *Newton. C.*	0,913
D°. *Ward.*	0,912
Sallad Oil. *Reynolds.*	0,904
Oleum. *Villalpandus.*	0,900
Id. *Petitus.*	0,891
Ol. Raparum. *Fahrenheit.*	0,913
Id. It. Tinct. Chalyb. Mynſicht. It. Tinct. Sulphur cum Sp. Terebinth. *Freind.* It. Huile de ſemences de navets. *Muſſchenb.*	0,853
Sp. Mellis. *Muſſchenb.*	0,895
Sp. Salis Ammoniaci cum calce viva.	0,890
Oleum Aurantiorum. *Muſſchenb.*	0,888
Spirit of turpentine. *Newton. C.*	0,874
Tinct. Caſtorei. Item Sp. Vini camphorat. *Freind.*	0,870
Oil of turpentine. *Boyle* V. 22. *a.*	0,864
Ol. Terebinth. *Freind.*	0,793
Ol. Ceræ. *Muſſchenb.*	0,831
Tinctura Corallii. *Freind.*	0,828
Aqua cocta. *Freind.*	0,750
Air. *Newton. C.*	0,00125
Aër *Princip. Edit.* 3. p. 512. Aër juxta ſuperficiem terræ occupat quaſi ſpatium 850 partibus majus quam aqua ejuſdem ponderis.	0,00118

The

The ſame, by an experiment made by the late Mr *Francis Haukſbee*, F. R. S. when the barometer ſtood at 29,7 inches. See *Phyſico Mathem. Exp.* p. 74. 0,00113

As to the abſolute weight of water with which all the other bodies are compared in theſe tables, Mr *Boyle* tells us in his *Medicina Hydroſt.* printed in the new edition of his works, V. 19. *b.* that he had found by his own experiments, that a cubic inch of clear water weighed 256 *Troy* grains. And Mr *Ward* of *Cheſter*, who afterwards purſued this affair with great accuracy, determined that a cubic inch of common clear water did weigh by his tryals 253.18 like troy grains, or 0.527458 decimals of the *Troy Ounce*, or 0.578697 of the *ounce averdupois*, agreeable to what Mr *Reynolds* had formerly delivered, who found the inch cubic of rain water to weigh by his experiments 0.579036 decimals of the ſame averdupois ounce, differing from the other only 0.000339 parts.

But, as the accuracy of all the experiments in theſe tables depends upon the identity of the weight of common water, it may not be improper to aſcertain that point by a note taken from Mr *Boyle's Medicina Hydroſtatica*, V. 18. *b.* where he expreſſes himſelf in the following manner.

—— "It ſpeciouſly may, and probably will, be objected, that —— "there may be a great diſparity betwixt the liquors that are called, and "that deſervedly, *common water*. And ſome travellers tell us from the "preſs, that the water of a certain eaſtern river, which if I miſtake not is "*Ganges*, is by a fifth part lighter than our water. But ——having had "upon ſeveral occaſions the opportunity as well as curioſity to examine "the weight of divers waters, ſome of them taken up in places very di-"ſtant from one another. I found the difference between their ſpecific "gravities far leſs than almoſt any body would expect. And if I be not "much deceived by my memory (which I muſt have recourſe to, becauſe "I have not by me the notes I took of thoſe trials) the difference be-"tween waters, where one would expect a notable diſparity, was but "about the thouſandth part (and ſometimes perchance very far leſs) of "the weight of either. Nor did I find any difference conſiderable in "reference to our queſtion, between the weight of divers waters of dif-"ferent kinds, as ſpring-water, river-water, rain-water, and ſnow-"water; though this laſt was ſomewhat lighter than any of the reſt. "And having had the curioſity to procure ſome water brought into *Eng-"land*, if I much miſremember not, from the river *Ganges*, itſelf; I "found it very little, if at all, lighter than ſome of our common "waters."

The heavieſt fluid we are acquainted with, next to *Mercury*, is *Oil* of *Vitriol*, or water impregnated with the *vitriolic acid* in the higheſt degree we can obtain it, being almoſt double the weight of water.

The next is probably the *ſaturated ſolution* of the *fix'd ſalt of vegetables* being a ponderous ſalt, and diſſolving freely in water.

The

The next to this is *ſpirit of nitre*. *ſpirit of ſalt* is lighter, and inferior in weight to the *ſaturated ſolution* of *ſalt* itſelf.

It is obſervable, that *marine* or *common ſalt* and *nitre* differ little in gravity, contrary to the nature of their *ſpirits*.

The ſeveral *ſolutions* of *common ſalt*, if accurately repeated, would ſhew in what proportion the gravities of fluids increaſe, upon the addition of ſalt: and that *ſea-water* does not contain one twenty-fourth part of ſalt.

I have omitted in this table the three animal fluids, milk, ſerum of blood, and urine, as the ſame may be ſeen before in the *8th* table, that of *animal parts*; but it may be noted in general, that the ſpecific gravity of all theſe fluids is nearly the ſame as that of ſea water.

There are in Dr *Freind*'s table ſeveral decoctions of plants, which I have inſerted, altho' they are not I think of much uſe, nor greatly to be depended upon. Several of them are lighter than common water, in contradiction to Dr *Jurin*'s obſervation, that *Vegetable parts* are all heavier that water: But it is probable theſe experiments were made before the *decoctions* were reduced to the temper of *common water*.

What is meant by the *aqua cocta* of Dr *Freind* in his table, I cannot imagine; not having any idea of ſuch a change by boiling or otherwiſe, as can deprive common water of a full fourth part of it's weight.

Since the denſity of the air is as the force by which it is compreſſed, it follows that the weight of any portion of air muſt vary in the ſame proportion with the weight of the whole *atmoſphere*: which in our climate is not leſs than $\frac{1}{10}$ of the whole weight, allowing the *Barometer* to vary from 28 to 31 inches.

Again, by an experiment of the late Mr *Haukſbee*'s in his *Phyſ. Mechan. Exp.* pag. 170. the denſity of the air varies one eighth part between the greateſt degree of heat in ſummer, and that of cold in the winter ſeaſon. So that the air, in a hard froſt, when the *Mercury* ſtands at 31 inches, is near a fifth part ſpecifically heavier, than it is in a hot day when the *Mercury* ſtands 28 inches.

Tab. XI. *From Monſ.* Homberg *and* John Caſpar Eiſenſchmid, *of the proportion of the ſpecific weights of certain fluids in the winter, to the weights of the ſame in the ſummer ſeaſon.*

Mercurius	1,00479
Aqua pluvialis	1,00809
Aqua fluviatilis	1,00811
Aqua diſtillata.	1,00815
Spirit. Vitriol.	1,01272
Lac bubulum	1,01316
Aqua marina	1,01351
Spir. Salis	1,01467
Acetum	1,01600
Ol. Vitrioli	1,02131
Ol. Terebinth.	1.02141
Aqua fortis	1,02637
Ol. Tartari	1,03013

Spir.

Spir. Vini	1,03125
Spir. Nitri	1,04386

The oils of olive and ſweet almonds congealing with the cold, could not be examain'd by the *Araeometer* in the winter ſeaſon.

According to this table, the increaſe of the ſpecific weight of common water in the winter above it's weight in the ſummer, is not more than about the one hundred and twenty-fourth part of the whole; which is little more than half of what Prof. *Muſſchenbroek* has elſwhere accounted the ſame, *deſorte qu' un pied cubique Rhenan d'eau, qui peſe environ* 64 *livres en Eté ſe trouvera être en Hiver de preſque* 65 *livres. Eſſai de Phyſique, p.* 424. but ſure this difference is much too great.

Notwithſtanding that all fluids are condenſed by cold, it is only till ſuch time as they are ready to freeze; for upon the freezing they immediately expand again, ſo as for the ice to be lighter ſpecifically than the fluid of which it is formed, and to ſwim in it: *Muſſchenbroek* gives the ſpecific weight of ice to be to that of water commonly as 8 to 9. *La peſanteur de la glace eſt ordinairment a celle de l'eau, comme* 8 *a* 9. p. 441. I am not acquainted with any other accurate experiments upon this ſubject and it is hard to get ice in which there are not large bubbles of air included.

The *Philoſ. Soc.* at *Oxford*, together with their table of *Specific Gravity* already ſo often mentioned in the foregoing pages, communicated beſides at the ſame time, to the *Royal Society*, another table of a groſſer nature indeed, but which being printed in the ſame Num. 169. of the *Philoſ. Tranſ.* and appearing to be of uſe for many purpoſes: I have thought the ſame not improper to be here alſo tranſcribed.

The following bodies were poured gently into the veſſel, and thoſe in the firſt 12 experiments were weigh'd in ſcales turning with two ounces; but the laſt 7 were weighed in ſcales turning with one ounce. The pounds and ounces here mentioned are averdupois weight.

Of the weight of a cubic foot of divers grains, &c. tried in a veſſel of well-ſeaſon'd Oak, whoſe concave was an exact cubic foot.

	lb	℥
1. A foot of *Wheat* (worth 6*s.* a buſhel).	47	8
2. *Wheat* of the beſt ſort (worth 6*s* 4*d.* a buſhel). Both ſorts were red *Lammas Wheat* of laſt year.	48	4
3. The ſame ſort of *Wheat* meaſured a ſecond time.	48	2
4. White *Oats* of the laſt year. The beſt ſort of *Oats* were 2*d.* in a buſhel better than theſe.	29	8
5. Blue *Peaſe* (of the laſt year) and much worm-eaten.	49	12
6. White *Peaſe* of the laſt year but one.	50	8
7. *Barley* of the laſt year (the beſt ſort ſells for 1*s.* 6*d.* in a quarter more than this).	41	2
8. *Malt* of of the laſt year's *Barley*, made 2 months before.	30	4
9. Field *Beans* of the laſt year but one.	50	8
10. Wheaten *Meal* (unſifted).	31	0
11. Rye *Meal* (unſifted).	28	4

	℔	ʒ
12. Pump-*Water.*	62	8
13. Bay *Salt.*	54	1
14. White Sea-*Salt.*	43	12
15. *Sand.*	85	4
16. Newcaſtle *Coal.*	67	12
17. Pit *Coal*, from *Wedneſbury* 63; but this is very uncertain in the filling the interſtices betwixt the greater pieces.	63	0
18. *Gravel.*	109	5
19. Wood *Aſhes.*	58	5

Of the ſame nature is alſo the following account of *The difference of the weight of ſome liquors upon the tun compared to rain water*, from the experiments made formerly by Mr *Reynolds* in the *Tower* of *London*, and communicated to the *Royal Society*, with his others before-mentioned, by Mr *Smethwick*, *July* 7. 1670.

	℔	ʒ	*Averd.*
Muſcadine wine was found heavier than rain water	11	2	
Milk	8	4	
Sherry	5	3	
Ale	5	2	
Canary Wine	3	3	
Small Beer	1	3	
White wine was found lighter than rain water	1	2	
Rheniſh wine	1	4	
Claret	1	6	
Sallet Oil	21	6	

The proportion given by this author as the true one of the *Averdupois* pound to the *Troy* pound is, that 14 of the former are equal to 17 of the latter.

From whence the *Averdupois* pound would be found equal to 6994.285 and the *ounce* to 437.143 *Troy grains*; which is indeed a little leſs than the ſame have ſince been determined by others; for Mr *Ward* of *Cheſter* gives from a very nice experiment as he calls it, of his own, that one pound averdupois was equal to 14 ounces 11 penyweight and 15½ *Troy grains*, or to 6999½, and conſequently the ounce averdupois to 437.47 of the ſame grains. And ſeveral gentleman of the *Royal Society* who very carefully on 22 *April* 1743. examined the original ſtandards of weights kept in the *Chamberlain's Office* of his MAJESTY'S *Exchequer*, found, upon the medium of the ſeveral trials which they made with thoſe ſtandards, that the *Pound Averdupois* was equal to 7000.14, and the *Ounce Averdupois* to 437.51 *Troy grains*. *Phil. Tranſ.* N°. 470.

I ſhall conclude theſe papers with the two tables from *Marinus Ghetaldus* mentioned in the beginning, which I here tranſcribe, with an account of ſome of their uſes, in his own words.

Ad

Ad comparandum inter se duodecim corporum genera, gravitate, & magnitudine Tabella.

	Aurum	Arg. vivum.	Plumb.	Argent.	Æs	Ferrum	Stann.	Mel	Aqua	Vinum	Cera	Oleum
Oleum	$20\frac{8}{11}$	$14\frac{62}{77}$	$12\frac{6}{11}$	$11\frac{3}{11}$	$9\frac{9}{11}$	$8\frac{8}{11}$	$8\frac{4}{55}$	$1\frac{32}{55}$	$1\frac{1}{11}$	$1\frac{4}{55}$	$1\frac{5}{221}$	1
Cera	$19\frac{12}{21}$	$14\frac{32}{147}$	$12\frac{1}{21}$	$10\frac{52}{63}$	$9\frac{9}{21}$	$8\frac{8}{21}$	$7\frac{89}{105}$	$1\frac{109}{210}$	$1\frac{1}{21}$	$1\frac{13}{420}$	1	
Vinum	$19\frac{19}{59}$	$13\frac{331}{413}$	$11\frac{41}{59}$	$10\frac{30}{59}$	$9\frac{9}{59}$	$8\frac{8}{59}$	$7\frac{31}{59}$	$1\frac{28}{59}$	$1\frac{1}{59}$	1		
Aqua	19	$13\frac{4}{7}$	$11\frac{1}{2}$	$10\frac{1}{3}$	9	8	$7\frac{2}{3}$	$1\frac{9}{20}$	1			
Mel	$13\frac{3}{29}$	$9\frac{73}{203}$	$7\frac{27}{29}$	$7\frac{11}{87}$	$6\frac{6}{29}$	$5\frac{15}{29}$	$5\frac{3}{29}$	1				
Stannum	$2\frac{21}{37}$	$1\frac{221}{259}$	$1\frac{41}{74}$	$1\frac{44}{111}$	$1\frac{8}{37}$	$1\frac{3}{37}$	1					
Ferrum	$2\frac{3}{8}$	$1\frac{39}{56}$	$1\frac{7}{10}$	$1\frac{7}{24}$	$1\frac{1}{8}$	1						
Æs	$2\frac{1}{9}$	$1\frac{32}{63}$	$1\frac{5}{18}$	$1\frac{4}{27}$	1							
Argentum	$1\frac{26}{31}$	$1\frac{68}{217}$	$1\frac{7}{62}$	1								
Plumbum	$1\frac{15}{23}$	$1\frac{29}{161}$	1									
Arg. viv.	$1\frac{38}{95}$	1										
Aurum	1											

Quæro, exempli gratia, quam habet rationem in gravitate plumbum ad aurum, Intelligatur plumbum, quoniam levius est auro, gravitatem habere 1, et in linea plumbi, in prima columna nominata, sub titulo auri, quæratur auri gravitas, ea erit $1\frac{15}{23}$. Plumbum igitur ad aurum rationem habebit in gravitate ut 1, ad $1\frac{15}{23}$. Si enim sumantur duo corpora magnitudine æqualia, unum plumbeum alterum aureum, sit autem plumbei corporis gravitas 1, aurei erit $1\frac{15}{23}$; quare corpus plumbeum ad corpus aureum ejusdem magnitudinis rationem habebit in gravitate ut 1,

ad $1\frac{1}{2}\frac{5}{3}$. Comparantur autem inter ſe genera diverſa gravitate, in corporibus magnitudine æqualibus.

Rurſus, quæro quam habet rationem in gravitate aqua ad argentum vivum. Intelligatur aqua, ut levior argento vivo gravitatem, habere 1, et in linea aquæ, ſub titulo argenti vivi, quæratur argenti vivi gravitas, ea erit $13\frac{+}{7}$; aqua igitur ad argentum vivum rationem habebit in gravitate ut 1, ad $13\frac{+}{7}$.

Contra, quæro quomodo ſe habent in magnitudine aurum et plumbum. Intelligatur aurum, quoniam gravius eſt plumbo, magnitudinem habere 1, et in linea plumbi, ſub titulo auri, quæratur plumbi magnitudo ea erit $1\frac{1}{2}\frac{5}{7}$; aurum igitur ad plumbum ſe habebit in magnitudine ut 1, ad $1\frac{1}{2}\frac{5}{7}$: ſi enim ſumantur duo corpora æque gravia, unum aureum, alterum plumbeum, ſit autem corporis aurei magnitudo 1, plumbei erit $1\frac{1}{2}\frac{5}{3}$; quare corpus aureum ad corpus plumbeum ejuſdem gravitatis ſe habebit in magnitudine ut 1, ad $1\frac{1}{2}^{5}$. Comparantur autem inter ſe genera diverſa magnitudine, in corporibus æque gravibus.

Quæro denique, quomodo ſe habent in magnitudine ferrum, et aqua, ponatur ferrum, ut gravius aqua magnitudinem habere 1, et in linea aquæ, ſub titulo ferri, quæratur aquæ magnitudo, ea erit 8, ferrum igitur ad aquam ſe habebit in magnitudine ut 1, ad 8.

Altera,

Altera, ad comparandum inter se duodecim corporum genera, gravitate, et magnitudine, Tabella.

	Aurum.	Arg. viv	Plum.	Argent.	Æs	Ferrum	Stann.	Mel.	Aqua	Vinum	Cera	Oleum
Aurum	100	71$\frac{3}{7}$	60$\frac{10}{19}$	54$\frac{22}{57}$	47$\frac{2}{19}$	42$\frac{2}{19}$	38$\frac{18}{69}$	7$\frac{12}{190}$	5$\frac{5}{19}$	5$\frac{10}{57}$	5$\frac{5}{209}$	4$\frac{47}{57}$
Arg. viv.	100	100	84$\frac{14}{19}$	76$\frac{8}{57}$	66$\frac{6}{19}$	58$\frac{18}{19}$	54$\frac{10}{19}$	10$\frac{13}{19}$	7$\frac{7}{19}$	7$\frac{14}{57}$	7$\frac{7}{209}$	6$\frac{43}{57}$
Plumbum	100	100	100	89$\frac{52}{69}$	78$\frac{6}{23}$	69$\frac{13}{23}$	64$\frac{8}{23}$	12$\frac{19}{23}$	8$\frac{16}{23}$	8$\frac{38}{69}$	8$\frac{76}{253}$	7$\frac{67}{69}$
Argent.	100	100	100	100	87$\frac{3}{31}$	77$\frac{13}{31}$	71$\frac{12}{31}$	14$\frac{1}{31}$	9$\frac{21}{31}$	9$\frac{16}{31}$	9$\frac{81}{341}$	8$\frac{27}{31}$
Æs.	100	100	100	100	100	88$\frac{8}{9}$	82$\frac{2}{9}$	16$\frac{1}{9}$	11$\frac{1}{9}$	10$\frac{25}{27}$	10$\frac{20}{33}$	10$\frac{5}{27}$
Ferrum	100	100	100	100	100	100	92$\frac{1}{2}$	18$\frac{1}{8}$	12$\frac{1}{2}$	12$\frac{7}{24}$	11$\frac{41}{44}$	11$\frac{11}{24}$
Stannum	100	100	100	100	100	100	100	19$\frac{27}{37}$	13$\frac{12}{37}$	13$\frac{32}{111}$	12$\frac{366}{407}$	12$\frac{43}{111}$
Mel	100	100	100	100	100	100	100	100	68$\frac{28}{29}$	67$\frac{71}{87}$	65$\frac{265}{319}$	63$\frac{19}{87}$
Aqua	100	100	100	100	100	100	100	100	100	98$\frac{1}{3}$	95$\frac{5}{11}$	91$\frac{2}{3}$
Vinum	100	100	100	100	100	100	100	100	100	100	97$\frac{47}{649}$	93$\frac{13}{59}$
Cera	100	100	100	100	100	100	100	100	100	100	100	96$\frac{2}{63}$
Oleum	100	100	100	100	100	100	100	100	100	100	100	100

Quæro, exempli gratia, quænam sit ratio in gravitate, auri ad argentum. Intelligatur aurum quoniam gravius est argento, gravitatem habere 100, et in linea auri, sub titulo argenti, reperietur argenti gravitas $54\frac{22}{57}$, aurum igitur ad argentum rationem habebit in gravitate ut 100, ad $54\frac{22}{57}$. Si enim sumantur duo corpora, magnitudine æqualia, unum aureum alterum argenteum, sit autem aurei corporis gravitas 100, erit

argentei

argentei $54\frac{2}{5}\frac{2}{7}$; quare corpus aureum, ad corpus argenteum ejuſdem magnitudinis, rationem habebit in gravitate, ut 100, ad $54\frac{2}{5}\frac{2}{7}$.

Quæro, quomodo ſe habet in gravitate aqua ad vinum; quoniam aqua gravior eſt vino, intelligatur ejus gravitas 100, et quoniam in linea aquæ, ſub titulo vini, datur vini gravitas $98\frac{1}{3}$, aqua ad vinum ſe habebit in gravitate, ut 100, ad $98\frac{1}{3}$.

Contra quæro quomodo ſe habent in magnitudine argentum, et aurum. Intelligatur argentum ut levius auro, magnitudinem habere 100, et in linea auri, ſub titulo argenti, quæratur auri magnitudo, ea erit $54\frac{2}{5}\frac{2}{7}$, argenteum igitur ad aurum ſe habebit in magnitudine, ut 100, ad $54\frac{2}{5}\frac{2}{7}$. Si enim ſumantur duo corpora æqua gravia, unum argenteum, alterum aureum, ſit autem argentei corporis magnitudo 100, erit aurei $54\frac{2}{5}\frac{2}{7}$; quare corpus argenteum, ad corpus aureum ejuſdem gravitatis, ſe habebit in magnitudine, ut 100, ad $54\frac{2}{5}\frac{2}{7}$.

Quæro denique, quomodo ſe habent in magnitudine aqua et argentum vivum. Quoniam aqua levior eſt argento vivo, intelligatur ejus magnitudo 100, et in linea argenti vivi, ſub titulo aquæ, quæratur argenti vivi magnitudo, et reperietur $7\frac{7}{19}$, aqua igitur ad argentum vivum ſe habebit in magnitudine, ut 100, ad $7\frac{7}{19}$.

A letter from Robert Southwell, *Eſq; to Mr* Henry Oldenburg, *concerning ſome extraordinary* Ecchoes, *lately communicated to the* R. S. *by the Rev.* Henry Miles, *D. D. & F.* R. S. No. 480. p. 219. May *and* June 1746, dated Kingſale, Sept. 19. 1661. *Read* June 5. 1746.

IX. I muſt needs account myſelf very happy, in that I partake ſo conſtant and freſh intelligence of the matters of the world; and that from ſo active a hand, as that I know no example of greater exactneſs and induſtry any where than what is with you.

I am very much rejoiced at the happy advancement of learning in the *Royal Society*; and that the radiant influence of His Majeſty is like to ſmile upon it. And as to your query concerning ſounds and ecchoes, I do remember, that the Duke of *Tuſcany* * has made rare trials concerning the velocity in the motion of ſound; and I gave Mr *Boyle*, in almoſt a ſheet of paper, an Account and a Diſcourſe upon thoſe experiments, and the manner of them.

As for whiſpering places, the beſt I ever ſaw was that at *Glouceſter*: but in *Italy*, in the way to *Naples*, two days from *Rome*, I ſaw, in an inn, a room with a ſquare vault, where whiſpering, you could eaſily hear it at the oppoſite corner, but not in the leaſt manner at the ſide corner that was nearer to you.

I ſaw another, in the way from *Paris* to *Lyons*, in the porch of a common inn, which had a round vault; but neither of theſe were comparable to that of *Glouceſter*; only the difference between theſe two laſt was, that to this, holding your mouth to the ſide of the wall, ſeveral could hear you on the other ſide; the voice being more diffuſed. But, to the former, it being a ſquare room, and you whiſpering in the corner, it was only audible in the oppoſite corner; and not to any diſtance from thence,

* See the Exp. of the Academy *del Cimento*.

as

as to diſtinction of the words. And this virtue was common to each corner of the room, and not confined to one.

As to ecchoes, there is one at *Bruſſels* that anſwers 15 times: but when I was at *Milan*, I took a coach to go two miles from thence to a nobleman's palace, now not in great repair, and only a peaſant or contadine living in one end of it. The building is of ſome length in the front, and has two wings jetting forward; ſo that it wants only one ſide of an oblong figure. About 100 paces before the houſe, there runs a ſmall brook, and that very ſlowly; over which you paſs from the houſe into the garden. We carried ſome piſtols with us; and, firing one of them, I heard 56 reiterations of the noiſe. The firſt 20 were with ſome diſtinction; but then, as the noiſe ſeemed to fly away, and anſwer at a great diſtance, the repetition was ſo doubled, as that you could hardly count them all; ſeeming as if the principal ſound was ſaluted in its paſſage by reports on this and that ſide at the ſame time.

There were of our company that reckoned above 60 reiterations when a louder piſtol went off; and indeed it was a very grateful divertiſement. But on the other ſide the houſe, on the oppoſite wing, it would not ſound; and only (to this advantage) in a certain chamber here two ſtories high from the ground.

CHAP. V.

HYDRAULICKS.

I. MR *Philip Williams*, chief engineer to our water-works at *Norwich*, a man of great ingenuity, who, in his time, has been author of many curious inventions, has contrived lately a machine for the raiſing of water to ſupply cities, drain marſhy grounds, or other uſeful purpoſes, where no head of water can be procured, and the current runs very ſlowly: circumſtances which render moſt other engines uſeleſs.

A Deſcription of a Water-Wheel *for Mills, invented by Mr* Philip Williams. *In a letter from Mr* Wm. Arderon, *F. R. S. to Mr* Baker, *F. R. S.* N°. 478. p. 1. Jan. & Feb. 1746. dated *Norwich, May* 30. 1745. *Read* Jan. 9. 1745-6.

With his leave, I now ſend you a drawing of this machine, which I ſhall endeavour to explain in a manner to be underſtood.

The *axis* of the firſt mover is cut into the form of an hexangular priſm, of dimenſions ſuitable to the force requir'd, as is repreſented by the letter *A*. Into this, ſeveral ſets of holes are mortiſed, as *B B B*. Theſe are intended to receive different ſets of ſails made of iron plates, one whereof is repreſented in Fig. 54. all which ſails are weathered in the ſame manner as thoſe deſigned for windmills; only in theſe the extremity of their ends ſtands parallel to the planes of each end of the *axis*, thoſe ends I mean which are placed fartheſt from the centre.

Fig. 53.

This hexangular *axis*, when employed, muſt be placed parallel to the moving ſtream, and may lie even with its ſurface: but the engine will act

Fig. 54.

moſt

moſt vigorouſly, when it and all the ſails employed are entirely under water, as is eaſy to comprehend. Each ſet of the ſails before deſcribed contains ſix in number, and are ſo contrived as to be put in and taken out at pleaſure; whence it follows, that when a ſingle ſet of ſails is made uſe of, the engine produceth a ſingle effect, when two ſets a double, and ſo on, till the deſired *momentum* is acquired, with the ſame quantity of running water, provided there be room to fix a ſufficient number of ſails.

It is farther to be obſerved, that when this engine is placed with it's ſails made and weathered as above directed, they will move with equal velocity, even ſuppoſing the current ſhould change it's courſe, and come upon them in a quite contrary direction, as the caſe really happens in rivers where the tide ebbs and flows; where moſt other engines yet invented are of little ſervice.

About ſix weeks ago I had the pleaſure to ſee a model of this engine tried. It was fixed in our river, in a place where the water moved only 27 feet in 20″ in which time the firſt mover made ſix revolutions. It's diameter was no more than two feet and two inches; yet it would have lifted 14 pounds two yards high in the above-mentioned time, had not a misfortune happend to it's caſe which made it not perform quite ſo much.

It appeared to me ſomewhat extraordinary, that the circumference of it's firſt mover (I mean any determined part thereof) paſſed through a ſpace of 42 feet in 20″; which is nearly twice as faſt as the motion of the water: and as the *momentum* will be in proportion to the number of the ſets of ſails that are employed, it's force is capable of being greatly augmented with the ſame quantity of water: a thing not to be admitted without ſufficient experiment, but what ſeems extremely plain in Theory, and what I am apt to think will anſwer when brought to Practice.

This engine, when once ſeen, requires little ſkill for the conſtruction of it, is made at a ſmall expence, and kept in repair with eaſe.

A deſcription of a Clepſydra, *or Water-Clock; by the Hon.* Charles Hamilton, *Eſq*; N°. 479. p. 171. Mar. *and* Apr. 1746. *Read* April 24. 1745-6. Fig. 55. *The Machine in perſpective.*

II. An open canal *e e*, is ſupplied with a conſtant and equal ſtream by the ſiphon *d*; and has at each end *f f*, open pipes, of exactly equal bores, which deliver the water that runs along the canal *e*, alternately into the veſſels *g* 1, *g* 2, in ſuch a quantity as to raiſe the water from the mouth of the *tantalus s*, to the top of the *tantalus t*, exactly in an hour. The canal *e e*, is equally poiſed by the two pipes *f* 1, *f* 2, upon a centre *r*; the ends of the canal *e*, are raiſed alternately, as the cups *z z*, are depreſſed, to which they are connected by lines running over the pulleys *l l*. The cups *z z*, are fixed at each end of the balance *m m*, which moves up and and down upon it's centure *v* *.

* The *letters* of reference anſwer to Fig. 55, 56, *and* 57. ſome being ſeen in one, that do not come in ſight in the others.

n 1,

n 1, *n* 2, The edges of two wheels or pulleys, moving different ways alternately, and ſo fitted to the cylinder *o* (by oblique teeth both in the cavity of the wheel, and upon the cylinder; which, when the wheel *n* moves one way [*i. e.* in the direction of the minute-hand], meet the teeth of the cylinder, and carry the cylinder with it; and, when *n* moves the contrary way, ſlip over thoſe of the cylinder, the teeth no more meeting, but receding from each other; or it may be done by catches or locks which require a longer deſcription), one or other of theſe wheels, *n n*, continually moves *o* in the ſame direction, with an equal and uninterupted motion: for the contrivance is ſuch, that the inſtant one ceaſes to act, the other begins, and ſo on.

A fine chain goes twice round each wheel, having at one end a weight, *x*, always out of water, which equiponderates with *y* at the other end, when kept floating at the ſurface of the water in the veſſel *g*, which *y* muſt always be. The two cups *z z*, one at each end of the balance *m m*, keep it in *equilibrio*, till one of them is forced down by the weight and impulſe of the water, which it receives from the *tantalus s t i*: each of theſe cups *z z*, has likewiſe a *tantalus* of it's own *h h*, which empties it after the water has done running form *g*, and leaves the two cups again *in equilibrio*; *q* is a drain to carry off the water.

Fig. 56. repreſents the dial-plate, with the hour and minute-hands, the weight and float belonging to *n* 2. The front of the *tantalus* in *g* 2, marked *s t i*, of which *s* the mouth is 18 inches above the bottom of the veſſel *g*, and 18 inches below the top of the *tantalus t*. *i* is the iſſuing leg of the *tantalus*, which diſcharges the water out of the veſſel *g* into the cup *z*, as ſoon as it runs over the top *t*, till the water ſinks as low as *s*.

Fig. 56. *The front of the* Clepſydra.

Fig. 57. *The profile of the* Clepſydra.

Fig. 58. *The plan of the* Clepſydra *to it's full dimenſion.*

The caſe *u u* incloſes the whole machine, except the ciſtern that ſupplies the ſiphon *d*, which may be placed at any diſtance from it, as is moſt convenient, provided the iſſuing leg *d*, of the ſiphon is lengthened out ſo as to give a conſtant ſtream into the canal *e*. This caſe *u u* ſupports the *axis* of the cylinder *o* behind, and the dial-plate *p p* before; in the centre of which turns the *axis o*, with the index *k* at it's extremity, being the minute-hand. The hours may be deſcribed by two common wheels, as in ordinary clock-work. For cheap work, chains paſſing round pulleys would do inſtead of wheels with teeth.

The motion of the Clepſydra *is effected in the following manner.*

The ſhort leg of the ſiphon *d* is placed in a ciſtern, with it's mouth ſomething below the mouth of the waſte-pipe; which ciſtern is ſupplied with a conſtant ſtream, rather more than runs out at the ſiphon *d*; which overplus going off at the waſte-pipe, the water always remains at the ſame height in the ciſtern, and yet always delivers a conſtant and equal flow into the canal *e e*; conſequently, there is not the leaſt intermiſſion. As the end of the canal *e*, fixed to the pipe *f* 1, is in the figure the loweſt, the water runs all through the pipe *f* 1, into the veſſel *g* 1, till it runs over the top of the *tantalus t*; when it immediately runs out at *i* into the cup *z*, at the end of the balance *m*, and forces it down, the balance *m* moving on it's centre *v*. When one ſide of *m* is brought down, the ſtring which connects

it to *f* 1, running over the pulley *l*, raises the end *f* 1, of the canal *e*, (which turns upon it's center *r*) higher than *f* 2 ; consequently, all the water which constantly runs through the siphon *d*, instantly runs through *f* 2 into *g* 2, till the same operation is performed in that vessel, and so on alternately.

As the height the water rises in *g* in an hour, *viz.* from *s* to *t*, is equal to the circumference of *n*, the float *y* rising that height along with the water, lets the weight *x* act upon the pulley *n*, which carries with it the cylinder *o* ; and, giving a revolution, makes the index *k* describe an hour upon the dial-plate. This revolution is performed by the pulley *n* 1 ; the next is to be by *n* 2, whilst *n* 1 goes back, as the water in *g* 1 runs out through the *tantalus* ; for *y* must follow the water, as it's weight increases out of water.

The *axis o* always keeps moving the same way ; the index *k* describes the minutes ; the *tantalus's* must be wider than the siphon *d*, that the vessels *g g* may be sure to be empty as low as *s*, before the water returns to them.

CHAP. VI.

GEOGRAPHY and *NAVIGATION.*

Observations determining the Longitude of Kingston *in* Jamaica, *by Mr* Ja. Short. *F. R. S.* N°. 496. p. 523. Nov. *&c.* 1750. *Read* Nov. 1. 1750.

I. I Take this opportunity of laying before the *Society*, two observations proper for determining the difference of longitude between *London* and *Kingston*, which came to my hands some time since. They were made by *Alex. Macfarlane*, Esq ; of *Kingston*, *F. R. S.* who is provided with a compleate *apparatus* of astronomical instruments, which he purchased of *Colin Campbell*, Esq; As this gentleman is well versed both in the Theory and Practice of Astronomy ; I think the following observations may be depended on for fixing the longitude of *Kingston* ; especially as we have the same observations made at the house of Mr *G. Graham* in *Fleetstreet*, *Lond.* *

Eclipse *of the* Moon, Oct. 22, 1743.

	h	′	″
Beginning of total darkness at - - - - - - - - - -	9	10	58
End of the eclipse at - - - - - - - - - - - - - -	10	51	33

Transit of Mercury *over the* Sun, Oct. 25, 1743.

The ingress of *Mercury* upon the sun could not be seen ; the sun being then below the horizon.

Excessus e disco Solis, or the last exterior contact, at $7^h\ 56'\ 43''$ *a. m.*

By the first observation of the eclipse of the moon, compared with the same eclipse observed here, *Kingston* is found to be $5^h\ 6'\ 2''$ to the west of *London*.

* See Vol. VIII. p. 172 and 202.

And

Fig. 57.

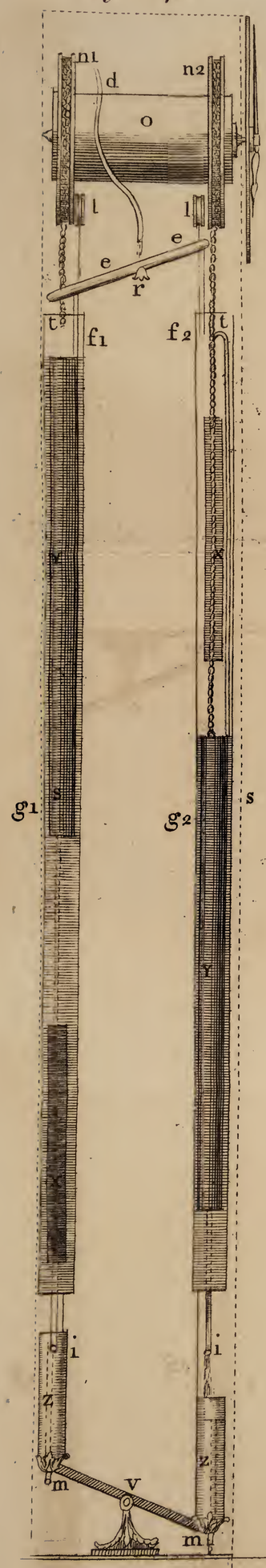


Fig. 55.

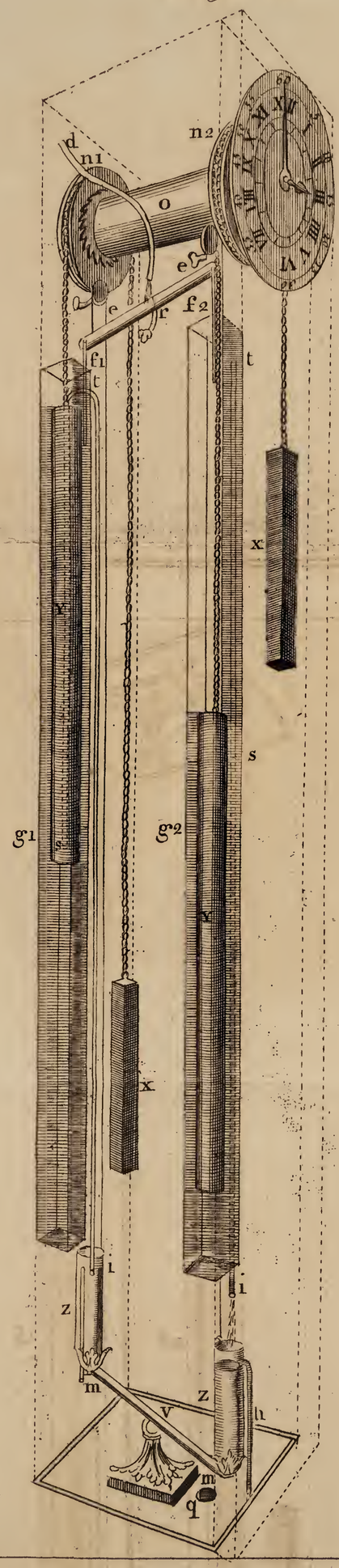


Fig. 56.

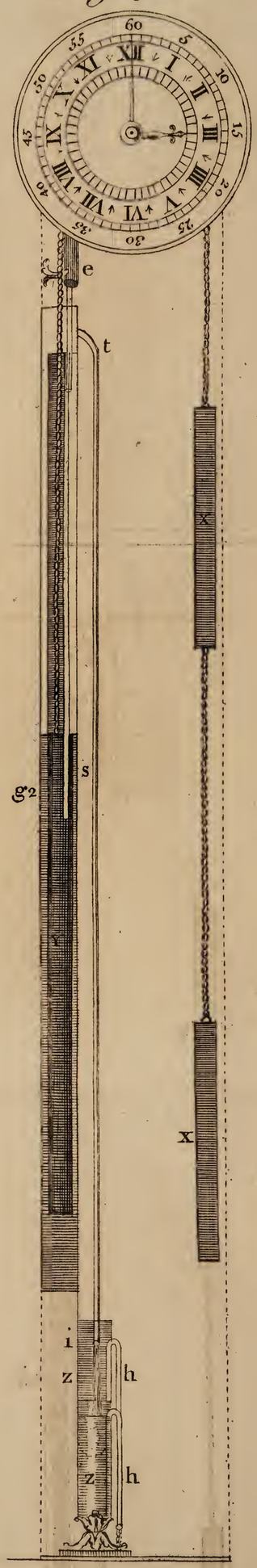


Fig. 53.

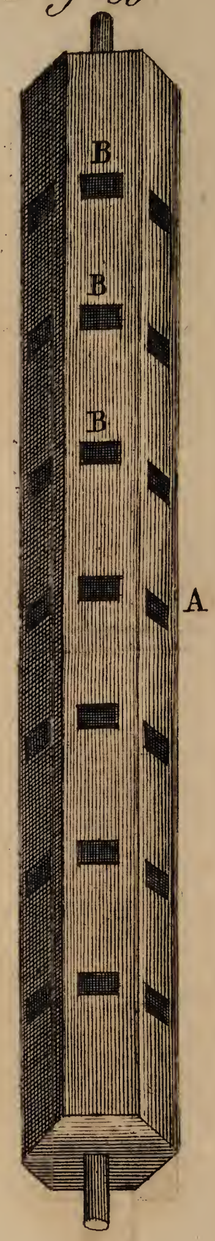
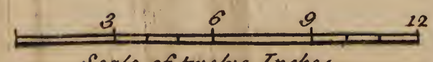


Fig. 54.



Scale of twelve Inches.

And by the *Transit* of *Mercury* neglecting his parallax, *Kingston* is found to be $5^h\ 5'\ 33''$.

This last is the most to be depended on for settling the longitude of *Kingston*; because in all observations of an eclipse of the moon, an error of a minute or two may be allowed, arising from the indistinctness of the penumbra.

II. As you are desirous to hear something more particular concerning the *Russian* expeditions to the North and North-East of *Asia*, I will here give you an account of all that has come to my knowledge relating to the same. But as I should, on the one hand, be very glad that these observations might give any light concerning the passage now sought through *Hudson's Bay*, I should, on the other be very sorry, if Mr *Behring*'s opinion, who believed that the new land he had discovered was joined to *California*, should rather lead us to doubt of the success of that glorious undertaking. I wish, however, that a happy experiment may soon inform us certainly of the truth. In the mean time you will not be sorry to be acquainted with the reasons upon which Mr *Behring*'s suspicions were founded, notwithstanding the objections you have been pleased to make, and to communicate to me upon that head.

Extract of a letter from Mr Leonard Euler, *Prof. Math. and Member of the* Imp. Soc. *at* Petersburgh, *to the Rev. Mr* Cha. Wetstein, *Chaplain and Sec. to his R. Highness the* Prince *of* Wales, *concerning the* Discoveries *of the* Russians *on the* N E. Coast *of* Asia. N°. 482. p. 421. Jan. *and* Feb 1747. *dated* Berlin, Dec. 10. 1746. *Read* Feb. 5. 1746-7.

First, This new land, which he fell in with at the distance of 50 *German* miles from *Kamschatka* towards the east, was followed by him, and coasted for a great way, though I cannot say how far: from whence alone it will appear, that an abatement must be made in the distance of 30°, or thereabouts, which you suppose to be between the last known head-land of *California* towards the west, and the farthest extremity of this new discovered land towards the east.

Secondly, Capt. *Behring* having had the opportunity of observing an eclipse of the moon at *Kamschatka*, concluded from the same, that that place lay much farther off to the east, than is expressed in any map; and that, to represent it truly, it ought to be transferred into the other hemisphere, as it's longitude is more than 180 degrees [E. from the Isle of *Ferro*]. For this reason Captain *Behring*'s new land will be considerably approached to the last known part of *California*, and will not indeed appear to be many degrees from it.

What we have therefore still to hope is only, that in this unknown district there may be found some streight, by which the *pacific sea* may freely communicate with *Hudson's Bay*; but if it shall appear that there is no such passage, it must then be concluded, that whatever further progress may happen to be made through *Hudson's Bay*, the opening at last must only be into the *frozen sea*, from whence there could be no passing into the *pacific ocean*, but by the neighbourhood of *Kamschatka*; and this way would without doubt be too long, and too dangerous, to be master'd in the course of one summer.

I very much doubt whether the *Russians* will ever publish the particulars of their discoveries, either such as have been made from *Kamschatka* towards

America, or ſuch as have been made upon the northern coaſts of *Aſia*. And indeed it is but very much in general that I know the ſucceſs of this laſt expedition. What I do was communicated to me by order of the Court, from the College of Admiralty, for me to make uſe of it in the Geography of *Ruſſia*, which I was at that time charged with.

They paſſed along in ſmall veſſels, coaſting between *Nova Zembla* and the Continent, at divers times, in the middle of ſummer, when thoſe waters are open. The firſt expedition was from the river *Oby*; and at the approach of winter the veſſels ſhelter'd themſelves by going up the *Jeniſka*; from whence the next ſummer they returned to ſea, in order to advance further eaſtward; which they did to the mouth of the *Lena*, into which they again retired for the winter-ſeaſon.

The third expedition was from this river, to the fartheſt North-Eaſt cape of *Aſia*. But here they loſt ſeveral of their boats, and a great part of their crew, ſo as to be diſabled from proceding, and from making the whole tour, ſo as to arrive at *Kamſchatka*.

It was however thought, that a further attempt was then unneceſſary, becauſe Captain *Behring* had already gone round that cape, ſailing northward from *Kamſchatka*.

The *Ruſſians* have not attempted the paſſage round *Nova Zembla*; but as they have paſſed between that land and the coaſt of *Aſia*, and as the *Dutch* did formerly diſcover the northern coaſts of *Nova Zembla*, we may now be well aſſured, that that country is really an iſland.

A letter from Arthur Dobbs, *Eſq; of* Caſtle-Dobbs *in* Ireland, *to the Rev. Mr* Chawetſtein, *Chap. and Sec. to the Prince of* Wales, *concerning the diſtances between* Aſia *and* America. N°. 483. p. 471. Mar. &c. 1747. *Read* April 9. 1747.

III. I am extremely obliged to you for the trouble you have taken, in correſponding with Prof. *Euler* * upon the *Ruſſian* diſcoveries eaſtward from *Kamſchatka*, and communicating to me the accounts he had of *Behring*'s laſt voyage, and of his diſcovery of the lands N. E. of *Japon*; which the Prof. could only have inaccurately, not having ſeen any journal to fix the Lat. and Long. of the countries he then diſcover'd: but ſince Prof. *Euler*, ſway'd by the opinion of Captain *Behring*, ſeems ſtill to believe that the laſt land he diſcover'd is joined to *California*, which country is now known to be part of the continent of *America*, and not an iſland (in which fact of it's being continuous to *California* I differ ſtill in opinion from him) for, if that were a fact to be depended upon, I would candidly own, that there could be no paſſage from the N. W. of *Hudſon's Bay* to the weſtern ocean of *America*, without ſailing near 70° of Long. the diſtance of the N. E. cape of *Aſia* from the N. W. of *Hudſon's Bay*, in a parallel almoſt as far N. as the polar circle, before the paſſage can be made to the *pacific ocean*; which might therefore be very reaſonably call'd an impracticable paſſage, as it could not poſſibly be made in one ſummer, (if at all) and ſince Prof. *Euler* has been ſo kind as to give me Capt. *Behring*'s reaſons for ſupporting his opinion, which are principally from the ſmall diſtance he ſuppoſed it was from the the coaſt he diſco-

* See the preceding article.

vered,

vered, to the weſtern *American* coaſt at *California* (which he imagined was much nearer his N. E. cape of *Aſia* than it is in fact); I muſt therefore, in return to the Profeſſor's goodneſs, in communicating to me all he has known in that diſcovery, beg leave to give you this further trouble of communicating to the Profeſſor my reaſon for ſtill diſſenting from *Behring*'s opinion, that the land he diſcovered laſt was part of the continent of *America*, or continuous with *California*; and if he find the reaſons for ſupporting my opinion make it more probable, that there ſtill may be a large opening betwixt theſe new-diſcovered countries and *California*, I am ſenſible it will give the ingenious and learned Profeſſor great pleaſure, to think we may yet hope for a paſſage by *Hudſon's Bay* to the weſtern *American* ocean, without being obſtructed with ice after paſſing *Hudſon's Streight*.

The Profeſſor imagines I might have been led aſtray, by not conſidering, that the N. E. cape of *Aſia* is much more eaſterly than has been laid down in any former charts; which is now known accurately, by the eclipſe of the moon obſerved by Captain *Behring* at *Kamſchatka*.

I have an abſtract of his Journal by me, upon his firſt diſcovery in 1728, and 1729, when he obſerved that eclipſe, and the calculation of the long. from it; and ſtand by his long. he has fixed; and allow that his N. E. cape is in the other hemiſphere; reckoning eaſtward, either from *Fero*, as the firſt meridian, or from *London*; which laſt I ſhall follow.

Behring fixes his N. E. cape 126° 7′ E. long. from *Tobolſki*; and *Tobolſki* is 86° E. from *Fero*; ſo the cape is 212° 7′ E. of *Fero*, or about 194° E. from *London*. —— By Captain *Middleton*'s obſervation of *Jupiter*'s *Satellite* at *Churchill* river in *Hudſon's Bay*, that river is 95° W. from *London*; which, added to 194°, makes 289°; conſequently the N. E. cape of *Aſia* is 71° diſtant from *Churchill*, to complete 360°; which, in the lat. of 65°, computing 8 leagues to a degree of long. of which 20 make a degree of lat. the diſtance betwixt that cape and *Hudſon's Bay* would be 568 ſuch leagues.

From the known long. of the N. cape of *Japon* in 40° lat. which is pretty exactly known, from the obſervations made by the Jeſuits at *Peking*, and is about 150° E. from *London*, and from the beſt computed long. of *California* in 40° N. lat. it lies in 130° long. W. from *London*, making together 280°, leaves 80° for the diſtance of *California* from *Japon*; allowing 17 leagues to a degree of long. in 40° N. lat. the diſtance would be about 1360 leagues: by the ſame calculation *California* muſt be at leaſt 7 or 800 ſuch leagues from the N. E. cape of *Aſia*; ſo that, in ſo great a ſpace there may be very great countries or iſlands *, without ſuppoſing the new diſcovered country continuous to *California*,

* The *Japoneſe*, in their maps of the World printed in *Japon*, have laid down in this very tract two iſlands as large as *Ireland*, with the names to them, as appears in that map bought by Dr *Kempfer* in *Japon* in 1686; now in Sir *Hans Sloane*'s Muſeum. *C. M.*

and

and might well allow of an open channel or ſea, from 50 to 100 leagues wide, between the diſcovered coaſt and *California*.

By the account given to Prof. *Euler*, *Behring* ſailed ſouthwardly to the iſles of *Japon*, and from thence ſailed eaſtwardly 50 *German* miles, about 250 *Engliſh* miles; which makes about 80 leagues, of 20 to a degree. At that diſtance from *Japon* he diſcovered land, which he coaſted N. W. ſtill approaching towards the N. E. cape, without going aſhore, until he came to the entrance of a great river; where ſending his boats and men aſhore, they never returned, being either loſt, killed, or detained by the natives, which made his diſcovery incomplete; his ſhip being ſtranded, and he afterwards died in an uninhabited iſland.

As no lat. nor long. are fixed by this account, I muſt believe he ſailed from *Kamſchatka* S. E. perhaps more ſoutherly than to 50° lat.; and there found land N. E. from *Japon*; otherwiſe, by coaſting it N. W. he could never approach the N. E. cape, which is, at leaſt 40° long. E. of *Japon*; and if he made land 80 leagues E. of *Japon*, he muſt have ſailed N. E. to make the N. E. cape. I have therefore reaſon to believe this coaſt was part of that he ſaw in his firſt voyage, where he loſt his anchor; and is the coaſt *Gama* diſcovered, and the *Dutch* afterwards called the *Company's Land*, E. of the ſtreights of *Uzicez*, which is at leaſt 7 or 800 leagues W. of any known land of *America*, and above 1000 near the lat. of *Japon*: ſo that, if I ſhould allow 700 leagues for countries or iſlands E. of his new-diſcovered coaſt, there might ſtill be a paſſage of 100 leagues for the ſouthern or *pacific ocean* to communicate with *Hudſon's Bay*, and to cauſe ſuch great tides and currents, as are found on the N. W. of *Hudſon's Bay*; as alſo a free paſſage for the whales, which are ſeen in all the openings N. W. of that bay, and are caught there in numbers by the *Eſkemaux* ſavages: for, as theſe don't go in by *Hudſon's Streight* from our *Atlantic Ocean*, it cannot be preſumed that they ſhould go up by *Japon* towards the N. E. cape, and from thence go 70°, or above 560 leagues, to *Hudſon's Bay*, and be there in *June*, and, after ſtaying until *Sept.* return again the ſame way to the ſouthern ocean, to paſs the winter. —Now, as *Behring* only coaſted at a diſtance, he could not poſſibly know whether it was a continent, or great iſland; the laſt of which ſeems the moſt probable: however, a few months now, if our ſhips return ſafe, will give us a certainty on one ſide or the other; altho' I am ſanguine enough to believe they have by this time ſailed thro' and diſcovered this ſo much wiſhed for paſſage.

Theſe, Sir, are the reaſons I have ſtill to expect ſucceſs in the attempt I have promoted; and, if you think it may give any ſatisfaction to Prof. *Euler*, to know the reaſons that ſupport my belief of a practicable ſafe paſſage, be pleaſed to communicate it to him, with my compliments for the trouble I have given him by you, and accept of my beſt acknowledgments for your favours.

IV. It

IV. It is now ſome time ſince I received from M. *de L'iſle* part of a map of the world, found among the papers of the late Dr *Kæmpfer*. In this map were ſeveral *Chineſe* characters, ſome well, ſome ill written, which the late Prof. *Bayer* had attempted to decypher. —— In my anſwer to M. *de L'iſle*, I informed him that it was by no means a *Chineſe* work * ; that it could be of no ſervice to a learned *European*, ſuch as he or you were ; and that Mr *Bayer*'s explanations were full of faults. I ſuppoſe that M. *de L'iſle* has already writ you my thoughts concerning it from *Peterſbourg*. You have poſſibly ſeen in ſeveral books, what the *Chineſe* know, and have ſet down, concerning foreign countries : and there is no monument extant to prove, that before the arrival of the Jeſuits in this country, they had charts or maps of the world, any way reſembling that, which you found among *Kæmpfer*'s writings.

A letter from F. Anth. Gaubil, *Jeſ. to Dr* Mortimer, *Secr R. S. containing ſome account of the knowledge of Geography among the* Chineſe. *Tranſlated from the* French *by* T. S. M. D. *and F. R. S.* N°. 494 p 327. Jan. &c. 1750. *dated* Peking *Nov.* 9. 1748. *Read* Feb. 1. 1749.

It is now above 1600 years ſince they tolerably well knew the northern and eaſtern countries of *India*, and thoſe which lie between *China* and the *Caſpian* ſea. On theſe different countries their hiſtory affords ſeveral informations, which are not to be found in the *Greek*, *Latin*, or other hiſtorians. They had ſome, but very confuſed, notions of the regions beyond the *Caſpian* ſea ; ſuch as *Syria*, *Greece*, *Egypt*, and ſome parts of *Europe*. I do not ſpeak of the times of *Gentchiſkan* and his ſucceſſors ; for then the *Chineſe* were made acquainted with *Ruſſia*, *Poland*, *Germany*, *Hungary*, *Greece*, *&c.* from accounts given by their own contrymen who followed that prince, his ſons, and grandſons: but the monuments that remain of this their knowledge are very confuſed. As to the countries to the eaſt of *China*, there are proofs remaining in books, that, above 1700 years ago, the *Chineſe* were well acquainted with the eaſtern part of *Tartary* as far as the ſea, and the river *Ameur*, *Corea*, and *Japan*. Their books ſpeak alſo in general, and without ſufficiently entering into particulars of many countries to the E. and to the N. of *Japan*. With regard to the monuments of the *Cape of Good Hope*, which have been mentioned by ſome, there are none in *China* ; and if there have been any, they are now loſt. It was from the *Europeans*, that the *Chineſe* have learnt the name and the ſituation of the *Cape* [and you will ſoon ſee a Diſſertation, wherein all this affair will be circumſtantially treated].

V. My curioſity having lately led me to peruſe ſeveral books on the art of Navigation, I was ſomewhat ſuprized not to find in any one of them a clear explanation of that moſt curious paper † written by that excellent Mathematician Dr *Halley* ; who, not intending to write for beginners, as himſelf confeſſes, has drawn his concluſions in a manner, that ſeems to ſtand in need of an explanation, for the generality of readers : and as the maritime people are not the beſt acquainted with mathematical knowledge,

A letter from Mr John Robertſon *to the* Preſ. *containing an explanation of the late Dr* Halley's *demonſtration of the Analogy of the* Logarithmic Tangents *to the* meridian line, *or ſum of the* Secants.

* Doubtleſs it is the work of an *European*, who was giving ſome notion of Geography to a *Chineſe* or *Japaneſe* ; or perhaps that of a *Chineſe* or *Japaneſe* from memory of what he had heard from *Europeans*, or of the map which he might have ſeen with them.

† See Vol. I. Chap. vii. §. 38.

it

N°. 496 p. 559 Nov. &c. 1750. *Read* Nov. 22. 1750.

it might have been expected, that ſuch of the writers on Navigation within the laſt 50 years who have undertaken to demonſtrate the ſeveral parts of their ſubject, would have removed the difficulties in the Doctor's paper, inſtead of leaving them in the ſame ſtate in which they firſt appeared.

Dr *Halley*, in this tract, ſeems to have had two chief points in view; firſt, to prove that *the diviſions of the meridian line in a Mercator's chart, were analogous to the logarithmic Tangents of the half-complements of the latitudes.* And ſecondly, *To find a rule by which the tables of meridional parts might be computed from* Briggs's, *or the common logarithmic Tangents.* The former of theſe the Doctor has clearly and elegantly proved: but he has given rather too few ſteps to ſhew us clearly the inveſtigation of the latter.

Indeed in many of the treatiſes on Fluxions, it is ſhewn how to inveſtigate a rule to find the meridional parts to any latitude: but, to underſtand thoſe methods, requires ſome ſkill in algebraical and fluxionary computations; neither of which are neceſſary in this buſineſs, by keeping to the Doctor's principles, as will be evident from the following articles; ſome of which are already well known; yet it was thought convenient to annex them to this diſcourſe.

Article I. *If the circumference of a circle be divided into any number of equal parts by as many* radii, *and a line be drawn from the circumference cutting thoſe* radii, *ſo that their parts intercepted between this line and the centre be in a continued decreaſing geometric progreſſion; then will that interſecting line be a curve, called the proportional ſpiral, and will interſect thoſe* radii *at equal angles.*

This will be evident, by ſuppoſing the *radii* ſo near to one another, that the intercepted parts of the ſpiral may be taken as right lines: for then there will be a ſeries of ſimilar triangles, each having an equal angle at the centre, and the ſides about thoſe angles proportional.

Article II. *The ſame things ſtill ſuppoſed, the parts of the circumference of the circle, reckoned from any one point, may be taken as the logarithms of the* ratio's *between the correſponding rays of the ſpiral.*

For thoſe rays are a ſeries of terms in a continued geometric progreſſion; and the parts of the circumference from a ſeries of terms in arithmetic progreſſion. Now the terms of the arithmetic ſeries being taken as the exponents of the correſponding terms in the geometric ſeries, there will be the ſame relation between each geometric term and it's correlative, as between numbers and their logarithms. And hence the proportional ſpiral is alſo called the logarithmic ſpiral.

Article III. *That proportional ſpiral, which interſects it's* radii *at angles of* 45° *produces logarithms that are of* Napier's *kind.*

For,

For, if the difference between the firſt and ſecond terms in the geometric ſeries was indefinitely ſmall, and the firſt diviſion of the circumference was of the ſame magnitude; then may that part of the ſpiral, intercepted between the firſt and ſecond *radii*, be taken as the diagonal of a ſquare, two of whoſe ſides are parts of thoſe *radii*: therefore the ſpiral which cuts it's rays at angles of 45°, has a kind of logarithms belonging to it, ſo related to their correſponding numbers, that the ſmalleſt variation between the firſt and ſecond terms in the geometric ſeries, is equal to the logarithm of the ſecond term, a cypher being taken for the logarithm of the firſt. But of this kind are the hyperbolical logarithms, or thoſe firſt made by their inventor the Lord *Napier*: conſequently the logarithms to that ſpiral which cuts it's rays at angles of 45°, are of the *Napierian* kind.

The Rhumb-lines on the globe are analogous to the logarithmic ſpiral. Article IV.

For every oblique rhumb cuts the meridian at equal angles: and it is a property in ſtereographic projections, that the lines therein interſecting one another, form angles equal to thoſe which they repreſent on the ſphere. Therefore a projection of the ſphere being made on the plane of the equator, the meridians will become the *radii* of the equator, and the rhumbs interſecting them at equal angles, will become the proportional ſpiral.

Hence, the arcs of the equator, or the differences of long. reckoned from the ſame merid. are as the logarithms of thoſe parts of the correſponding meridians, intercepted between the centre and rhumb-line.

A Sea-Chart being conſtructed, wherein the meridians are parallel to one another, and the lengths of the degrees of latitude increaſe in the ſame proportion as the meridional diſtances decreaſe on the globes, will conſtitute a Mercator's chart; wherein, beſides the poſitions of places having the ſame proportions to one another as on the globes, the rhumb lines will be repreſented by right lines. Article V.

For none but right lines can cut at equal angles ſeveral parallel right lines.

The diviſions of the meridian line on a Mercator's chart, are the ſame as a table of the differences of long. anſwering to each minute, or ſmall difference of lat. on the rhumb line making angles of 45° with the meridians. Article VI.

For, in ſuch a chart, the parallels of lat. are equal to the equator, and are at right angles to the meridians: and therefore a rhumb of 45° cuts the meridians and parallels of latitudes at equal angles; conſequently between the interſection of any meridian and parallel, and a rhumb cutting them at 45°, there muſt be equal parts of the meridian and parallel intercepted:

tercepted: now, on the equator, or parallels of lat. are reckoned all the ſucceſſive differences of longitudes, and on the meridians the ſucceſſive meridional differences of latitudes, or the diviſions of the nautical merid.: therefore on the rhumb of 45°, the ſucceſſive differences of long. are equal to the correſponding diviſions of the nautical merid.

Article VII. *The tangents of the angles which different rhumbs make with the meridians, are directly proportional to the differences of longitudes made on thoſe rhumbs, when the meridional differences of latitudes are equal; or, are reciprocally proportional to unequal meridional differences of latitudes on thoſe rhumbs, when the differences of longitudes are equal.*

For the meridional difference of lat. is to the diff. of long.; as *radius* is to the tangent of the angle of the courſe, or of the angle which the rhumb makes with the merid. therefore, when the meridional differences of latitudes are equal, the differences of longitudes are as the tangents of the courſes: but, when the differences of longitudes are equal, the meridional differences of latitudes are reciprocally as the tangents of the courſes.

Article VIII. *The logarithmic tangents of the half-complements of the latitudes, are analogous to the lengthened degrees in the nautical merid. line, in a Mercators's chart.*

For, in the ſtereographic projection of the ſphere on the plane of the equator, the latitudes of places are projected by the half-tangents of the complements of thoſe latitudes, which half-tangents are the rays of a proportional ſpiral: now, if a ſeries of ſucceſſive latitudes be taken on any rhumb, the correſponding differences of longitudes will be logarithms to the rays of the ſpiral, or to the tangents of the half-complements of thoſe latitudes: therefore the differences of longitudes are as the logarithmic tangents of the half-complements of the latitudes: but (*Art.* VI.) the lengthened degrees on the nautical merid. are as the differences of longitudes on the rhumb of 45°; conſequently the logarithmic tangents of the half-complements of latitudes are as the lengthened degrees on the nautical merid.

Corol. 1. When the angle between the rhumb line and the merid. is equal to 45°, then the longitudes of places on that rhumb are expreſſed by logarithms of *Napier*'s kind; whoſe correſponding numbers are natural tangents of the half-complements of the latitudes to arcs expreſſed in parts of the *radius*.

Corol. 2. Hence, to any two places on a rhumb of 45°, the difference of long. or the meridional diff. of lat. is equal to the diff. of the *Napierian* logarithmic tangents of the half-complements of the latitudes of thoſe places, eſtimated in parts of the *radius*.

As

As there may be an indefinite variety of rhumbs, and therefore as many different kinds of logarithms, conſequently every ſpecies of logarithms has it's peculiar rhumb, diſtinguiſhable by the angle it makes with the merid.: therefore, among theſe there are two kinds, whereto the differences of longitudes are the differences of the logarithmic tangents of the half-complements of latitudes, eſtimated in minutes of a degree; one of them belonging to *Napier*'s form of logarithmic tangents, and the other to *Briggs*'s, or the common logarithmic tangents. *Corol.* 3.

The common logarithmic tangents are a table of the differences of longitudes, to every minute of lat. on the rhumb line making angles with the meridians of $51^\circ\ 38'\ 9''$. Article IX.

For, let z repreſent the merid. diff. of lat. between two places on the rhumb of 45°; or it's equal, the difference between the logarithmic tangents of the half-complements of the latitudes of thoſe places, eſtimated either in parts of the *radius*, or in minutes of a degree. Then,

As the circumference in parts of the *radius* = 62831,853, *&c.*
To the circumference in minutes of a degree = 21600.
So is a meridional diff. of lat. in parts of the *radius* = z.
To a merid. diff. of lat. in minutes of a degree, = 0,34377468, *&c.* $\times z$.

Whoſe correſponding rhumb is different from that which z belonged to; and the angle which this rhumb makes with the merid. will be found by the following analogy from *art.* 7.

As the meridional diff. of lat. on one rhumb = 0,34377468, *&c.* z.
To the merid. diff. of lat. on a rhumb of 45°, = z.
So is the natural tangent of the rhumb of 45°, = 10000.
To the natural tangent of the other rhumb, = 29088,821, *&c.*

Which tangent anſwers to $71^\circ\ 1'\ 42''$; and this is the angle that the rhumb line makes with the meridians, on which the differences of the logarithmic tangents of the half-complements of the latitudes, in *Napier*'s form, are the true differences of longitudes eſtimated in ſexageſimal parts of a degree.

Now *Napier*'s logarithms being to *Briggs*'s as 2,30258, *&c.* is to 1:

Therefore, 2,30258, *&c.* : 1 :: 29088,821, *&c.* : 12633,114, *&c.*; which is the tangent of $51^\circ\ 38'\ 9''$; and in this angle are the meridians interſected by that rhumb, on which the differences of *Briggs*'s logarithmic tangents of the half-complements of the lat. are the true differences of longitudes correſponding to thoſe latitudes.

The diff. between Briggs's *logarithmic tangents of the half-complements of the latitudes of any two places, to the merid. diff. of lat. in minutes between thoſe places, is in the conſtant ratio of* 1263,3, *&c.* to 1; or of 1 to 0,0007915704, *&c.* Article X.

For *Briggs*'s logarithmic tangents are as the differences of longitudes on the rhumb (*A*) of 51° 38′ 9″; whoſe natural tangent is 1263,3, *&c.*

The nautical meridian is a ſcale of longitudes on the rhumb (*B*) of 45° by *Art.* VI. whoſe tangent being equal to the *radius*, may be expreſſed by unity. And the differences of long. to equal differences of latitudes on different rhumbs, being to each other as the tangents of the angles thoſe rhumbs make with the meridians. Therefore,

As the tangent of *A* (51° 38′ 9″) = 1,2633, *&c.*
To the tangent of *B* (45°) = 1,0000;
So is the difference of long. on *A*, or the diff. between the logarithmic tangents of the half co-latitudes of two places
To the diff. of longitudes on *B*, or the merid. difference of latitudes of thoſe places.

And hence ariſe the rules which are given in nautical works, for finding the meridional parts by a table of common logarithmic tangents.

This curious diſcovery of Dr *Halley*'s, joined to that excellent thought of his, of delineating the lines, ſhewing the variation of the compaſs on the nautical chart, are ſome of the very few uſeful additions made to the art of navigation within the laſt 150 years: for if beſide theſe, we except the labours of that ingenions artiſt Mr *Richard Norwood*, who improved the art by adding to it the manner of ſailing in a current, and by finding the meaſure of a degree on a great cicle, the Theory of Navigation will be found nearly in the ſame ſtate in which it was left by that eminent mathematician Mr *Edward Wright*; who, about the year 1600, publiſhed the principles on which the true nautical art is founded; and ſhewed, what does not appear to have been known before, how to eſtimate a ſhip's true place at ſea, as well in long. as in lat. by the uſe of a table of meridional parts, firſt made by himſelf, and conſtructed by the conſtant addition of the ſecants, and which differs almoſt inſenſibly from ſuch a table made on Dr *Halley*'s principles, contained in the preceding articles.

I ſhall conclude this diſcourſe with an article, which altho' it be ſomewhat foreign to the preceding ſubject, yet, as it was diſcover'd while I was contemplating ſome part thereof, and perhaps is not exhibited in the ſame view by others, it may not be improper to annex it in this place: which is to demonſtrate this common logarithmic property, that *the fluxion of a number divided by that number, is equal to the fluxion of the* Napierian *logarithm of that number.*

Fig. 59. Let *BEG* be a logarithmic ſpiral, cutting it's rays at angles of 45°: then, if *AE* be taken as a number, *BC* will be it's *Napierian* or hyperbolic logarithm.

Alſo, let *CD* expreſs the fluxion of the logarithm *BC*; and the correſponding fluxion of the number *AE*, will be repreſented by *FG*, or it's equal *FE*; as the angles *FEG* and *FGE* are equal.

Now, $AC : CD :: AE : (EF =)\ FG.$

Therefore $CD = \frac{FG}{AE} \times AB.$

And

Fig. 58.

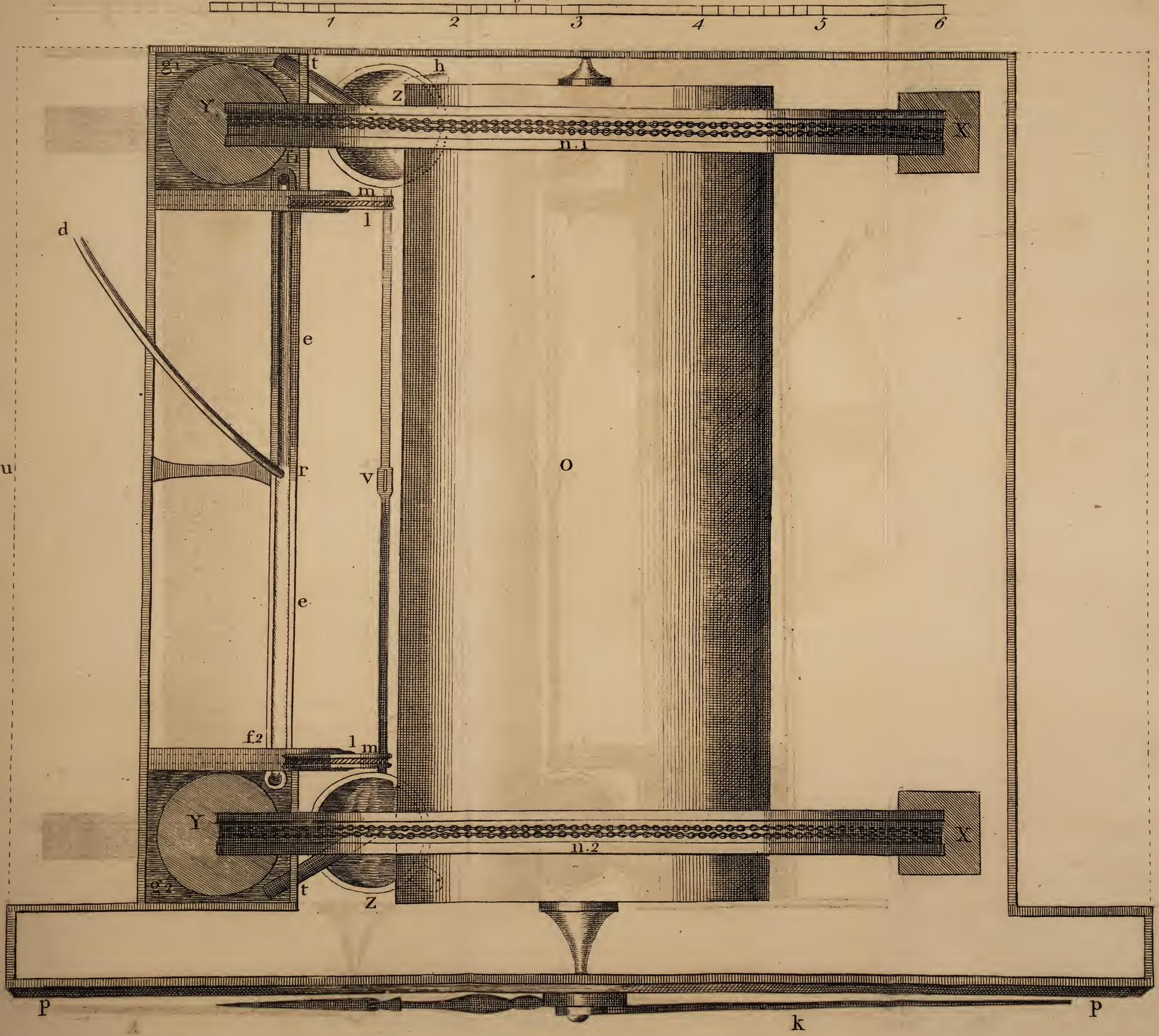


Fig. 60.

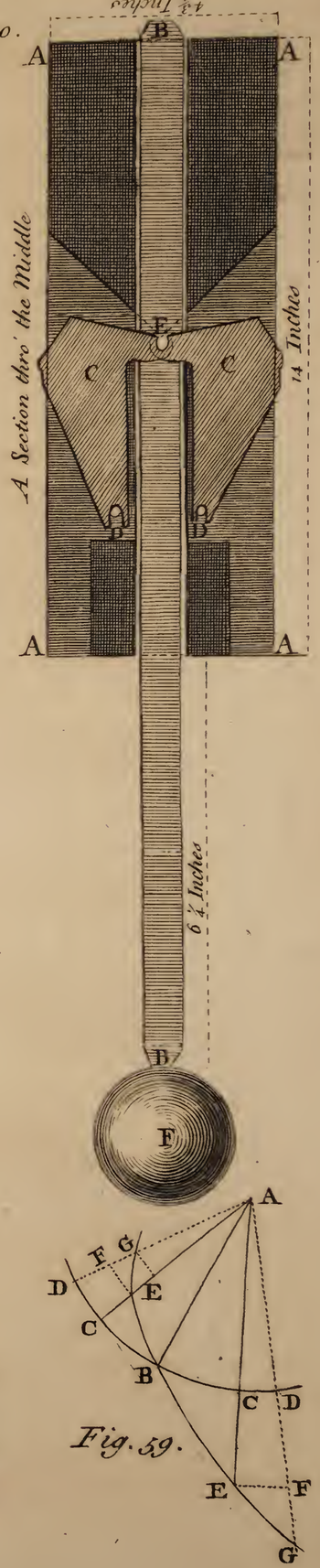


Fig. 59.

And if AB be taken as the unit or term from whence the numbers begin:

Then $CD = \frac{FG}{AE}$. *Q. E. D.*

VI. $AAAA$ repreſent a trunk of timber, with a ſquare hollow, through the centre of which paſſes the ſquare piece of timber BB. A groove on each ſide, in which are placed the two pieces of iron CC; the foot of each reſting on the pins DD, that paſs through the trunk; the upper part of the irons are hooked to an iron pin at E, which paſſes through the ſquare piece BB; which piece is hollowed between H and H, for the hooks of the irons CC to paſs up and down.

When the weight F touches the ground, the two irons CC ſink the trunk to G, which unhooks them at E; whereupon they fall off, and leave the trunk at liberty to float or riſe up again to the ſurface.

A machine of theſe dimenſions, loaded with an iron ball, F, of 12 pounds weight, being let down in water 100 fathoms deep, will go down to the bottom, and the trunk will return in $1' 3''$.

A machine for ſounding the Sea at any depth, or in any part, invented by Maſter Wm. Cook *in the year* 1738, *in a voyage to* Georgia. N°. 479 p. 146. Mar. *and* April, 1746. *Preſented* April 10. 1746. Fig. 60.

CHAP. VII.

MUSIC.

I. IN compliance with your requeſt, I here ſend you ſome of my thoughts on the various *genera* and *ſpecies* of the *Greek* Muſic. What they were, and how far the doctrine of the Ancients in this reſpect is reconcileable with the true nature of muſical ſounds, are, you know, queſtions which have not a little perplexed the learned.

That muſical intervals are founded on certain *ratio's* or proportions expreſſible in numbers, is an old diſcovery. Nobody is better acquainted with theſe proportions than yourſelf; and I am not a little obliged to you for the light you have herein given me. It is well known, that all muſical *ratio's* may be analyſed into the prime numbers 2, 3, and 5; and that all intervals may be found from the octave, fifth and third *major*; which reſpectively correſpond to thoſe numbers. Theſe are the Muſicians elements, from the various combinations of which all the agreeable variety of relations of ſounds reſult. This ſyſtem is ſo well founded on experience, that we may look upon it as the ſtandard of truth. Every interval that occurs in Muſic is good or bad, as it approaches to, or deviates from, what it ought to be on theſe principles. The doctrine of ſome of the Ancients ſeems different. Whoever looks into the numbers given us by *Ptolemy*, will not only find the primes 2, 3, and 5, but 7, 11, *&c.* introduced. Nay he

Of the various Genera *and* Species *of* Muſic *among the* Ancients, *with ſome* obſervations *concerning their* ſcale; *in a letter from* John Chriſtopher Pepuſch, *Muſic. D. & F. R. S. to Mr* Abr. de Moivre, *F. R. S.* N°. 481. p. 266. Oct. *&c.* 1746. *Read* Nov. 13. 1746. *here printed with alterations.*

he ſeems to think all fourths good, provided their component intervals may be expreſſed by ſuperparticular *ratio's*. But theſe are juſtly exploded conceits; and it ſeems not improbable, that the contradictions of different numerical hypotheſes, even in the age of *Ariſtoxenus*, and their inconſiſtency with experience, might lead him to reject numbers altogether. It is pity he did: had he made a proper uſe of them, we ſhould have had a clearer inſight into the Muſic of his times. However, what remains of the writings of this great Muſician, joined to my own obſervation and experience, has enabled me, I hope, to throw ſome light upon the obſcure ſubject of the ancient ſpecies of Muſic.

By the manner in which *Euclid* and others find the notes of their ſcale, it muſt have been compoſed of tones *major*, and *limma's*. Hence the ſeven intervals of one octave would be thus expreſſed in numbers, $\frac{9}{8}$, $\frac{256}{243}$, $\frac{9}{8}$, $\frac{9}{8}$, $\frac{256}{243}$, $\frac{9}{8}$, $\frac{9}{8}$.

Some modern authors have from this inferred the imperfection of the *Greek* Muſic. They alledge we here find the *ditonus*, or an interval equal to two tones *major*, expreſſed by $\frac{81}{64}$, inſtead of the true third *major* expreſſed by $\frac{5}{4}$. As there can be no queſtion of the beauty and elegance of the latter, the former therefore muſt be out of tune, and out of tune by a whole comma, which is very ſhocking to the ear. In like manner the trihemitone of the Ancients falls ſhort of the third *minor* by a comma; which is alſo the deficiency of their hemitone or *limma*, from the true ſemitone *major*, ſo eſſential to good melody. Theſe errors would make their ſcale appear much out of tune to us. This I readily grant; and add, that it appeared out of tune to them; ſince they expreſsly tell us, that the intervals leſs than the *diateſſaron* or fourth, as alſo the intervals between the fifth and octave, were diſſonant and diſagreeable to the ear. Their ſcale, which has been called by ſome the *ſcala maxima*, was not intended to form the voice to ſing accurately, but was deſigned to repreſent the ſyſtem of their modes and tones, and to give the true fourths and fifths of every key a compoſer might chooſe. Now if, inſtead of tones *major* and *limma's*, we take the tones *major* and *minor*, with the ſemitone *major*, as the moderns contend we ſhould, we ſhall have a good ſcale indeed, but a ſcale adapted only to the concinnous conſtitution of one key; and whenever we proceed from that into another, we find ſome fourth or fifth erroneous by a comma. This the Ancients did not admit of. If, to diminiſh ſuch errors, we introduce a temperature, we ſhall have nothing in tune but the octave. We ſee then the ſcale of the Ancients was not deſtitute of reaſon; and that no good argument againſt the accuracy of their practice can from thence be formed.

It was uſual among the *Greeks* to conſider a deſcending as well as an aſcending ſcale; the former proceding from acute to grave, preciſely by the ſame intervals as the latter did from grave to acute. The firſt ſound in each was the *Proſlambanomenos*. The not diſtinguiſhing theſe two ſcales has led ſeveral learned Moderns to ſuppoſe, that the *Greeks*, in ſome centuries, took the *Proſlambanomenos* to be the loweſt note in their ſyſtem; and,

and, in other centuries, to be the higheſt. But the truth of the matter is, that the *Proſlambanomenos* was the loweſt, or higheſt note, according as they conſidered the aſcending, or deſcending ſcale. The diſtinction of theſe is conducive to the variety and perfection of melody; but I never yet met with above one piece of Muſic, where the compoſer appeared to have any intelligence of this kind. The compoſition is about 150, or more, years old, for four voices; and the words are, *Vobis datum eſt noſcere myſterium regni Dei, cæteris autem in parabolis; ut videntes non videant, & audientes non intelligant.* By the choice of the words, the author ſeems to allude to his having performed ſomething not commonly underſtood.

I ſhall here give you an octave only of the aſcending and deſcending ſcales of the diatonic *genus* of the Ancients, with the names for their ſeveral ſounds, as alſo the correſponding modern letters.

Aſcending.		Deſcending.
A	Proſlambanomenos	*g*
$\frac{9}{8}$		$\frac{8}{9}$
B	Hypate Hypaton	*f*
$\frac{256}{243}$		$\frac{243}{256}$
C	Parhypate Hypaton	*e*
$\frac{9}{8}$		$\frac{8}{9}$
D	Lychanos Hypaton	*d*
$\frac{9}{8}$		$\frac{8}{9}$
E	Hypate Meſon	*c*
$\frac{256}{243}$		$\frac{243}{256}$
F	Parhypate Meſon	*b*
$\frac{9}{8}$		$\frac{8}{9}$
G	Lychanos Meſon	*a*
$\frac{9}{8}$		$\frac{8}{9}$
a	Meſe	*G*

Where you ſee the ſame *Greek* names ſerve for the ſounds in the aſcending and deſcending ſcales.

In the octave here given, four ſounds, *viz.* the *Proſlambanomenos*, *Hypate Hypaton*, *Hypate Meſon*, and *Meſe*, were called *ſtabiles*, from their remaining fixed throughout all the *Genera* and *Species*.

The other four ſounds being the *Parhypate Hypaton*, *Lychanos Hypaton* *Parhypate Meſon*, and the *Lychanos Meſon*, were called *mobiles*, becauſe they varied according to the different ſpecies and varieties of Muſic.

I come now to determine the queſtion, what theſe different *genera* and *ſpecies* were. You know, that by *genus* and *ſpecies* was underſtood a diviſion of the *diateſſaron*, containing 4 ſounds, into 3 intervals. The *Greeks* conſtituted 3 *genera*, known by the names of *enharmonic*, *chromatic*, and *diatonic*. The *chromatic* was ſubdivided into 3 ſpecies, and the *diatonic* into 2. The 3 *chromatic* ſpecies were the *chromaticum molle*, the *ſeſquialterum*,

ſeſquialterum, and the *toniæum*. The 2 *diatonic* ſpecies were the *diatonicum molle*, and the *intenſum*; ſo that they had ſix ſpecies in all. Some of theſe are in uſe among the Moderns, but others are as yet unknown in theory or practice.

I now proceed to define all theſe ſpecies, by determining the intervals, of which they ſeverally conſiſted; begining by the *diatonicum intenſum*, as the moſt eaſy and familiar.

The *diatonicum intenſum* was compoſed of two tones, and a ſemitone: but, to ſpeak exactly, it conſiſts of a ſemitone *major*, a tone *minor*, and a tone *major*. This is in daily practice; and we find it accurately defined by *Didymus* in *Ptolemy*'s *Harmonics* publiſhed by Dr *Wallis*.

The next ſpecies is the *diatonicum molle*, as yet undiſcovered, as far as appears to me, by any modern author. It's component intervals are, the ſemitone *major*, an interval compoſed of two ſemitones *minor*, and the complement of theſe two to the fourth, being an interval equal to a tone *major*, and an *enharmonic dieſis*.

The third ſpecies is the *chromaticum toniæum*, it's component intervals are, a ſemitone *major*, ſucceeded by another ſemitone *major*; and, laſtly, the complement of theſe two to the fourth, commonly called a ſuperfluous tone.

The fourth ſpecies is the *chromaticum ſeſquialterum*, which is conſtituted by the progreſſion of a ſemitone *major*, a ſemitone *minor*, and a third *minor*. This is mentioned by *Ptolemy*, as the *chromatic* of *Didymus*. Examples among the Moderns are frequent.

The fifth ſpecies is the *chromaticum molle*. It's intervals are two ſubſequent ſemitones *minor*, and the complements of theſe two to the fourth; that is, an interval compounded of a third *minor*, and an *enharmonic dieſis*. This ſpecies I never met with among the Moderns.

The ſixth and laſt ſpecies is the *enharmonic*. *Salinas* and others have determined this accurately. It's intervals are, the ſemitone *minor*, the *enharmonica*, *dieſis* and the third *major*.

Examples of four of theſe ſpecies may be found in modern practice. But I do not know of any Theoriſt who ever yet determined what the *chromaticum toniæum* of the Ancients was: nor have any of them perceived the analogy between the *chromaticum ſeſquialterum* and our modern *chromatic*. The *enharmonic*, ſo much admired by the Ancients, has been little in uſe among our Muſicians as yet. As to the *diatonicum intenſum* it is too obvious to be miſtaken.

Ariſtoxenus and others often mention the tone as divided into 4 parts, and the ſemitone into 2; thereby making 10 diviſions or *dieſes* in the fourth. And this is true, if we conſider theſe ſounds in one tenſion; that is, either aſcending or deſcending: but, accurately ſpeaking, when we conſider all the *dieſes* or diviſions of the fourth, both aſcending, and deſcending, we ſhall find 13; 5 to each tone, and 3 to the ſemitone *major*. But then it is to be obſerved, that ſome of theſe diviſions will be leſs than the *enharmonic dieſis*: for, if we divide the ſemitone *major* into the ſemi-

tone

tone *minor*, and *enharmonic dieſis*: aſcending, for inſtance, *E*, 𝄪*E*, *F*, and then divide in like manner deſcending, *F*, 𝄫*F*, *E*, we ſhall have the ſemitone *major* divided into three parts thus, *E*, ♭*F*, 𝄪*E*, *F*; where the interval between ♭*F* and 𝄪 *E*, is leſs than the *enharmonic dieſis* between *E* and ♭*F* and between 𝄪 *E* and *F*, is as eaſily proved.

Now, if we ſuppoſe theſe ſmall intervals equal, by increaſing the leaſt diviſion, and diminiſhing the true *enharmonic dieſis*, we ſhall then have a fourth divided into 13 equal parts; and, conſequently, the octave divided into 3 ſuch equal parts; which gives us the celebrated temperature of *Huygens*, the moſt perfect of all.

From this it appears, that the diviſion of the octave into 31 parts, was neceſſarily implied in the doctrine of the Ancients. The firſt of the Moderns who mentioned ſuch a diviſion was Don *Vincentino*, in his book intitled *L'Antica Muſica ridotta alla moderna prattica*, printed at *Rome*, 1555, *folio*. An inſtrument had been made according to his notion; which was condemned by *Zarlino* and *Salinas*, without ſufficient reaſon. But Mr *Huygens*, having more accurately examined the matter, found it to be the beſt temperature that could be contrived. Though neither this great Mathematican, nor *Zarlino*, *Salinas*, nor even Don *Vincentino*, ſeem to have had a diſtinct notion all theſe 31 intervals, nor of their names, nor of their neceſſity to the perfection of Muſic.

I muſt obſerve to you, that I received, ſome time ago, a manuſcript from *Florence*, where a Muſician of that city had rightly named theſe intervals of the octave. I found their names, you know, many years ago.

In *Huygens*'s temperature the tones are all equal: but, in a true and accurate practice of ſinging, they are not ſo. And I muſt add, that the tone divided in every ſpecies muſt be the tone *minor*; for the diviſion of the tone *major* is harſh and inelegant. So that, in the diviſion of the fourth, it is to be obſerved, that in every ſpecies, the tone *major* muſt either be an undivided interval, or make part of one.

You may perhaps wonder how the foregoing doctrine can be found in the writings of the Ancients, ſince the diſtinction of tones into *major* and *minor* is no where mentioned in their writings. But it is to be obſerved, that tho' the terms do not occur, yet the thing itſelf was not unknown to them. I own, they have not expreſſed themſelves fully; yet, from the whole of their writings come to our hands, I think the doctrine before laid down may be well ſupported. But, as it would require ſome time to put this in a juſt light, I muſt defer it to another opportunity.

II. I think the incloſed paper is the effect of great ingenuity and much thought; and as the ſubject-matter of it may tend to give great improvement and pleaſure to many, not only in our own country, but every-where, I hope my preſenting it may not be thought improper, that it may thereby be printed and publiſhed to the world.

A letter from Mr John Freke, *F. R. S. Surgeon to St* Bartholomew's *Hoſpital, to the* Preſ. *incloſing*

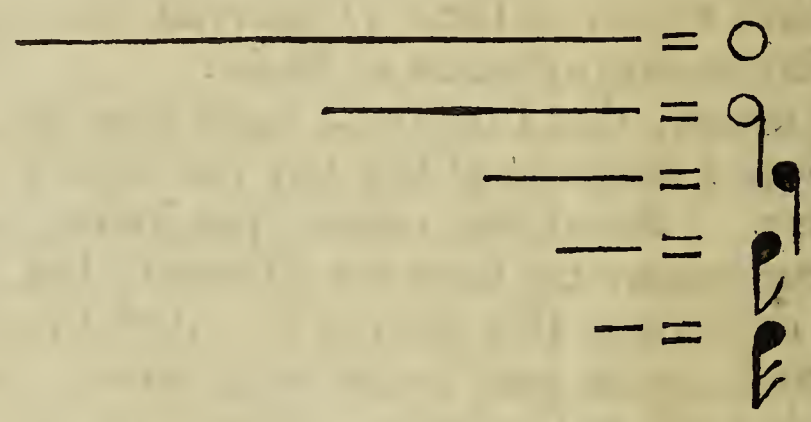
a paper of the late Rev Mr Creed, *concerning a* machine *to write down* extempore *Voluntaries, or other pieces of* Muſic. N°. 483. p. 445. Mar. *&c.* 1747. *Read* Mar. 12. 1746-7.

It was invented and written by Mr *Creed*, a Clergyman, who was eſteemed, by thoſe who knew him, to be a man well acquainted with all kinds of mathematical knowledge. And was ſent me by a gentleman of very diſtinguiſhed merit and worth.

A demonſtration of the poſſibility of making a machine *that ſhall write* extempore Voluntaries, *or other pieces of* Muſic, *as faſt as any maſter ſhall be able to play them upon an* organ, harpſicord, *&c. and that in a character more natural and intelligible, and more expreſſive of all the varieties thoſe inſtruments are capable of exhibiting, than the character now in uſe.*

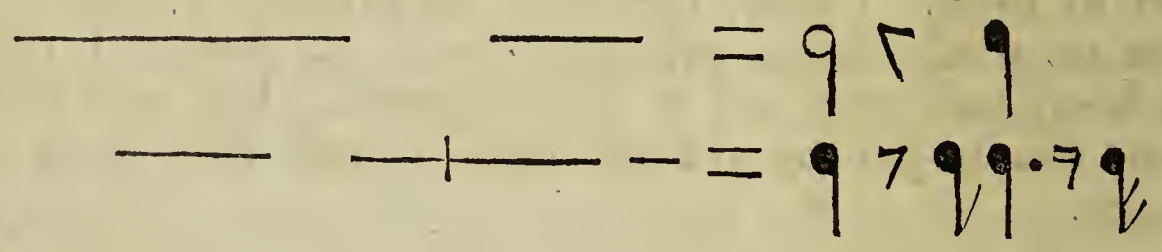
Maxim I. All the varieties thoſe inſtruments afford fall under theſe 3 heads: *Firſt*, The various durations of ſounds, commonly called *minims*, *crotchets*, &c. *Secondly*, The various durations of ſilence, commonly called *reſts*. *Thirdly*, The various degrees of acuteneſs or gravity in muſical ſounds, as *A re*, *B mi*, &c.

Maxim II. Strait lines, whoſe lengths are geometrically proportion'd to the various durations of muſical ſounds, will naturally and intelligibly reqreſent thoſe durations. *Ex. gr.*



The firſt (being 2 inches) repreſents a *ſemibreve*.
The ſecond is 1 inch, and denotes a *minim*.
The third is half an inch, and ſignifies a *crotchet*.
The fourth is a quarter, and anſwers to a *quaver*.
The fifth is an eighth, and ſtands for a *ſemiquaver*.

Maxim III. The quantity of the blank intervals, or diſcontinuity of the lines, will exactly repreſent the duration of ſilence or reſts. *Ex. gr.*



Maxim IV. The different degrees of Muſical ſounds, as *Gamut*, *A re*, *B mi*, &c. may be repreſented by the different ſituations of thoſe black lines upon the red ones or faint ones.

Fig. 61.

Problem. *To make a machine to write Muſic in the aforeſaid character, as faſt as it can be play'd upon the organ or harpſicord, to which the machine is fixed.*

That

That a cylinder may be made by the application of a circulating, not a vibrating, *pendulum*, to move equally upon it's *axis* the quantity of 1 inch in a ſecond of time, which is about the duration of a *minim* in *allegro's*; *Poſtulatum.*

Suppoſe the cylinder *a* to be ſuch, and to move under the keys of an organ, as *b*, *c*, *d*, and nail points under the heads of the keys, it is manifeſt, that if an organiſt play a *minim* upon *c*, that is, if he preſs down *c* for the ſpace of a ſecond, the nail will make a ſcratch upon the cylinder of 1 inch in length, which is my mark for a *minim*. Fig 62.

Again, if he reſt a *crotchet*, that is, if he ceaſe playing for the ſpace of half a ſecond, the cylinder will have moved under the nails half an inch without any ſcratch; but if the organiſt next preſſeth down *d* for the ſpace of half a ſecond, the nail under *d* will make a ſcratch upon the cylinder half an inch long, which is my mark for a *crotchet*. It will likewiſe be differently ſituated from the ſcratch that was made by *c*, and conſequently diſtinguiſhed from it as much as the notes now in uſe are from one another by their different ſituation in the lines. (*Vide Fig.* 61.)

Theſe three inſtances include all that can be performed upon an organ, *&c.* (Maxim I.)

Therefore it is already demonſtrated, that whatever is play'd upon the organ during one revolution of the cylinder *a* (*Fig.* 62.) will be inſcribed upon it in intelligible characters. —— I proceed to ſhew how this operation may be continued for a long time.

In *Fig.* 63. *aa*, *b*, *c*, *d*, are the ſame as in *Fig.* 62. Let *x* be a long ſcroll of paper wound upon ſuch a cylinder as *z*. Let *e e e e* be the ſame ſcroll brought over the cylinder *a a*, to be wound upon the cylinder *y y*, as faſt as the motion of *a a* (which is determined by a *pendulum*) will permit. Fig. 63.

It is manifeſt, that whatever is play'd upon the organ during the winding up of *y y* will be written on the ſcroll by the pencils *b*, *c*, *d*, *&c.*

All the graces in Muſic being only a ſwift ſucceſſion of ſounds of minute duration, will be expreſſed by the pencils by ſmall hatches geometrically proportion'd to thoſe durations. *Ex. gr.*

A ſingle *Beat*

A double *Beat*

A *Shake*

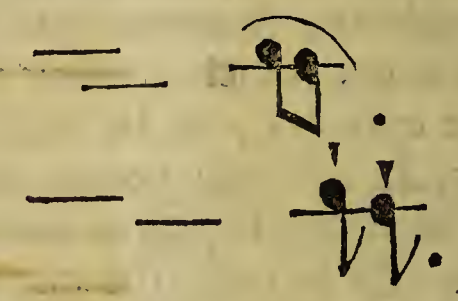
A *Turn*

A ſingle *Backfall*

A double *Backfall*

A *Shake* and *Turn*

If a line commence exactly over or under the termination of another, it is an indication of a *ſlur*; as



So a ſmall interval indicates the contrary; as

Flat or ſharp notes are implied by their ſituation on the red lines; the natural notes being always drawn between them, *viz.* in the ſpaces. (*Vide* Fig 61)

The ſcroll may be prepared before-hand with red lines to fall under their reſpective pencils. It is the ſureſt way to rule them after; though it is feaſible or poſſible to contrive that they may be ruled the ſame inſtant the Muſic is writing.

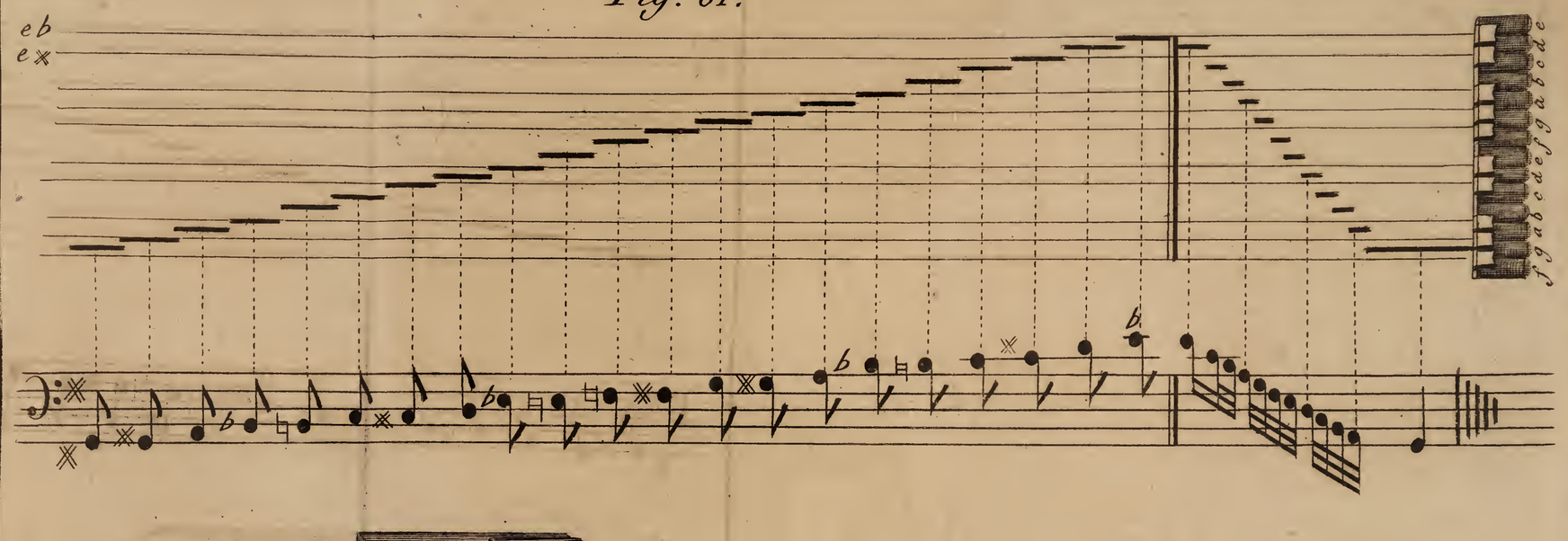
The places of the bars may be noted by two ſupernumerary pencils, with a communication to the hand or foot of a perſon beating time.

Grave Muſic from briſk, ſlow from faſt, *&c.* will be better diſtinguiſhed by this machine, than in the ordinary way by the words *Adagio*, *Allegro*, *Grave*, *Preſto*, &c. for, by theſe words, we only know in general this muſt be ſlow or faſt, but not to what degree, that being left to the imagination of the performer; but here I know exactly how many notes muſt be play'd in a ſecond of time; *viz.* as many as are contain'd in 1 inch of the ſcroll *per poſtulatum*.

Laſtly, whereas, in the ordinary way of writing Muſic, you have either no character for graces, or ſuch as do not denote the time and manner of their performance, here you have the the minuteſt particles of ſound that compoſe the moſt tranſient graces mathematically delineated.

N. B. Though, to facilitate the demonſtration, I ſuppoſe the pencils to be fixed under the heads of the keys, and conſequently to require a very broad ſcroll to paſs under them; yet I intend the pencils a more commodious ſituation, *viz.* the motion of the keys to be communicated by ſmall rods to them (which I know better how to do than to deſcribe, the ſcheme would be ſo perplex'd). The pencils are to be made of ſteel, and ranged in cloſe order like the teeth of a ſmall comb, ſo that a very narrow ſcroll will do. I can prepare the paper to receive a very black impreſſion from the pencils at ſo cheap a rate, that, at the expence of 6 *d.* in paper, I can take in writing all the Muſic that the ſwifteſt hand ſhall be able to play in an hour.

Fig. 61.



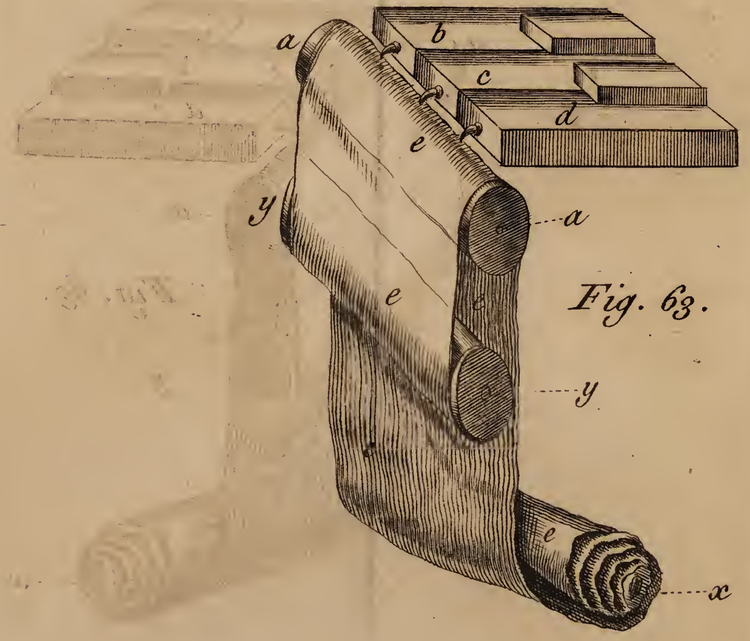
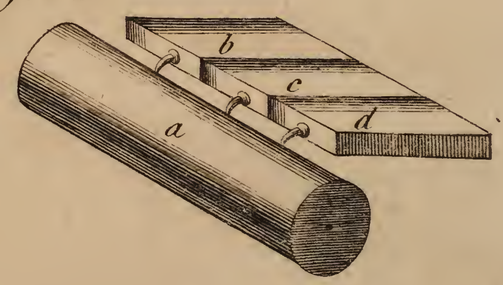
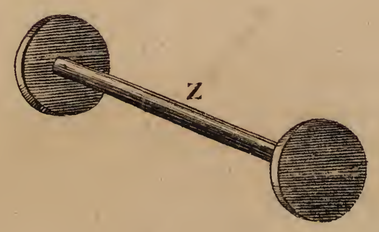


Fig. 63.

Fig. 62.





THE

Philosophical Transactions

ABRIDGED.

PART II.

CONTAINING THE

Physiological PAPERS.

CHAP. I.

PHYSIOLOGY, METEOROLOGY, PNEUMATICKS.

I. 1. THE electrical sparks from metals, such as iron and silver, are capable of kindling all such fluids as may be otherwise kindled by actual flame. And this experiment succeeds best, when the *Quinta essentia vegetabilis* is held in a spoon under the cross of a sword, whose point is turned towards the electrifying glass (Tab. II. *Fig.* 4.) * In like manner, the same spirits may easily be set on fire, by the sparks proceeding from an electrified tube of tin.

This experiment with the sparks coming from metals when made electric, was first made by Dr *Ludolph*, of *Berlin*; who, toward the beginning

Abstract of what is contained in a book concerning Electricity, *just published at* Leipsic, 1744. *by* John Henry Winkler, Greek *and* Latin Professor *there*;

* In the Author's original book.

from Art. 75, *to* Art. 79. N°. 474 p. 166. June, &c. 1744. *Read* Nov. 22. 1744.

beginning of the preſent year 1744, kindled, with the ſparks excited by the friction of a glaſs tube, the *ethereal ſpirits of* Frobenius. This was done at the opening of the *Royal Academy*, and in the preſence of ſome hundreds of perſons. This account was not only related in the *Berlin Gazette*, of the 30th of *May* laſt; but has been ſince confirmed by ſeveral letters, ſent from *Berlin* to *Leipſic*, to Count *Manteuffel*, immediately after the experiment.

Mr *Marſchall*, who now ſtudies here, alſo communicated to me a letter he had received from *Berlin* concerning the ſame; and I have ſince been alſo certified of it, by the account of ſeveral men of learning, that had ſeen the experiment at *Berlin*, and that have ſince viſited me at this place. Laſtly, Mr *Reinhart*, who came hither about laſt *Eaſter*, with Count *Zaluſki*, Great Chancellor of *Poland*, told me, that the experiment was not difficult to be made; and that the liquor, called *Quinta eſſentia vegetabilis* *, might very readily be kindled by the electrical ſparks. I immediately ſent for ſome of that eſſence, and found the experiment ſucceed to my wiſh.

Red-hot iron ſets no ſpirits on fire, tho' held very near to thoſe ſpirits; but if that iron is made electric, its electric ſparks very readily kindle all well-rectified ſpirits.

The ſparks that proceed from the body of a man, made electrical, kindle ſpirits as quick as thoſe from electrified metal, whether the body of the man is rendered electric immediately by the glaſs tube, or by the intermediate tube of tin.

I made this experiment with ſucceſs upon myſelf, before his Excellency Count *Manteuffel*, at his houſe, about the middle of laſt *May*, in the preſence of Profeſſor *Chriſtian Wolf*, of *Hall*, and many others. Neither myſelf, nor any of the company, knew, at that time, that the electric ſparks, from the body of a man, were capable of kindling ſpirits; but, upon ſeeing the *Quinta eſſentia vegetabilis* kindled with extraordinary quickneſs, by the ſparks proceeding from an iron tube that was ruſty, one of the company ſtarted the queſtion, whether the ſparks, from the body of a man, might not poſſibly do the ſame? Upon which I immediately ſtept on to a frame, over which blue ſilken lines were extended: I took hold with one hand of the ruſty iron tube, and held the fingers of the other over ſome of the *Quinta eſſentia*; and the ſparks from my fingers immediately ſtruck with ſuch violence into the ſilver ſpoon that held it, that the eſſence was in a moment ſet all in a flame.

This experiment, ſo unexpected, gave the greateſt ſatisfaction to all the company; and an account of it was publiſhed in the *Leipſic Gazette* of the 21ſt of *May*; where it was alſo mentioned, that divers other experiments, with the ſparks of electrified metal, had already been made both at *Dantzic*, and at *Berlin*.

Dead fowls, pork, and veal, both raw and dreſt, may be made electric by a tin tube, or by the hand of a man; inſomuch that the ſparks, pro-

* *i. e. Spirit* of *Wine* ſo highly rectified, as, being pour'd upon *gunpowder*, and then being ſet on fire, will at laſt flaſh the gunpowder. *C. M.*

ceeding

ceeding from thoſe ſeveral bodies, will alſo kindle the ſame eſſence. If ſuch fluid bodies, as are uſually kindled by flame, are not fine enough, they need only be warm'd a little in the ſpoon: or the ſpirits may be lighted a little before, and blown out again, before they are brought to the electrical body.

In this manner I have kindled, with the electrical ſparks, camphorated ſpirits of wine, coloured with ſaffron, the common *Eſſentia vegetabilis*; and even *French* brandy, and corn-ſpirits, only taking the precaution of warming theſe liquors a little before.

Even oil, pitch, and ſealing-wax, may be lighted by the electric ſparks, provided they are before heated to a degree that is next to kindling.

2. After Mr *Du Fay* had diſcovered by accident, that an electrified human body, if touched by another not electrified, would emit ſparks that pricked pretty ſharply, theſe experiments were repeated in the univerſity of *Leipſic*; and inſtead of the glaſs tube which Mr *Gray* and Mr *Du Fay* uſed, they applied a glaſs ball, ſuch as Mr *Hawkſbee* formerly uſed in his electrical experiments. On this occaſion it was obſerved, that electrified bodies, eſpecially thoſe of animals and metals, emitted a fire ſo ſtrong, that not only ſpirit of wine moderately warmed, which ſucceeds very eaſily, but alſo other inflammable bodies, ſuch as gunpowder, pitch, brimſtone, and ſealing-wax, being firſt well heated, may be ſet on fire. I relate theſe laſt experiments on the credit of another; but the former I can affirm on my own experience whilſt the glaſs ball, thro' which an iron axle paſſes, is turned ſwiftly round, there is put upon it as near as poſſible an iron tube, made of iron plates tinned over, near an inch in diameter, and 3 or 4 feet long; and laid horizontally on lines of blue ſilk: and to keep the tube from doing any hurt to the glaſs ball as it turns round, I put into it's hollow extremity ſome bundles of various ſorts of thread, ſome plain, and others covered with gold or ſilver, the extremities of which whilſt they touch the ball, amongſt other pleaſant *phænomena*, make the force in the iron tube much ſtronger. The other extremity of this tube is held by a man, who ſtands upon a cake of pitch 2 or 3 inches thick, poured into a wooden veſſel: and then the electrical force is ſo diffuſed thro' his whole body; that any part of it will attract and repel alternately leaf-gold and other light bodies, and if any part, either of the iron tube, or of the electrified perſon, is touched by another not electrified, it will emit ſparks, that are extremely pungent. It will often happen alſo, that if the electrified perſon ſtanding on the pitch has a ſword on, ſparks will be emitted from the extremity of the ſheath, even of their own accord.

Of Electrical fire by Sam. Chriſtian Hollman, *Prof. Pub. Ord.* Gotting. *In a letter to Dr* Mortimer, *dated* Gottingen, Oct. 15. 1744. N°. 475. p. 239. Jan. &c. 1745. *Read* Jan. 10. 1744-5.

Let the perſon who ſtands on the pitch hold a gold or ſilver laced hat under his arm, and let another not electrified touch the edging, and he will feel a ſmart ſtroke and pain in his arm. If a perſon not electrified holds highly rectified ſpirit of wine, moderately warmed, in a ſpoon,

ſpoon, and an electrified perſon brings his finger, an iron key, or the point of a ſword near the ſurface of the ſpirit, it will immediately be inflamed. If an electrified perſon holds a ſpoon with ſpirit of wine in his hand, and one of the company puts his finger near it, the ſame effect will follow. If 2, 3, or 4, ſtand upon pitch, and join their hands, or unite by the mediation of a cord, iron tube, &c. the laſt will perform the ſame with the firſt and ſecond.

I do not mention other newly diſcovered *phænomena*, relating to the attraction and repulſion of the electrified body. I ſhall only add, that when the glaſs ball is turned round, a hand muſt be uſed, that is dry, and not too hot; for nothing has yet been found equal to a human hand.

A letter from the Rev. Hen. Miles, *D. D. F. R. S. to Mr* Hen. Baker, *F. R. S. of firing* Phoſphorus *by* Electricity. *Ibid* p. 290 *Read* March 7. 1744-5.

3. It came into my head laſt night, to try whether the *effluvia* of an excited glaſs tube would not kindle *Phoſphorus*; and having been uſing my tube for the ſake of a little exerciſe, I took a ſmall bit of about a $\frac{1}{4}$ of an inch long, which has lain by me theſe ten years; and having nothing at hand convenient for holding it, I roll'd it up in a ſmall piece of white paper; and applying it to the excited tube, it immediately took fire, emitting a conſiderable quantity of flame and ſmoke: after ſome time I quenched it, by dipping it into water, which was ready for that purpoſe; and taking it out again without ſtaying any longer than to be ſatisfied it was not on fire, I applied it as before, when it ſuddenly took fire, as at firſt: this I repeated in the ſame manner for 6 or 7 times with the like effect; tho' the *Phoſphorus* could not be drained of the water, eſpecially as the paper about it was wet.

The room in which I made the trial was not abſolutely dark, having a dull fire (tho' without any candle): the tube I uſe is about 2 feet $\frac{1}{2}$ long, the diameter of the bore nearly one inch, the thickneſs about $\frac{1}{8}$ of an inch, hermetically ſealed at one end (which ſort are, by the way, moſt convenient for rubbing): the *Phoſphorus* was held generally about 5 inches from the tube; but once or twice bringing it nearer, I could perceive a continued ray of light from the tube to the *Phoſphorus*. Some occaſions calling me away in the midſt, I could not be more accurate; but I would not omit to tell you one obſervation I made, upon pretty ſmartly exciting the tube, that the corruſcations of light were larger, more ſubſtantial, and of a more regular form than I had ever obſerved them before, this happened, not when the *Phoſphorus* was applied, but in the intervals. Whether any of the fumes of the *Phoſphorus*, which remained in the room, might contribute hereto, I cannot tell, tho' it is not very likely. Tho' I never made many trials with *Phoſphorus*, yet as I am not inſenſible, that ſome ſolid kinds of it will be inflamed by the mere action of the air upon it, when it is taken out of the water in which it is uſually kept; I was therefore minded to try whether the air would have that effect upon mine, and accordingly took it out of the water, with a *forceps*, and laid it down on a ſhelf, ſo as nothing touch'd it but the inſtrument which held it, but I could not perceive

perceive the leaſt glimmering of light, tho' the place was ſufficiently dark, after it had lain there for the ſpace of half an hour, which I thought long enough to ſatisfy me, that it was not kindled by the action of the air upon it in the above-mentioned experiment.

A repreſents the tube which I held in my right-hand, and excited with my left, having on a glove, which I find more convenient for me in rubbing it. I ſhould obſerve, that my method then was to rub it ſmartly for about half a ſcore times up and down; and then giving it one briſk ſtroke, beginning at the end from me, upon diſcharging my hand quick from the tube, the corruſcations of light appear'd as mark'd α and β, both in ſize and form: ſome allowance may be thought reaſonable to be made for one's judgment in ſuch a caſe, the motion being ſo very ſudden, and the *phænomenon* ſo ſoon diſappearing. But I intend to repeat the experiment whenever the temperature of the air ſhall be favourable, which I don't find it to be this morning. I forgot to mention, that, during this trial, I found the *effluvia* troubleſome to my eyes to a great degree, occaſioning a very ſenſible ſmarting pain, which did not go off for ſome time; tho' I never deſignedly brought the tube near my face. This was the firſt time of uſing this tube. Fig. 22.

4. § 1. Hollow glaſs balls, and veſſels of glaſs, which are rubbed by rotation and application of the hand, excite ſuch an Electricity in metals and perſons near them, that the electrical ſparks, which are emitted on the approach of a body void of Electricity, burſt out in a continual ſtream. *New obſervations on* Electricity; *by* Jo. Hen. Winkler, Gr. *and* Lat. Prof. Pub. Ord. *and Rector of the Univerſity of* Leipſic. *Ibid.* p. 307. *Preſ.* Mar. 21. 1744-5.

§ 2. But if the glaſs tubes and veſſels are rubbed up and down, the ſparks are emitted by intervals.

§ 3. For the more convenient rubbing of the tubes, I have cauſed a machine to be made after the following manner. Four columns are inſerted into a plank *a b c d*. On the tops *g h* of the 2 middle ones *e* and *f* are ſcrewed little planks, the middle part of which is hollowed, ſo as to fit the convexity of the glaſs tube. To theſe little planks others of the ſame kind hollowed in like manner are ſcrewed. One of theſe columns with its little planks is repreſented in *Fig.* 3. where *i k* ſhews the lower plank, *l m* the upper one, and *n o* the ſcrews that faſten them. The cavities of the upper and lower plank are ſo lined with buck-ſkin and hair, as cloſely to embrace the glaſs tube, which is to be drawn backwards and forwards. The extremities of the tube *q q* are armed with braſs caſes, which are cemented to them. *The kinds of* Electricity *excited by attrition.* Fig. 2. Fig. 3. Fig. 2.

To the caſes are annexed rings, to which are faſtened hempen cords, one of which *q r* is drawn thro' a hole of the column *t u*, and the other *q ſ* over a pully *x* faſtened to the column *y z*. Then the glaſs tube, being drawn backwards and forwards by two perſons, abundantly communicates the Electricity excited therein to an iron tube $\alpha\beta$, placed in

nets of ſilk. To the extremity of the iron tube α are tied ſilver threads, which touch the glaſs tube between the two columns *e g* and *f h*.

§ 4. And tho' the ſparks excited by the rotation of a glaſs ball flow continually on the ſurfaces of metals; yet thoſe which ariſe from glaſs veſſels drawn to and fro are more vehemently pungent, provided that theſe veſſels are of the ſame magnitude with the balls, and the glaſs is equally good.

§ 5. The electrical ſparks alſo, which are raiſed on the ſurfaces of metals by the drawing of glaſs tubes, exceed the ſparks excited by the turning round of glaſs veſſels.

§ 6. Glaſs balls rubbed by the hand as they are turned round ſhew more Electricity, than by the application of a leathern cuſhion.

§ 7. In experiments made either by turning the ball, or drawing the tube, there is need of three perſons. But in uſing the Turners wheel there wants only one.

The method of increaſing Electricity.

§ 8. I call that Electricity *ſimple*, which is raiſed by one glaſs veſſel, ball, or tube; *double*, which is raiſed by 2, *triple* by 3, *quadruple* by 4, and ſo on.

§ 9. The Electricity, which I raiſed by the attrition of 2 glaſs balls, of the diameter of $\frac{1}{2}$ a *Paris* foot, was ſo great in water, ſnow, and ice, that the electrical ſparks flying from theſe bodies have ſet fire to pure ſpirit of wine warmed.

In water the experiment is made 2 ways. For either the ſpirit is applied in a ſmall ſpoon, and hanging from an electrified iron tube: or elſe a finger dipped in warm ſpirit of wine is extended over water in a tin veſſel, but at a certain diſtance from the ſurface of the water. To the veſſel, covered with a ſilken net, is added an iron wire, which reaches to the glaſs ball, tube, or veſſel, in the electrical machine. Snow and ice alſo are laid upon the ſilken net in the tin veſſel.

§ 10. To make the Electricity ſtill greater, 2 machines are ſo placed as to have each of them 2 balls, which communicate the Electricity to the ſame iron tube. Over each machine is laid a ſilken net *a b*, to which the iron tube *c d* is joined, which extends near the machine 2 iron arms, *b c*, *e f* and *b d*, *g h*, to which ſilver threads are joined touching the balls in *i k l m*.

Fig. 1.

If inſtead of balls I make uſe of glaſs cups, which as they are turned round are rubbed by cuſhions; I add no ſilver threads to the iron arms, to touch the veſſels. For I have found, that by adding theſe the Electricity is diminiſhed.

The Electricity, *when it returns into the body, from which it firſt proceeded, is diminiſhed.*

§ 11. The machine with the glaſs veſſel, and a man that turns the glaſs veſſel with his foot after the manner of the Turners, reſt upon ſilken nets large enough for the machine and the man to be at a conſiderable diſtance from the wooden ſides, to which the nets are faſtened.

Fig. 5.

§ 12. When the glaſs veſſel as it turns is rubbed by the cuſhion, not only the iron tube placed on the tube and neareſt the veſſel, but alſo the man

manand the machine diſcover a certain Electricity, by which light bodies under a glaſs ball, which another man holds in his hand, are variouſly moved.

§ 13. The ſame happens, if a ball is made uſe of inſtead of the veſſel; and the perſon, who applies his hand as it turns, ſtands with one foot on the machine, and the other on the ſilken net.

§ 14. But when things are thus conſtituted, and the iron tube *a b* is placed on the ſilken net near the glaſs veſſel or ball, another tube *c d* is added, and extended in ſuch a manner as to touch the machine in *e*, the ſparks, which before were excited, ceaſe, and the attracting force is greatly diminiſhed. Fig. 10.

§ 15. The machine, by which Electricity may conveniently be excited *in vacuo*, and propagated thro' a glaſs ball into the air, and communicated to all ſorts of bodies, is repreſented in *Fig.* 6. Electricity *in vacuo*. Fig. 6.

It conſiſts of a glaſs veſſel *a b c d*, to the baſes of which *a c* and *b d* are cemented brazen plates, to one of which *a c* is annexed a wooden arm *e f*. In this wooden arm and another plate *b d* are conical cavities, into which little axles may be put, which being in form of a ſcrew are faſtened into the ſides of the metallic ſupport *g h i k l m*, which being furniſhed with a male ſcrew *m n*, may be inſerted into a female ſcrew in the orb of the pneumatical machine. The male ſcrew paſſes thro' a hole of a bent elaſtic plate. To the foot of the ſupport *l m* a plate *n o* is ſcrewed, the upper part of which *p q* being lined with buckſkin and hair approaches the glaſs veſſel. Fig. 8. Fig 9. Fig. 7.

Into the ball *a b c d* is infixed a perforated metalline cylinder *g*, thro' the cavity of which a piece of catgut is paſſed. This catgut is wound about the wooden arm *e f* within the ball, and has a button which faſtens it to a bent elaſtic plate perforated at the end *r ſ t*. The catgut is let out of the bell thro' a hog's bladder open on both ſides. One part of the bladder is bound about the metalline tube *g*, and tied with a piece of packthread; and the other *u* is ſtrongly faſtened between 2 knots made in the catgut. The bladder is wetted, ſo that after it has been wiped on the inſide with a linen cloth, it may eaſily be extended or contracted. On the outſide of the bladder appears a certain part of the catgut *u x*, by the drawing of which the glaſs veſſel may be agitated and rubbed under the bell. Fig. 6.

§ 16. In a ſquare iron veſſel $\alpha\ \beta\ \gamma\ \delta$, which is placed either in a ſilken net extended over a hollow glaſs veſſel *a b c d*, Fig. 8. or upon reſin, or ſealing wax, and has an iron ſtile $\gamma\ \varepsilon$, annexed to it and extended toward the cuſhion, are placed ſmall bits of leaf-gold. To a moveable metalline cylinder $\zeta\ \eta$, which may be thruſt thro' the middle of the neck of the bell, is annexed tranſverſly an iron wire $\eta\ \vartheta$, 2 or 3 lines diſtant from the pieces of leaf-gold, which leap towards it, as ſoon as the glaſs veſſel, on the air being drawn out of the bell, is agitated, and rubbed by the cuſhion. Fig. 6, 7, 8.

§ 17. In the other perforated ſide of the bell λ, a ſmall glaſs tube is fixed, thro' which an iron wire, ϰ λ δ, reaches to the middle of the glaſs veſſel, an exceeding ſmall ſpace being between the veſſel and the wire. The tube and the wire are ſo ſtrongly cemented with ſealing-wax, that no air can penetrate. And that it may be all driven out, the moveable cylinder ζ η, is covered with ſuet, where it touches the neck of the bell. On drawing the catgut *x u g*, the wire not only conceives Electricity from the agitation and attrition of the veſſel, but alſo propagates it thro' the glaſs tube ſtopt with ſealing-wax, and communicates it to bodies laid on ſilk, which touch the wire on the outſide in ϰ, ſo that the metals emit electrical ſparks in the dark, on the approach of bodies void of Electricity.

§ 18. Thus alſo Electricity excited without is communicated to the wire, and pervades thro' the ſealed tube, and emits fire in the dark at the end of the wire within the bell, and attracts the leaf-gold on the iron veſſel.

Uſe of the machine deſcribed in Fig. 4.

§ 19. Between the 2 anterior columns *a b* and *c d*, are ſuſpended glaſs veſſels or balls *e* and *f*, and an elaſtic plate *i k* is put into the upper hole of the third poſterior column, and a wheel is added to the ſide. A catgut faſtened to the elaſtic plate in *k* is wound round the longer arms of the veſſels, and faſtened to the moveable plank *b l m n*. Thus the glaſs veſſels may be turned round.

§ 20. In order to turn a glaſs veſſel or ball, a cord *o p q r* is brought round the wheel, and the wooden pullies of the veſſels or balls, and may be ſtraitened or looſened by means of a ſcrew applied to the hinder part of the machine.

§ 21. The anterior columns are faſtened by 2 braces, from which 2 perforated cylinders ſtand out, in the hinder part of which a very ſmall column is fixed, in which again two little cylinders covered with buck-ſkin with hair underneath is faſtened: but in the fore part an inſtrument in which ſilken threads are extended, to which an iron tube with 2 arms is faſtened. This tube is held by perſons ſtanding on ſilken nets to be electrified. Into this tube if a ſword is put, which hangs by the hilt with a ſilken thread, the electrical ſparks will be emitted from it's ſhell, and kindle ſpirit of wine in a ſmall ſpoon. Fig. 11. So what I call an electrical ſtar * is laid on a large ſilken net, and connected by means of an iron wire with the brachiated glaſs tube, annexed to a ſmaller net near the veſſels or balls. As ſoon as the glaſs veſſels in turning receive the friction of the cuſhions, or hand, the rays of the ſtar emit ſhining ſtreaks in the dark, and when the ſtar is turned round, deſcribe a lucid circle.

§ 22. When the veſſels are turned round, ſilver threads, touching the veſſels are joined to the arms of the iron tube. Thus a continued ſtream of Electricity is obtained. But on the contrary the Electricity is diminiſhed, if the extremities of the veſſels, as they turn, have ſilver

* See the *Acta Germanica*, or *Literary Memoirs of* Germany, Vol. II. p. 123.

threads

Fig. 1.

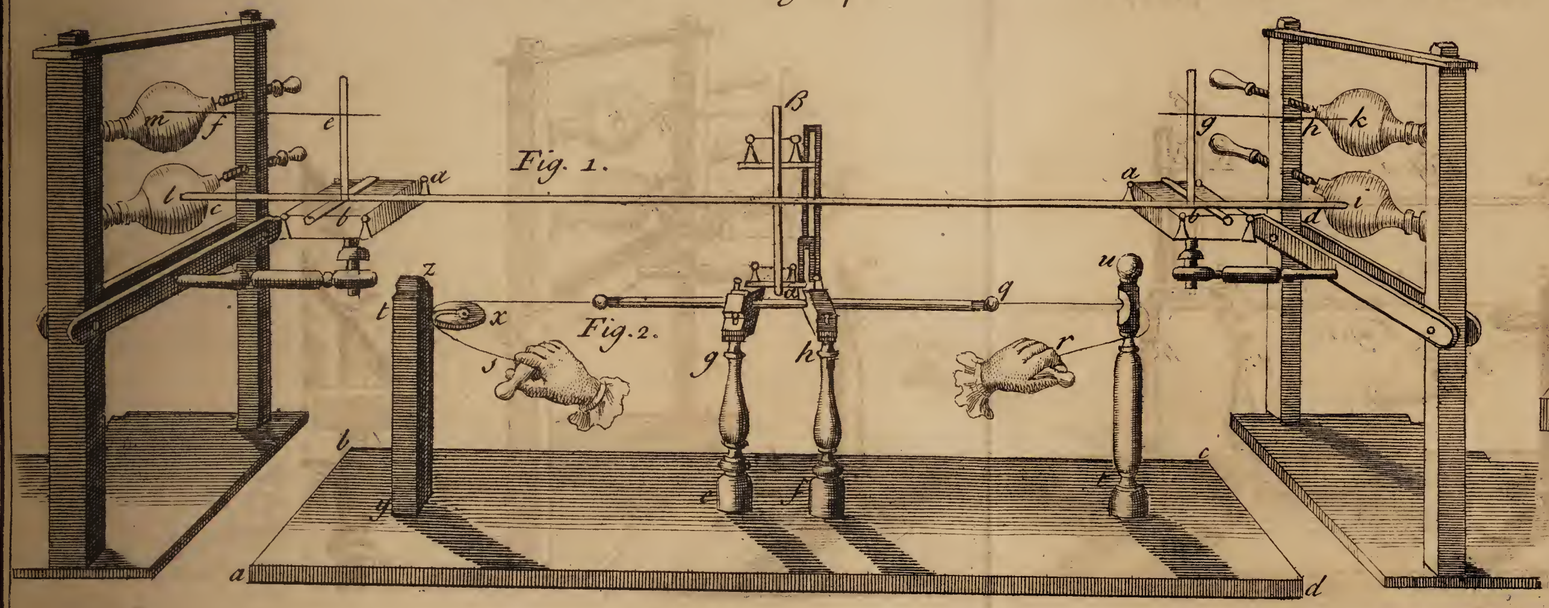
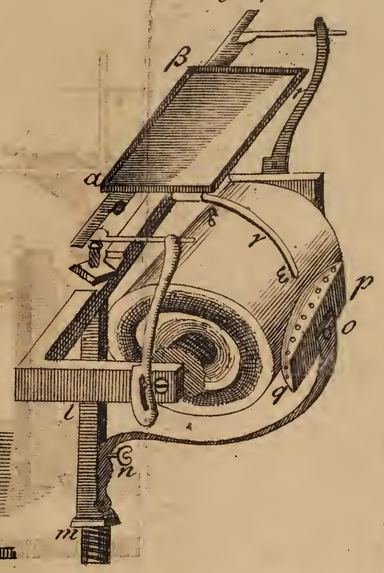


Fig. 2.

Fig. 7.



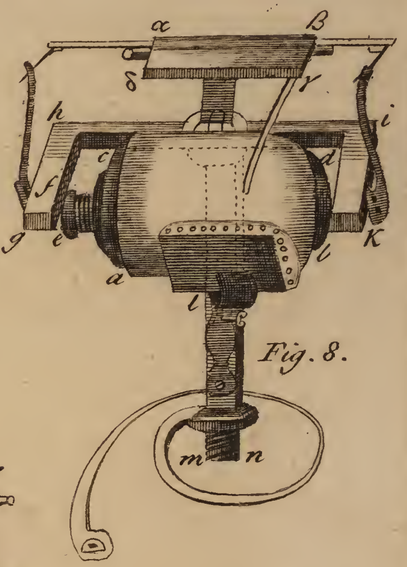


Fig. 8.

Fig. 5.

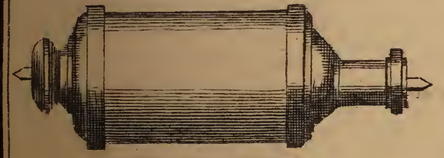


Fig. 3.

Fig. 4.

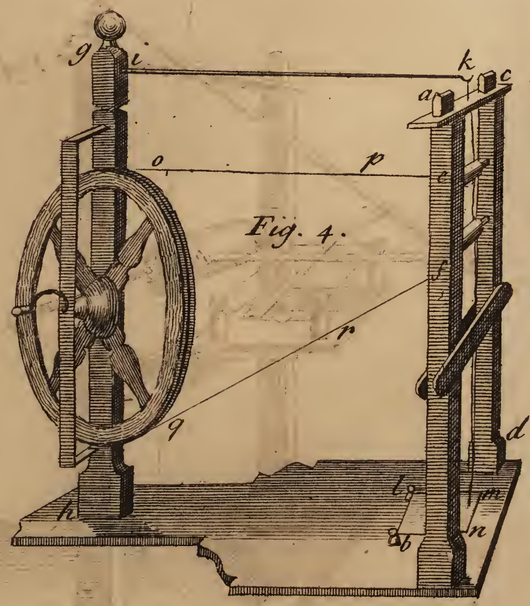


Fig. 10.

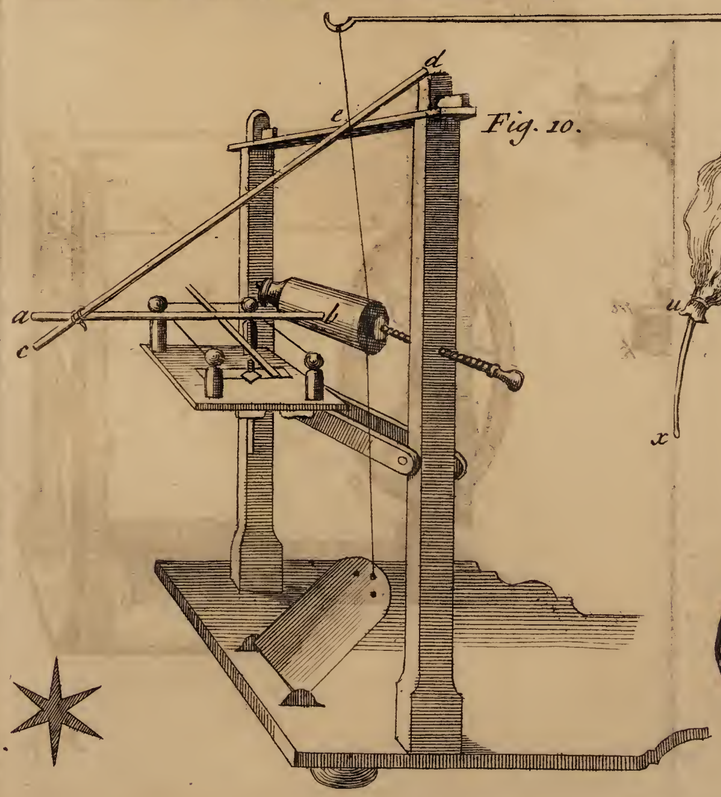


Fig. 11.

Fig. 9.

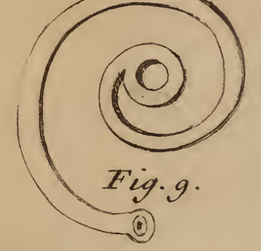
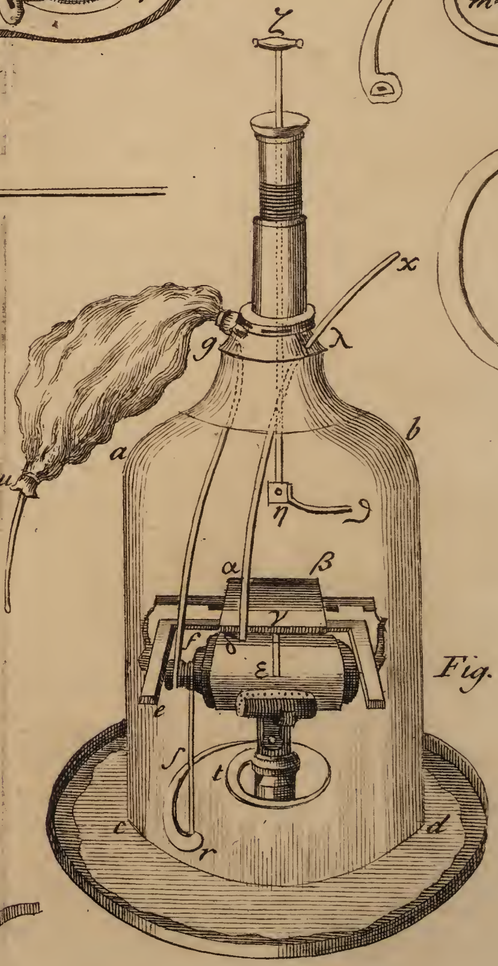


Fig. 6.



threads added, which touch the veſſels. In like manner if cuſhions are applied inſtead of the hand, the Electricity decreaſes.

5. A hollow globe of glaſs, of 6 or 8 inches diameter, being ſwiftly turned round upon it's *axis*, by means of a large wheel, in the manner Mr *Haukſbee* formerly adviſed; and being rendered as electrical as poſſible by the application of a dry woollen cloth, or rather of a very dry hand; if, whilſt in this ſwift rotation, it be brought near the end of an iron bar, ſuſpended by ſtrings of ſilk that are exceedingly well dried, ſuch an electric power will be communicated to the iron, that upon touching the other end of it with one's finger, not only ſparks of fire, in the uſual manner, will be emitted very briſkly, but even blood will be drawn from the finger; the ſkin of which will be burſt, and a wound appear as if made by a cauſtic.

Abſtract of a letter from M. De Bozes, *Prof. Exper. Philoſ. at the Acad of* Wirtemberg, *to M.* De Maizeaux. *Communicated by Mr* Baker *from Mr* Ellis, *and tranſlated out of the* Latin *by Mr*. Baker. N°. 476. p. 419. Apr. *&c.* 1745. *Read* May 23. 1745.

2. If highly rectified ſpirit of wine heated in a ſpoon, the ethereal ſpirit of *Frobenius*, oil of turpentine, ſulphur, pitch, or reſin melted, be applied to the iron bar, inſtead of one's finger, the ſparks proceeding therefrom will ſet it on fire inſtantly.

3. A chair being ſuſpended by ropes of ſilk, made perfectly dry, a man placed therein is rendered ſo much electrical by the motion of the above-mentioned globe, that, in the dark, a continual radiance, or *corona* of light, appears incircling his head, in the manner ſaints are painted.

4. If ſeveral ſuch-like globes, or electric tubes, are brought near the man ſuſpended in the chair, the motions of the heart and arteries are very ſenſibly increaſed; and if a vein be opened under the operation, the blood that comes from it appears lucid like *phoſphorus*, and runs out faſter than when the man is not electrify'd.

5. Water, in like manner, ſpouting from an artificial fountain ſuſpended by ſilk lines, ſcatters itſelf in luminous little drops; and a larger quantity of water is thrown out, in any given time, than when the fountain is not made electric.

N. B. If 3, 4, or 5 globes be employed, the effect will be proportionably better: and M. *L'Abbé Nollet* has found, that globes or tubes made of glaſs, coloured blue with *zaffer*, are preferable to others; for when the glaſs is blue, the experiments ſucceed in all weathers; whereas, in damp weather, the white glaſs loſes much of it's electric power.

6. In the late edition of the works of the Hon. Mr *Boyle*, * is a letter from Mr *Clayton*, dated *June* 23. 1684. at *James* city in *Virginia*; in which he gives Mr *Boyle* an account of a ſtrange accident (as he calls it); and adds, that he had incloſed the very paper Colonel *Digges* gave him of it, under his own hand and name, to atteſt the truth; and that the ſame was alſo aſſerted to him by Madam *Digges*, his lady, ſiſter to the

A letter from the Rev. Henry Miles, *D. D. F. R. S. to the* Preſ. *containing Obſervations of* luminous Emanations.

* Vol. V. Page 646.

wife

from human Bodies, *and from* Brutes; *with some Remarks on* Electricity. *Ibid* p. 441. *dated* May 9. 1745. *Read* June 13. 1745.

wife of Major *Sewall*, and daughter of the Lord *Baltimore*, to whom this accident happened.

This paper, very unhappily, came not to hand till after Mr *Boyle*'s works were printed; and therefore could not be inſerted with Mr *Clayton*'s letter: but, having ſince met with it, I preſent the following exact copy of it.

" *Maryland*, *Anno* 1683.

" There happened, about the month of *November*, to one Mrs *Su-*
" *ſannah Sewall*, wife to Major *Nic. Sewall* of the province aboveſaid,
" a ſtrange flaſhing of ſparks (ſeem'd to be of fire) in all the wearing
" apparel ſhe put on, and ſo continued till *Candlemas*: and, in the
" company of ſeveral, *viz.* Captain *John Harris*, Mr *Edward Branes*,
" Captain *Edward Poulſon*, &c. the ſaid *Suſannah* did ſend ſeveral of
" her wearing apparel; and, when they were ſhaken, it would fly out
" in ſparks, and make a noiſe much like unto bay-leaves when flung
" into the fire; and one ſpark litt on Major *Sewall*'s thumb-nail, and
" there continued at leaſt a minute before it went out, without any
" heat: all which happened in the company of

Wm. Digges,

* " My Lady *Baltimore*, her mother-in-law, for ſome time before
" the death of her ſon *Cæcilius Calvert*, had the like happened to her;
" which has made Madam *Sewall* much troubled at what has happened
" to her."

" They cauſed Mrs *Suſanna Sewall* one day to put on her ſiſter
" *Digges*'s petticoat, which they had tried beforehand, and would not
" ſparkle; but at night when Madam *Sewall* put it off, it would ſparkle
" as the reſt of her own garments did."

The celebrated *Bartholin* of *Copenhagen*, in his collection of anatomical hiſtories that are unuſual, † which he intitles *Mulier ſplendens*, gives us a parallel inſtance in a noble lady of *Verona* in *Italy*, which, he ſays, he had from an account of the *phænomenon* publiſhed by *Petrus à Caſtro*, a learned Phyſician of the ſame place, in a ſmall treatiſe intituled *De Igne Lambente*. There is this circumſtance not mentioned in Mrs *Sewall*'s caſe (tho' perhaps it would have happened, if trial had been made, as well as in the caſe of the *Italian* lady); which I think not improper to mention, in *Bartholin*'s own words. ——— " *ut quo-*
" *tiens leviter linteo corpus tetigerit, ſcintillæ ex artubus copioſe proſiliant,*
" *cunctis domeſticis conſpicuæ, non ſecus ac ſi è ſilice excuterentur.*" At the concluſion of this relation he refers us to a book of his, intituled, *De Luce Animalium*, for more inſtances of theſe lucid *effluvia*; and

* The additional lines are not in Colonel *Digges*'s hand, but ſeem to be in Mr *Clayton*'s.

† Cent. III. Hiſt. LXX. ‖ Hiſt XII.

ſays

ſays, he has there ſhown the cauſe of them at large; but, as I have not yet got a ſight of that book, I can ſay nothing further —— only, that in the ſecond Cent. of the hiſtories above-mentioned, * he aſſerts, that he has prov'd, in his book *de Luce*, &c. that light is connatural or innate to all, as well vegetables as animals.

There is another author, Dr *Simpſon*, who publiſhed a Philoſophical Diſcourſe of Fermentation, dedicated to the *R. Soc.* 1675. who takes notice of light proceeding from animals, on the frication or pectation (as he calls it) of them; and inſtances in the combing a woman's head, the currying of a horſe, and the frication of a cat's back; the two laſt of which are known to moſt. I cannot tell whether it be material to add, that, according to this gentleman's hypotheſis, he would aſſign the principles of fermentation, which he ſuppoſes to be *Acidum & Sulphur*, as the cauſe of theſe lucid *effluvia* in animals. His hypotheſis I may not take upon me to judge of; but I humbly apprehend, the properties of the *effluvia* in animal bodies are many of them common with thoſe produced from glaſs, *&c.*; ſuch as their being lucid, their ſnapping, and their not being excited without ſome degree of friction, and, I preſume, I may add, Electricity; for I have, by repeated trials, found a cat's back to be ſtrongly electrical when ſtroak'd.

P. S. In the account of ſome of the earlier electrical experiments made by Mr *Gray* †, we are informed, that he electrified ſeveral other bodies, beſides animal ſubſtances, by drawing them between his thumb and fingers; in particular, linen of divers ſorts, paper, and fir-ſhavings, which would not only be attracted to his hand, but attract all ſmall bodies to them, as other electric bodies do. Now, notwithſtanding this laſt circumſtance of their attracting, as well as being attracted, may it not be queſtioned, whether, in this way of trial, it appears that they are electrical bodies, or Electrics *per ſe?* Is it not doubtful (ſince his fingers muſt be excited conſiderably in this experiment) whether he did not communicate Electricity to them from his hand, rather than excite it in them? I have no doubt but that the principle is inherent in many other bodies beſides animal, poſſibly, in all bodies whatever; but as it is allow'd, I ſuppoſe generally, that animals have a greater quantity of it reſiding in them, than other ſubſtances, there ſeems room to admit the doubt I have mention'd, which I ſubmit to the conſideration of ſuch as are curious in experiments of this kind.

7. The Society having heard, from ſome of their correſpondents in *Germany* ‖, that what they call a vegetable quinteſſence had been fired by Electricity, I take this opportunity to acquaint you, that, on *Friday* *Experiments and obſervations, tending to illuſtrate the nature*

* Hiſt. XII.

† See Vol. V. Part ii. Chap. I. Sect. III. 1.

‖ See the preceding Articles.

evening

and properties of Electricity; *by* Will. Watſon, *Apothecary, F. R. S.* N°. 477. p. 481. Aug. &c. 1745. *Read at ſeveral meetings of the* R. S. *between* Mar. 28, *and* Oct. 24, 1745. *here printed with alterations. A letter to* Martin Folkes, *Eſq*; *Pr. R. S. dated* Mar. 27, 1745.

evening laſt, I ſucceeded, after having been diſappointed in many attempts, in ſetting ſpirits of wine on fire by that power.

The preceding part of the week had been remarkably warm, and the air very dry; than which nothing is more neceſſary towards the ſucceſs of electrical trials: to theſe I may add, that the wind was then eaſterly, and inclining to freeze. I that evening uſed a glaſs ſphere, as well as a tube; but I always find myſelf capable of ſending forth much more fire from the tube than from the ſphere, probably from not being ſufficiently uſed to the laſt.

I had before obſerv'd, that, altho' * non-electric bodies made electrical, loſe almoſt all that Electricity, by coming either within or near the contact of *non-electrics* not made electrical. It happens otherwiſe with regard to *Electrics per ſe*, when excited by rubbing, patting, &c.; becauſe from the rubbed tube I can ſometimes procure five or ſix flaſhes from different parts; as though the tube of 2 feet long, inſtead of being one continued cylinder, conſiſted of five or ſix ſeparate ſegments of cylinders, each of which gave out it's Electricity at a different exploſion.

The knowledge of this theorem is of the utmoſt conſequence towards the ſucceſs of electrical experiments; inaſmuch as you muſt endeavour, by all poſſible means, to collect the whole of this fire at the ſame time. Prof. *Hollman* ſeems to have endeavour'd at this, and ſucceeded, by having a tin tube; in one end of which he put a great many threads, whoſe extremities touch'd the ſphere when in motion, and each thread collected a quantity of electrical fire, the whole of which center'd in the tin tube, and went off at the other extremity. Another thing to be obſerved is to endeavour to make the flaſhes follow each other ſo faſt, as that a ſecond may be viſible before the firſt is extinguiſh'd. When you tranſmit the electrical fire along a ſword, or other inſtrument, whoſe point is ſharp, it often appears as a number of diſſeminated ſparks, like wet gunpowder or wild-fire: but if the inſtrument has no point, you generally perceive a pure bright flame, like what is vulgarly call'd the *blue ball*, which gives the appearance of ſtars to fired rockets.

The following is the method I made uſe of, and was happy enough to ſucceed in. I ſuſpended a poker in ſilk lines; at the handle of which I hung ſeveral little bundles of white thread, the extremities of which were about a foot at right angles from the poker. Among theſe threads, which were all attracted by the rubbed tube, I excited the greateſt electrical fire I was capable, whilſt an aſſiſtant, near the end of the poker, held in his hand a ſpoon, in which were the warm ſpirits. Thus the

* I call *Electrics per ſe*, or originally *Electrics*, thoſe bodies, in which an attractive power towards light ſubſtances is eaſily excited by friction; ſuch as glaſs, amber, ſulphur, ſealing-wax, and moſt dry parts of animals, as ſilk, hair, and ſuch-like. I call *Non-Electrics*, or conductors of Electricity, thoſe bodies in which the above property is not at all, or very ſlightly, perceptible; ſuch as wood, animals living or dead, metals, and vegetable ſubſtances. See *Gray, Du Fay, Deſaguliers, Wheler*, in the *Philoſ. Tranſ.*

thread

thread communicated the Electricity to the poker, and the ſpirit was fired at the other end. It muſt be obſerv'd in this experiment, that the ſpoon with the ſpirit muſt not touch the poker; if it does, the Electricity, without any flaſhing, is communicated to the ſpoon, and to the aſſiſtant in whoſe hand it is held, and ſo is loſt in the floor.

By theſe means I fired ſeveral times not only the ethereal liquor or *Phlogiſton* of *Frobenius*, and rectified ſpirit of wine, but even common proof ſpirit. Theſe experiments, as I before obſerved, were made laſt *Friday* night, the air being perfectly dry. *Sunday* proved wet, and *Monday* ſomewhat warm; ſo that the air was full of vapour, wind S. W. and cloudy. Under theſe diſadvantages, on *Monday* night I attempted again my experiments; they ſucceeded, but with infinitely more labour than the preceding, becauſe of the unfitneſs of the evening for ſuch trials.

I lately acquainted you, that I had been able to fire ſpirit of wine, *Phlogiſton* of *Frobenius*, and common proof ſpirit, by the power of Electricity. Since which (till yeſterday) we have had but one very dry fine day; *viz.* *Monday Apr.* 15. wind E. N. E.; when, about 4 in the afternoon, I got my *apparatus* ready, and fired the ſpirit of wine four times from the poker as before, 3 times from the finger of a perſon electrified, ſtanding upon a cake of wax, and once from the finger of a ſecond perſon ſtanding upon wax, communicating with the firſt by means of a walking-cane held between their arms extended. The horizontal diſtance in this caſe between the glaſs tube and the ſpirit was at leaſt ten feet.

A letter to the Royal Society, *dated* Apr. 25, 1745. *Read* April 25, 1745.

You all know, that there is the repulſive power of Electricity, as well as the attractive; inaſmuch as you are able, when a feather, or ſuch-like light ſubſtance, is replete with Electricity, to drive it about a room, which way you pleaſe. This repulſive power continues, until either the tube loſes it's excited force, or the feather attracts the moiſture from the air, or comes near to ſome non-electric ſubſtance; if ſo, the feather is attracted by, and it's Electricity loſt in, whatever non-electric it comes near. In electrified bodies, you ſee a perpetual endeavour to get rid of their Electricity. This induced me to make the following experiment.

I placed a man upon a cake of wax, who held in one of his hands a ſpoon with the warm ſpirits, and in the other a poker with the thread. I rubbed the tube amongſt the thread, and electrified him as before. I then ordered a perſon not electrified to bring his finger near the middle of the ſpoon; upon which, the flaſh from the ſpoon and ſpirit was violent enough to fire the ſpirit. This experiment I then repeated three times.

In this method, the perſon by whoſe finger the ſpirit of wine is fired, feels the ſtroke much more violent, than when the electrical fire goes from him to the ſpoon. This way, for the ſake of diſtinction, we will call the repulſive power of Electricity.

The late Dr *Deſaguliers* has obſerved, in his excellent Diſſertation concerning Electricity. ‘ That there is a ſort of capriciouſneſs attending

'theſe experiments, or ſomething unaccountable in their *phæno-*
'*mena*, not to be reduced to any rule. For ſometimes an experi-
'ment, which has been made ſeveral times ſucceſſively, will all at
'once fail.' Now I imagine, that the greateſt part, if not the whole of this matter, depends upon the moiſture or dryneſs of the air; a ſudden though ſlight alteration in which, perhaps not ſufficient to be obvious to our faculties, may be perceived by the very ſubtle fire of Electricity. For,

1ſt, I conceive, that the air itſelf (as has been obſerved by Dr *Deſaguliers*) is an electric *per ſe*, and of the vitreous kind; therefore it repels the Electricity ariſing from the glaſs tube, and diſpoſes it to electrify whatever non-electrical bodies receive the *effluvia* from the tube.

2dly, That water is a non-electric, and, of conſequence, a conductor of Electricity. This is exemplified by a jett of water being attracted by the tube, from either electrics *per ſe* conducting Electricity, and non-electrics more readily when wetted; but what is more to my preſent purpoſe, is, that if you only blow through a dry glaſs tube, the moiſture from your breath will cauſe that tube to be a conductor of Electricity.

Theſe being premiſed, in proportion as the air is replete with watery vapours, the Electricity ariſing from the tube, inſtead of being conducted, as propoſed, is, by means of theſe vapours, communicated to the circumambient atmoſphere, and diſſipated as faſt as excited.

This theory has been confirmed to me by divers experiments, but by none more remarkably than on the evening of the day I made thoſe before-mention'd; when the vapours, which in the afternoon, by the ſun's heat, and a briſk gale, were diſſipated, and the air perfectly dry, deſcended again in great plenty, upon the abſence of both, and in the evening was very damp. For between ſeven and eight o'clock, I attempted again the ſame experiments in the ſame manner, without being able to make any of them ſucceed; though all thoſe mentioned in this paper, with others of leſs note, were made in leſs than half an hour's time.

I am the more particular in this, being willing to ſave the labour of thoſe, who are deſirous of making this kind of trials. For, although ſome of the leſſer experiments may ſucceed almoſt at any time, yet I never could find, that the more remarkable ones would ſucceed but in dry weather.

A letter to the Royal Society *Read* Octob. 24. 1745.

In ſome papers I lately did myſelf the honour to lay before you, I acquainted you of ſome experiments in Electricity; particularly I took notice of having been able to fire ſpirit of wine by what I called the repulſive power thereof; which I have not heard had been thought of by any of thoſe *German* gentlemen, to whom the world is obliged for many ſurpriſing diſcoveries in this part of Natural Philoſophy.

How far, ſtrictly ſpeaking, the ſpirit, in this operation, may be ſaid to be fired by the repulſive power of Electricity, or how far that power, which

which repels light ſubſtances when fully impregnated with Electricity, fires the ſpirit, may probably be the ſubject of a future inquiry; but, as I am unwilling to introduce more terms into any demonſtration than what are abſolutely neceſſary for the more ready conception thereof, and as inflammable ſubſtances may be fired by Electricity two different ways, let the following definitions at preſent ſuffice of each of theſe methods.

But firſt give me leave to premiſe, that no inflammable ſubſtances will take fire, when brought into or near the contact of electrics *per ſe* excited to Electricity. This effect muſt be produced by non-electrical ſubſtances impregnated with Electricity received from the exciting electrics *per ſe*. But to return:

1*ſt*, I ſuppoſe that inflammable ſubſtances are fired by the attractive power of Electricity, when this effect ariſes from their being brought near excited non-electrics.

2*dly*, That inflammable ſubſtances are fired by the repulſive power of Electricity; when it happens, that the inflammable ſubſtances, being firſt electrified themſelves, are fired by being brought near non-electrics not excited.

This matter will be better illuſtrated by an example. Suppoſe that either a man ſtanding upon a cake of wax, or a ſword ſuſpended in ſilk lines, are electrified, and the ſpirit, being brought near them, is fired, this is ſaid to be performed by the attractive power of Electricity. But if the man electrified, as before, holds a ſpoon in his hand containing the ſpirit, or the ſame ſpoon and ſpirit are placed upon the ſword, and a perſon not electrified applies his finger near the ſpoon, and the ſpirit is fired from the flame ariſing from the ſpoon and ſpirit upon ſuch application, this I call being fired by the repulſive power. Of the two mention'd kinds I generally find the repulſive power ſtrongeſt.

Since my laſt communication, the ſpirit has been fired both by the attractive and repulſive power thro' four perſons ſtanding upon electrical cakes, each communicating with the other, either by the means of a walking-cane, a ſword, or any other non-electric ſubſtance. It has likewiſe been fired from the handle of a ſword held in the hand of a third perſon.

I have not only fired *Frobenius*'s *Phlogiſton*, rectified ſpirit, and common proof ſpirit, but alſo *Sal volatile oleoſum*, ſpirit of lavender, dulcified ſpirit of Nitre, Peony-water, *Daffy*'s elixir, *Helvetius*'s ſtyptic, and ſome other mixtures where the ſpirit has been very conſiderably diluted; likewiſe diſtilled vegetable oils, ſuch as that of turpentine, lemon, orange-peels, and juniper; and even thoſe of them which are ſpecifically heavier than water, as oil of ſaſſafras; alſo reſinous ſubſtances, ſuch as balſam *Capivi*, and turpentine; all which ſend forth, when warmed, an inflammable vapour. But expreſſed vegetable oils, as thoſe of olives, linſeed, and almonds, as well as tallow, all whoſe vapours are uninflammable, I have not been able yet to fire; but theſe

indeed will not fire on the application of lighted paper. Beſides, if theſe laſt would fire with lighted paper, unleſs their vapours were inflammable, I can ſcarce conceive they would fire by Electricity; becauſe, in firing ſpirits, *&c.* I always perceive, that the Electricity ſnaps, before it comes in contact with their ſurfaces, and therefore only fires their inflammable vapours.

As an excited non-electric emits almoſt all its fire, if once touch'd by a non-electric not excited, I was deſirous of being ſatisfy'd, whether or no the fire emitted would not be greater or leſs in proportion to the volume of the electrified body. In order to this, I procured an iron bar about 5 feet long, and near 170 pounds in weight; this I electrified lying on cakes of wax and reſin, but obſerved the flaſhes ariſing therefrom not more violent than thoſe from a common poker. In making this experiment, being willing to try the repulſive force, it once happen'd, that whilſt the bar was at one end electrifying, a ſpoon lay upon the other; and, upon an aſſiſtant's pouring ſome warm ſpirit into the ſpoon, the electrical flaſh from the ſpoon ſnapped, and fired the firſt drop of the ſpirit; which unexpectedly fired not only the whole jett as it was pouring, but kindled likewiſe the whole quantity in the pot, in which I uſually have it warm'd.

I find, in firing inflammable ſubſtances from the finger of a man ſtanding upon wax, that, *cæteris paribus*, the ſucceſs is more conſtant, if the man, inſtead of holding the thread (the uſe of which I communicated in a former paper) in his hand, the thread is ſuſpended at the end of an iron rod held in one hand, and he touches the ſpirit with one of the fingers of the other.

If a man, ſtanding upon the electrical cake with a diſh or deep plate of water in one hand, and the iron rod with the thread in the other, is made electrical, and a perſon not electrified touches any part either of the plate or water, the flaſhes of fire come out plentifully; and whereever you bring your finger very near, the water riſes up in a little cone, from the point of which the fire is produced, and your finger, though not in actual contact, is made wet. The ſame experiment ſucceeds through three or more people.

In firing inflammable ſubſtances, the Perſon who holds the ſpoon in his hand to receive the electrical flaſhes, when the finger of the electrified perſon is brought near thereto, not only feels a tingling in his hand, but even a ſlight pain up to his elbow. This is moſt perceptible in dry weather, when the Electricity is very powerful.

There is conſiderable difficulty in firing electrics *per ſe*, ſuch as turpentine and balſam *Capivi*, by the repulſive power of Electricity; becauſe, in this caſe, theſe ſubſtances will not permit the Electricity to paſs through them: therefore, when you would have this experiment ſucceed, the finger of the perſon who is to fire them, is to be applied as near to the edge as poſſible of theſe ſubſtances when warmed in a ſpoon, that the flaſhes from the ſpoon (for theſe ſubſtances will emit none) may

ſnap,

ſnap, where they are ſpread the thinneſt, and then fire their *effluvia*. This experiment, as well as ſeveral others, ſerves to confute that opinion, which has prevailed with many, that the Electricity floats only upon the ſurfaces of bodies.

If an electrical cake is dipp'd in water, it is thereby made a conductor of Electricity; the water hanging about it tranſmitting the electrical *effluvia* in ſuch a manner, that a perſon ſtanding thereon can by no means be electrified enough to attract the leaf-gold at the ſmalleſt diſtance; though the perſon ſtanding upon the ſame cake when dry, attracted a piece of fine thread hanging at the diſtance of two feet from his finger. We muſt here obſerve, that the cake being of an unctuous ſubſtance, the water will no-where lie uniformly thereon, but adhere in ſeparate *moleculæ*; ſo that, in this inſtance, the Electricity jumps from one particle of water to another, till the whole is diſſipated.

From the appearance of the threads, amongſt which I rub the tube, I can frequently judge, though the ſpirit may be many feet diſtant from them, whether or no it will fire; becauſe, when the perſons ſtanding upon the wax are made electrical enough to fire the ſpirit, the threads repel each other at their lower parts, where they are not confin'd to a conſiderable diſtance; and this diſtance is in proportion as the threads are made electrical.

If two perſons ſtand upon electrical cakes at about a yard's diſtance from each other, one of which perſons, for the ſake of diſtinction, we will call *A*, the other *B*; if *A*, when electrified, touches *B*, *A* loſes almoſt all his Electricity at that touch only, which is received by *B*, and ſtopped by the electrical cake: if *A* is immediately electrified again to the ſame degree as before, and touches *B*, the ſnapping is leſs upon the touch; and this ſnapping, upon electrifying *A*, grows leſs and leſs, till *B*, being impregnated with Electricity, though received at intervals, the ſnapping will no longer be ſenſible.

That glaſs will repel and not conduct the Electricity of glaſs, has been mention'd by others, who have treated of this ſubject; but the experiments to determine this matter muſt be conducted with a great deal of caution; for, unleſs the glaſs tube, intended to conduct the Electricity, be as warm as the external air, it will ſeem to prove the contrary, unleſs in very dry places and ſeaſons. Thus I ſometimes have brought a cold though dry glaſs tube near three foot long into a room where there has been a number of people; when, upon placing the tube upon ſilk lines, and laying ſome leaf-ſilver upon a card at one end, and rubbing another glaſs tube at the other, the ſilver has, contrary to expectation, been thrown off as readily as from an iron rod. At firſt I was ſurprized at this appearance; but then conjectur'd, that it muſt ariſe from the coldneſs of the glaſs, condenſing the floating vapour of the room. In order then to obviate this, I warm'd the tube ſufficiently, and this effect was no longer produc'd, but the ſilver lay perfectly ſtill.

If

If a number of pieces of finely ſpun glaſs, cut to about an inch in length, little bits of fine wire of the ſame length, of what metal you pleaſe, and ſmall cork-balls, are either put all together, or each by themſelves, into a dry pewter plate, or upon a piece of poliſhed metal, they make, in the following manner, a very odd and ſurpriſing appearance. Let a man, ſtanding upon electrical cakes, hold this plate in his hand, with the bits of glaſs, wire, *&c.* detached from each other, as much as conveniently may be; when he is electrified, let him cauſe a perſon ſtanding upon the ground to bring another plate, his hand, or any other non-electric, exactly over the plate, containing theſe bodies. When his hand, *&c.* is about 8 inches over them, let him bring it down gently: as it comes near, in proportion to the ſtrength of the Electricity, he will obſerve the bits of glaſs firſt raiſe themſelves upright; and then, if he brings his hand nearer, dart directly up, and ſtick to it without ſnapping. The bits of wire will fly up likewiſe, and as they come near the hand ſnap aloud; you feel a ſmart ſtroke, and ſee the fire ariſing from them to the hand at every ſtroke: each of theſe, as ſoon as they have diſcharged their fire, falls down again upon the plate. The cork-balls alſo fly up and ſtrike your hand, but fall again directly. You have a conſtant ſucceſſion of theſe appearances, as long as you continue to electrify the man in whoſe hand the plate is held; but if you touch any part either of the man or plate, the pieces of glaſs, which before were upon their ends, immediately fall down.

Some few years ago, Sir *James Lowther* brought ſome bladders fill'd with inflammable air, collected from his coal-mines, to the *Royal Society*. This air flamed, upon a lighted candle being brought near it. This inflammability has occaſion'd many terrible accidents. Mr *Maud*, a worthy member of this *Society*, made at that time, by art, and ſhew'd the *Society*, air exactly of the ſame quality. I was deſirous of knowing if this air would be kindled by electrical flaſhes. I accordingly made ſuch air, by putting an ounce of filings of iron, an ounce of oil of vitriol, and four ounces of water, into a *Florence* flaſk; upon which an ebullition enſued, and the air, which aroſe from theſe materials, not only fill'd three bladders, but alſo, upon the application of the finger of an electrified perſon, took flame, and burnt near the top and out of the neck of the flaſk a conſiderable time. When the flame is almoſt out, ſhake the flaſk, and the flame revives. You muſt, with your finger dipped in water, moiſten the mouth of the flaſk as faſt as it is dried by the heat within, or the Electricity will not fire it: becauſe the flaſk, being an electric *per ſe*, will not ſnap at the application of the finger, without the glaſs being firſt made non-electric by wetting. It has ſometimes happen'd, if the finger has been applied before the inflammable air has found a ready *exit* from the mouth of the flaſk, that the flaſh has filled the flaſk, and gone off with an exploſion equal to the firing of a large piſtol; and ſometimes indeed it has burſt the flaſk. The ſame effect is produced from ſpirit of ſea ſalt, as from oil of vitriol; but as the acid of ſea-ſalt is much

lighter

lighter than that of vitriol, there is no neceſſity to add the water in this experiment.

Thoſe who are not much acquainted with Chemical Philoſophy, may think it very extraordinary, that, from a mixture of cold ſubſtances, which, both conjunctly and ſeparately, are uninflammable, this very inflammable vapour ſhould be produced. In order to ſolve this, it may not be improper to premiſe, that iron is compounded of a ſulphureous as well as a metallic part. This ſulphur is ſo fixed, that, after heating the iron red hot, and even melting it ever ſo often, the ſulphur will not be diſengaged therefrom: but, upon the mixture of the vitriolic acid, and by the heat and ebullition which are almoſt inſtantly produced, the metallic part is diſſolved, and the ſulphur, which before was intimately connected therewith, being diſengaged, becomes volatile. This heat and ebullition continue, till the vitriolic acid is perfectly ſaturated with the metallic part of the iron; and the vapour, once fired, continues to flame, until, this ſaturation being perfected, no more of the ſulphur flies off.

I have heretofore mentioned, how conſiderably perfectly dry air conduces to the ſucceſs of theſe experiments; but we have been lately informed, by an extract of a letter, that Abbé *Nolet* was of opinion, that they would ſucceed in wet weather, provided the tubes were made of glaſs tinged blue with zaffer. I have procured tubes of this ſort, but, after giving them many candid trials, I cannot think them equal to their recommendation. I firſt tried one of them in a ſmart ſhower of rain after a dry day, when the drops were large, and the ſpirit fired 3 times in about 4 minutes: the ſame effect ſucceeded, under the ſame circumſtances, from the white one; but, after 3 or 4 hours raining, when the air was perfectly wet, I never could make it ſucceed. And, to illuſtrate this matter further, I have been able, when the weather has been very dry, with once rubbing my hand down this blue tube, and applying it to the end of an iron rod 6 feet long, to throw off ſeveral pieces of leaf-ſilver lying upon a card at the other end of this rod; whereas I never have been able to throw it off by any means in very wet weather. Beſides, I am of opinion, that, after the electrical fire is gone from the tube, the tube has no ſhare in the conducting of it: my ſentiments on that head I laid before you in a former paper: for if the ſilk lines are wetted, they diffuſe all the Electricity; and the ſame effects happen, when the air is wet, be your glaſs of what colour it will.

It may not be improper here to obſerve, that zaffer, which is uſed by the Glaſs-makers and Enamellers, is made of cobalt or mundick calcined after the ſubliming the flowers. This being reduced to a very fine powder, and mixt with twice or thrice it's own weight of finely powder'd flints, is moiſtened with water, and put up in barrels, in which it ſoon runs into an hard maſs, and is called zaffer.

A dry ſponge hanging by a packthread at the end of an electrified ſword, or from the hand of an electrified man, gives no ſigns of being

made

made electrical: if it is well ſoak'd in water, wherever it is touch'd, you both ſee and feel the electrical ſparks. Not only ſo, but, if it is ſo full of water that it falls from the ſponge, thoſe drops in a dark room, receiv'd upon your hand, not only flaſh and ſnap, but you perceive a pricking pain. If you hold your hand, or any non-electrical ſubſtances, very near the water, which had ceaſed dropping when the ſponge was not electrified, drops again upon it's being electrified, and the drops fall in proportion to the receiv'd Electricity, as though the ſponge were gently ſqueez'd between your fingers. I was deſirous to know if I was able to electrify a drop of cold water, dropping from the ſponge, enough to fire the ſpirit; but, after many unſucceſsful trials, I was forced to deſiſt; becauſe the cold water dropping from the ſponge not only cool'd the ſpirit too much, but alſo render'd it too weak: likewiſe every drop carried with it great part of the Electricity from the ſponge.

I then conſider'd, in what manner I could give a tenacity to the water ſufficient to make the drops hang a conſiderable time; and this I brought about by making a mucilage of the ſeeds of fleawort. A wet ſponge then, ſqueez'd hard, and fill'd with this cold mucilage, was held in the hand of an electrified man, when the drops, forced out by the Electricity, aſſiſted by the tenacity of the liquor, hung ſome inches from the ſponge; and by a drop of this, I fired not only the ſpirit of wine, but likewiſe the inflammable air before-mentioned, both with and without the exploſion. What an extraordinary effect is this, that a drop of cold water (for the ſeeds contribute nothing, but add conſiſtence to the water) ſhould be the *medium* of fire and flame?

Camphire is a vegetable reſin, and, of conſequence, an electric *per ſe*. This ſubſtance, notwithſtanding it's great inflammability, will not take fire from the finger of a man, or any other body electrified, tho' made very warm, and the vapours ariſe therefrom in great abundance; becauſe, neither electrics *per ſe* excited, or electrified bodies, exert their force by ſnapping upon electrics *per ſe*, though not excited. If you break camphire ſmall, and warm it in a ſpoon, it is not melted by heat like other reſins; but, if that heat were continued, it would all prove volatile. To camphire thus warm'd, the finger of an electrified man, a ſword, or ſuch-like, will, in ſnapping, exert it's force upon the ſpoon, and the circumambient vapour of the camphire will be fired thereby, and light up the whole quantity expoſed. The ſame experiment ſucceeds by the repulſive power of Electricity.

A poker, thoroughly ignited, put into ſpirit of wine, or into the diſtilled oil of vegetables, produces no flame in either. It indeed occaſions the vapors to ariſe from the oil in great abundance; but if you electrify this heated poker, the electrical flaſhes preſently kindle flame in either. The experiment is the ſame with camphire. Theſe experiments, as well as the following, ſufficiently evince, that the electrical fire is truly flame, and that extremely ſubtil.

I have

I have made ſeveral trials in order to fire gunpowder alone, which I tried both warm and cold, whole and powder'd, but never could ſucceed: and this ariſes, in part, from it's vapours not being inflammable, and in part from it's not being capable of being fir'd by flame; unleſs the ſulphur in the compoſition is nearly in the ſtate of accenſion. This we ſee, by putting gunpowder into a ſpoon with rectified ſpirit, which, when lighted, will not fire the powder, till, by the heat of the ſpoon from the burning ſpirit, the ſulphur is almoſt melted. Likewiſe, if you hold gunpowder ground very fine in a ſpoon over a lighted candle, or any other flame, as ſoon as the ſpoon is hot enough to melt the ſulphur, you ſee a blue flame, and inſtantly the powder flaſhes off. The ſame effects are obſerved in the *Pulvis fulminans*, compoſed of nitre, ſulphur, and fixed alkaline ſalt. Beſides, when the gunpowder is very dry, and ground very fine, it (as you pleaſe to make the experiment) is either attracted or repell'd; ſo that, in the firſt caſe the end of your finger, when electrified, ſhall be cover'd over with the powder, though held at ſome diſtance; and in the other, if you electrify the powder, it will fly off at the approach of any non-electrified ſubſtance, and ſometimes even without it. But I can, at pleaſure, fire gunpowder, and even diſcharge a muſket, by the power of Electricity, when the gunpowder has been ground with a little camphire, or with a few drops of ſome inflammable chemical oil. This oil ſomewhat moiſtens the powder, and prevents it's flying away: the gunpowder then being warm'd in a ſpoon, the electrical flaſhes fire the inflammable vapour, which fires the gunpowder: but the time between the vapour firing the powder is ſo ſhort, that frequently they appear as the ſame, and not ſucceſſive operations, wherein the gunpowder itſelf ſeems fired by the Electricity: and, indeed, the firſt time this experiment ſucceeded, the flaſh was ſo ſudden and unexpected, that the hand of my aſſiſtant, who touch'd the ſpoon with his finger, was conſiderably ſcorch'd. So that there ſeems a fourth ingredient neceſſary to make gunpowder readily take fire by flame; and that ſuch a one as will heighten the inflammability of the ſulphur.

In common caſes, the lighted match, or the little portion of red-hot glaſs, which falls among the powder, and is the reſult of the colliſion from the flint and ſteel, fires the charcoal and ſulphur, and theſe the nitre. But if to theſe three ingredients you add a fourth, *viz.* a vegetable chemical oil, and gently warm this mixture, the oil, by the warmth, mixes intimately with the ſulphur, lowers it's conſiſtence, and makes it readily take fire by flame.

In theſe operations, notwithſtanding I always made uſe of the fineſt-ſcented oils of Orange-peel, Lemons, and ſuch-like, yet, upon the leaſt warming the mixture, the rank ſmell of balſam (*i. e.* of the ready ſolution of ſulphur) was very obvious.

Further Experiments and Obſervations, by the ſame N° 478. p. 41. Jan. *and* Feb. 1746. *Read* Feb. 6. 1745-6.

8. * As water is a non-electric, and of conſequence a conductor of Electricity, I had reaſon to believe, that ice was endow'd with the ſame properties. Upon making the experiment I found my conjectures not without foundation; for, upon electrifying a piece of ice, where-ever the ice was touch'd by a non-electric, it flaſhed and ſnapped. A piece of ice alſo, held in the hand of an electrify'd man, as in the beforementioned proceſſes, fired warm ſpirit, chemical vegetable oils, camphire, and gunpowder prepared as before. But here great care muſt be taken, that by the warmth of the hand, or of the air in the room, the ice does not melt; if ſo, every drop of water therefrom conſiderably diminiſhes the received Electricity. In order to obviate this, I cauſed my aſſiſtant, while he was electrifying, to be continually wiping the ice dry upon a napkin hung to the buttons of his coat; and this being electrified as well as the ice, prevented any loſs of the force of the Electricity. The experiment will ſucceed likewiſe, if, inſtead of the ice, you electrify the ſpirit, *&c.* and bring the ice not electrified near them. I muſt obſerve, that ice is not ſo ready a conductor of Electricity as water; ſo that I very frequently have been diſappointed in endeavouring with it to fire inflammable ſubſtances, when it has been readily done by a ſword, or the finger of a man.

In my firſt paper I took notice of my having obſerved two different appearances of the fire from electrified ſubſtances; *viz.* thoſe large bright flaſhes, which may be procured from any part of electrified bodies, by bringing a non-electric unexcited near them, and with which we have fired all the inflammable ſubſtances mention'd in the courſe of theſe obſervations; and thoſe, like the firing of wet gunpowder, which are only perceptible at the points or edges of excited non-electrics. Theſe laſt alſo appear different in colour and form, according to the ſubſtances from which they proceed: for, from poliſh'd bodies, as the point of a ſword, a ſilver probe, the points of ſciſſors, and the edges of the ſteel bar made magnetical by the ingenious Dr *Knight*, the electrical fire appears like a pencil of rays, agreeing in colour with the fire from *Boyle*'s *Phoſphorus*; but from unpoliſhed bodies, as the end of a poker, a ruſty nail, or ſuch-like, the rays are much more red. The difference of colour here, I am of opinion, is owing rather to the different reflection of the electrical fire from the ſurface of the body, from which it is emitted, than to any difference in the fire itſelf. Theſe pencils of rays iſſue ſucceſſively as long as the bodies, from which they proceed, are exciting; but they are longer and more brilliant, if you bring any non-electric not excited near them, though it muſt not be cloſe enough to make them ſnap. If you hold your hand at about two or three inches diſtance from theſe points, you not only feel ſucceſſive blaſts of wind from them, but hear alſo a crackling noiſe. Where there are ſeveral points, you obſerve at the ſame time ſeveral pencils of rays.

* This Paper is reprinted, with ſome miſtakes, in N°. 484. p. 695, *& ſeq.*

It

It appears, from experiments, that beſides the ſeveral properties that Electricity is poſſeſs'd of peculiar to itſelf, it has ſome in common with magnetiſm and light.

In common with Magnetiſm, Electricity counteracts, and in light ſub- PROP. I.
ſtances overcomes the force of gravity. Like that extraordinary power likewiſe, it exerts it's force *in vacuo* as powerfully as in open air, and this force is extended to a conſiderable diſtance through various ſubſtances of different textures and denſities.

Gravity is the general endeavour and tendency of bodies towards the COROL.
centre of the earth: this is over-come by the magnet, with regard to iron, and by Electricity, with regard to light ſubſtances, both in it's attraction and repulſion; but I have never been able to diſcern that vortical motion, by which this effect was ſaid to be brought about by the late Dr *Deſaguliers*, and others, having no other conception of it's manner of acting than as rays from a centre, which indeed is confirmed by ſeveral experiments: one of which, very eaſy to be tried, is, that if a ſingle downy ſeed of cotton-graſs is dropped from a man's hand, and in it's fall comes within the attraction of the rubbed tube; the down of this ſeed, which before ſeemed to ſtick together, ſeparates, and forms rays round the centre of the ſeed: or if you faſten many of theſe ſeeds, with mucilage of gum-arabic, round a bit of ſtick, the down of them when electrified, which otherwiſe hangs from the ſtick, is raiſed up, and forms a circular appearance round the ſtick. As theſe light bodies are directed in their motions only by the force impreſſed upon them, and as their appearance is conſtantly *radiatim*, ſuch appearance by no means ſquares with our idea of a *vortex*.

Some have imagined a polarity alſo, when they have obſerved one end of an excited glaſs tube repel light ſubſtances, and the other attract them; but this is a deception, ariſing from the whole length of the tube not being excited, but only ſuch part of it as has been rubb'd; ſo that as much of the tube as is held in the hand remains in an unexcited ſtate, and permits light ſubſtances to lie ſtill thereon, though forcibly repell'd at the other end. This attractive power of Electricity acts not only upon non-electrics, as leaf-gold, ſilver, thread, and ſuch-like, but alſo upon originally-electrics, as ſilk, dry feathers, little pieces of glaſs, and reſin: it attract all bodies, that are not of the ſame ſtandard of Electrity (if I may be allowed the expreſſion), as the excited body from which it proceeds. I found no body, however denſe, whoſe pores are not pervious to Electricity, by a proper management, not even gold itſelf.

In common with light, Electricity pervades glaſs, but ſuffers no refrac- PROP. II.
tion therefrom; I having, from the moſt exact obſervations, found it's direction to be in right lines, and that through glaſſes of different forms, included one within the other, and large ſpaces left between each glaſs.

Corol. This rectilineal direction is obſervable only as far as the Electricity can penetrate through unexcited originally-electrics, and thoſe perfectly dry; nor is it at all material, whether theſe ſubſtances are tranſparent, as glaſs; ſemidiaphanous, as porcelain, or thin cakes of white wax; or quite opaque, as thick woollen cloth, as well as woven ſilk of various colours; it is only neceſſary that they be originally-electrics. But the caſe is widely different with regard to non-electrics; wherein the direction, given to the Electricity by the excited originally-electric, is alter'd, as ſoon as it touches the ſurface of a non-electric, and is propagated with a degree of ſwiftneſs ſcarcely to be meaſured in all poſſible directions to impregnate the whole non-electric maſs in contact with it, or nearly ſo, however different in itſelf; and which muſt of neceſſity be terminated by an originally-electric, before the Electricity exerts the leaſt attraction; and then this power is obſerved firſt at that part of the non-electric the moſt remote from the originally-electric. Thus, for example, by an excited tube held over it, leaf-gold will be attracted through glaſs, cloth, *&c.* held horizontally in the hand of a man ſtanding upon the floor, and this attraction is exerted to a conſiderable diſtance. On the contrary, the rubbed tube will not attract leaf-gold, or other light bodies, however near, through ſilver, tin, the thinneſt board, paper, or any other non-electric, held in the manner beforementioned. But if you rub the paper over with wax melted, and by that means introduce the originally-electric therein, you obſerve the Electricity acts in right lines, and attracts powerfully. And here I muſt beg leave to remind you, not only of the former corollary, but of ſome of the former experiments alſo; by which it appears, that although, to make a non-electric exert any power, we muſt excite the whole maſs thereof, yet we can excite what part, and what only, of an originally-electric we pleaſe. Thus we obſerve, that leaf-gold, and the ſeed of cotton-graſs (which grows upon boggs, and is a very proper ſubject for theſe inquiries), are attracted under a glaſs jar made warm *, and turned bottom upwards, upon which are placed books, and ſeveral other non-electrics; and that the motions of the light bodies underneath correſpond with the motions of the glaſs tube held over them, the Electricity ſeeming inſtantaneouſly to paſs through the books and the glaſs. But this does not happen, till the Electricity has fully impregnated the non-electrics, which lie upon the glaſs; which received Electricity is ſtopped by the glaſs; and then theſe non-electrics dart their power directly through the upper part of the glaſs, after the manner of origi-

* I have conſtantly obſerved, that the electrical attraction through glaſs is much more powerful when the glaſs is made warm, than when cold. This effect may proceed from a twofold cauſe: firſt, warm glaſs does not condenſe the water from the air, which makes the glaſs, as has been before demonſtrated, a conductor of Electricity: ſecondly, as heat enlarges the dimenſions of all known bodies, and, conſequently, cauſes their conſtituent parts to recede from each other, the electrical *effluvia*, paſſing in ſtrait lines, find, probably, a more ready paſſage through their pores.

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nally-electrics. But if the thinneſt non-electric, even the fineſt paper, as I before mentioned, is held in the hand of a man at the ſmalleſt diſtance over the leaf-gold, and the Electricity is not ſtopped, not the leaſt power will be exerted, and the gold will lie ſtill. I muſt here remark likewiſe, that this law of Electricity is ſo conſtant and regular, that I have not found one deviation from it; ſo that even the quickſilver, ſpread thin, as it uſually is at the back of a plate of a looking-glaſs, will prevent the paſſing thro' of the electrical attraction, unleſs ſtopped by an originally-electric. This penetration of the electrical power through originally-electrics is much greater than has hitherto been imagined, and has cauſed the want of ſucceſs to great numbers of experiments. I have been at no ſmall pains to determine, how far this power can penetrate through a dry originally-electric; and have found, by repeated trials, that either in a cake of wax alone, or of wax and reſin mixed, when the Electricity is very powerful, it has paſſed, I ſay, in ſtrait lines through theſe cakes of the thickneſs of 2 inches and $\frac{4}{10}$; but I never could make it act through one of 2 inches and $\frac{8}{10}$; for in this it was perfectly ſtopped. So that the cakes commonly made uſe of to ſtop the Electricity, by being too thin, ſuffer a conſiderable quantity of the electrical power to pervade them, and be loſt in the floor. I make no doubt if the electrical power could be more increaſed, it would penetrate much further through theſe originally-electric bodies.

Electricity, in common with light likewiſe, when it's forces are collected, and a proper direction given thereto, upon a proper object, produces fire and flame. PROP. III.

The fire of Electricity (as I have before obſerved) is extremely delicate; and ſets on fire, as far as I have yet experienced, only inflammable vapours. Nor is this flame at all heightened, by being ſuperinduced upon an iron rod, red-hot with coarſer culinary fire, as in a preceding experiment; nor diminiſhed by being directed upon cold water. However I was deſirous of knowing, if this flame would be affected by a ſtill greater degree of cold; and in order to determine this, I made an artificial cold; by which the mercury, in a very nice Thermometer adjuſted to *Fahrenheit*'s ſcale, was depreſſed in about 4′, from 15° above the freezing point to 30° below it; that is, the mercury fell 45°. From this cold mixture, when electrified, the flaſhes were as powerful, and the ſtroke as ſmart, as from the red-hot iron. I could have made the cold more intenſe, but the above was ſufficient for my purpoſe. This experiment ſeems to indicate, that the fire of Electricity is affected neither by the preſence or abſence of other fire. For as red-hot iron, by Sir *I. Newton*'s ſcale of heat, is fixed at 192°, and as the *ratio* between Sir *Iſaac*'s degrees and *Fahrenheit*'s is as 34 to 180, it neceſſarily follows, that the difference of heat between the hot iron and the cold mixture is 1040°; and nevertheleſs this vaſt difference makes no alteration in the appearance of the electrical flame. We find likewiſe, that as the fire, ariſing from the refraction of the rays of light COROL.

by

by a *Lens*, and brought to a *Focus*, is obſerved, firſt, at ſome ſmall diſtance from their ſurfaces, to ſet on fire combuſtible ſubſtances; the ſame effect, as I have before obſerv'd, is produced in like manner by electrical flame.

I may perhaps be thought too minute in ſome of the before-mentioned particulars; but, in inquiries abſtruſe as theſe are, where we have ſo little *à priori* to direct us, the greateſt attention muſt be had to every circumſtance, if we are truly deſirous of inveſtigating the laws of this ſurpriſing power. For, as has been ſaid upon another occaſion, by my ever honoured friend *Martin Folkes*, Eſq; our moſt worthy *Preſident*, "That Electricity ſeems to furniſh an inexhauſtible fund for "inquiry: and ſure *phænomena* ſo various, and ſo wonderful, can ariſe "only from cauſes very general and extenſive; and ſuch as muſt have "been deſigned by the Almighty AUTHOR of nature for the produc-"tion of very great effects, and ſuch as are of great moment to the "ſyſtem of the univerſe."

§ 1. *A* Sequel *to the Experiments and Obſervations; in a letter to the* Royal Society *from the ſame* N°. 484 p. 704. Oct. &c. 1747. *Read* Oct. 30. 1746.

9. The favourable reception wherewith you honour'd ſome papers I laid before you ſome time ſince, relating to Electricity, emboldens me to trouble you again upon the ſame ſubject: and I am the more encouraged ſo to do, as the progreſs of our diſcoveries therein, both here and abroad, has been ſo rapid; that what, little more than a year ago, we conceived to be the *ne plus ultra* of our inquiries, is now regarded as mere rudiments.

§ 2. It were treſpaſſing too much upon you, to recount the great number of experiments I have made; for which reaſon I ſhall only take notice of ſuch as are either in themſelves ſtriking, or tend to illuſtrate ſome propoſition.

§ 3. At the beginning of laſt ſummer I cauſed a machine to be made for electrical purpoſes; the wheel whereof was four feet in diameter. In the periphery of this wheel were cut four grooves, correſponding with four globes of ten inches diameter, which were diſpoſed vertically at about 3 inches diſtance from each other. One, two, or the whole number of theſe globes might be uſed at pleaſure. They were mounted upon ſpindles of two inches diameter, and their mean motion round their axis was about 1100 times in a minute. As it is next to impoſſible to have theſe globes blown and mounted perfectly true, I order'd the leather cuſhions, with which they were rubb'd, to be ſtuffed with an elaſtic ſubſtance (curled hair) that the globes in their rotations might be as equally rubb'd as poſſible. You might likewiſe cauſe the globes to be rubb'd by the hands of your aſſiſtants; but under a certain treatment (of which hereafter) the cuſhions excite equally ſtrong. The leather cuſhions were now and then rubb'd over with whiting. As a minute detail of the parts of this machine would take up too much of your time, I have herewith laid before you a draught thereof.

I lined

I lined one of theſe globes to a conſiderable thickneſs, with a mix- § 4.
ture of wax and reſin, in order to obſerve whether or no the Electricity would be the ſooner or more ſtrongely excited; but I found no difference in the power of this globe from the others, which were without this treatment.

The power of Electricity is increaſed by the number and ſize of the § 5.
globes to a certain degree; but by no means in proportion to their number and ſize: therefore, as the bodies to be electrified, will contain only a certain quantity of Electricity, of which more largely hereafter; when that quantity is acquired, which is ſooneſt done by a number of globes, the ſurcharge is diſſipated as faſt as it is excited.

After the globes had been a few times uſed, I found myſelf maſter of § 6.
a much greater quantity of electrical power, with much leſs labour to myſelf, than when I uſed only tubes. I could attract and repel light ſubſtances at a much greater diſtance than before; fire ſpirits of wine, camphire, and all other ſubſtances whoſe vapours were inflammable, with great eaſe, and at any diſtance, with non-electrics placed upon originally-electrics: I could fire them, I ſay, at all times; though not equally eaſy, when the weather was moiſt.

I diſcover'd with this machine, and communicated to ſeveral Mem- § 7.
bers of this *Society*, ſeveral of the experiments ſaid to be firſt made by M. *le Monnier* at *Paris*, before the letter communicating them was received by our *Preſident* from thence.

I order'd another machine to be made for a friend of mine, which § 8.
carried a globe of 16 inches diameter. I united the power of this large globe with that of 3 of the others before-mention'd, and found the ſtrokes from the excited non-electrics not increaſed according to my expectation. In two experiments indeed, where the diſſipation of the whole power of theſe globes was viſible as faſt as it was excited, the effect of this additional globe was very conſiderable. The firſt was, when two pewter plates were held, one in the hand of an electrified man, and the other by one ſtanding upon the floor: when theſe plates were brought near each other, the flaſhes of perfectly pure and bright flame were ſo large, and ſucceeded each other ſo faſt, that, when the room was darken'd, I could diſtinctly ſee the faces of 13 people who ſtood round the room. The other was from a piece of large blunt wire hanging to the gun-barrel; from the end of which, when electrified, and any black * non-electric unexcited was brought near, though not near

* In the courſe of theſe obſervations, whenever I mention either originally-electrics or non-electrics, I always underſtand the whole *genus* of each. Thus when I mention a man placed upon originally-electrics, I am indifferent whether he is ſuſpended either in lines of dry ſilk, hair, or wool; or (which is much more convenient) if he ſtands upon glaſs, wax, reſin, pitch, ſulphur, *&c.* or upon different mixtures of theſe, if of a ſufficient thickneſs. As we are now maſters of a greater electrical power than heretofore, I have found the Electricity pervade, tho' in very ſmall quantity, originally-electrics of above four inches diameter.

enough

enough to cauſe a ſnap, a bruſh of blue lambent flame, totally different from the former, was very conſpicuous when the room was dark, of more than an inch long and an inch thick. I mention that what is held near the bottom of the wire ſhould be black, becauſe then you ſee this flame more ſharp. Here the phoſphoreal ſmell might be perceived at a conſiderable diſtance. If the back of your hand was brought ſo near this wire as to occaſion a ſnap, and theſe ſnaps were received for ſome time, you would feel them like ſo many punctures upon your ſkin, occaſioning red ſpots, which have laſted 24 hours.

§ 9. If, when a perſon is electrified, he brings his hand upon the cloaths of one that is not, they both have a ſenſation exactly reſembling that of many pins running into the ſkin, which continues as long as the globes are in motion. This is moſt perceptible when the cloaths are of thin woollen cloth or ſilk, animal ſubſtances; leſs ſo, when of linen or cotton, which are vegetable.

§ 10. If ſome oil of turpentine is ſet on fire in any veſſel held in the hand of an electrified man, the thick ſmoke that ariſes therefrom receiv'd againſt any non-electric of a large ſurface, held in the hand of a ſecond man ſtanding upon an electrical cake; this ſmoke, I ſay, at a foot diſtance from the flame, will carry with it a ſufficient quantity of Electricity for the ſecond man to fire any inflammable vapour. The electrical ſtrokes have been likewiſe perceptible upon the touching the ſecond man, when the non-electric held in his hand has been in the ſmoke of the oil of turpentine between 7 and 8 feet above the flame. Here we find the ſmoke of an originally-electric a conductor of Electricity.

§ 11. Likewiſe if burning ſpirit of wine be ſubſtituted in the place of oil of turpentine, and if the end of an iron rod in the hand of the ſecond man be held at the top of the flame, this ſecond man will kindle other warm ſpirits held near his finger. Here we find that flame conducts the Electricity, and does not perceptibly diminiſh it's force.

§ 12. Theſe two experiments demonſtrate, that the opinion of thoſe is erroneous, who ſuppoſe the electrical *effluvia* to be of a ſulphureous nature; and that theſe themſelves are ſet on fire at the ſnapping obſerv'd, when you bring non-electrics unexcited to thoſe that are. If their opinions were true, the electrical *effluvia* ſhould be deſtroyed by the flame in both the preceding experiments; the contrary of which is obſerved.

§ 13. I now proceed to take notice of that ſurpriſing effect, that extraordinary accumulation of the electrical power in a phial of water, firſt diſcover'd by Profeſſor *Muſſchenbroek*, a man born to penetrate into the deepeſt myſteries of Philoſophy: and I hope I ſhall ſtand excuſed, if I enter into a minute detail of the circumſtances relating thereto. The experiment is, that a phial of water is ſuſpended to a gun-barrel by a wire let down a few inches into the water through the cork; and this gun-barrel, ſuſpended in ſilk lines, is applied ſo near an excited glaſs globe, that ſome metallic fringes inſerted into the gun-barrel touch the globe in motion. Under theſe circumſtances a man graſps the phial with one hand, and touches

the

the gun-barrel with a finger of the other. Upon which he receives a violent ſhock through both his arms, eſpecially at his elbows and wriſts, and acroſs his breaſt. This experiment ſucceeds beſt, *cæteris paribus*,

1. When the air is dry.
2. When the phial containing the water is of the thinneſt glaſs.
3. When the outſide of the phial is perfectly dry.
4. In proportion to the number of points of non-electric contact. Thus if you hold the phial only with your thumb and finger the ſnap is ſmall; larger when you apply another finger, and increaſes in proportion to the graſp of your whole hand.
5. When the water in the phial is heated; which being then warmer than the circumambient air, may not occaſion the condenſing the floating vapour therein upon the ſurface of the glaſs.

From theſe conſiderations it is to be obſerv'd, that this effect ariſes § 14. from electrifying the non-electric water, included in the originally-electric glaſs; ſo that whatever tends to make the outſide of the glaſs non-electric by wetting it, as, a moiſt hand, damp air, or the water from the inſide of the phial, defeats the experiment, by preventing the requiſite accumulation of the electrical power.

That a gun-barrel is abſolutely neceſſary to make this experiment ſuc- § 15. ceed, is imaginary; a ſolid piece of metal of any form is equally uſeful. Nor have I yet found, that the ſtroke is in proportion to the quantity of electrified matter; having obſerved the ſtroke from a ſword as violent as that from a gun-barrel with ſeveral excited iron bars * in contact with it.

I have tried the effect of increaſing the quantity of water in glaſſes § 16. of different ſizes, as high as four gallons, without in the leaſt increaſing the ſtroke. If † filings of iron are ſubſtituted in the room of water, the effect is conſiderably leſſen'd. If mercury, much the ſame as water; the ſtroke is by no means increaſed in proportion to their ſpecific gravities, as might have been imagined ‖.

The phial ſhould not be leſs than can conveniently be graſped. I gene- § 17. rally make uſe of thoſe, which hold ſeven or eight ounces, and fill them about four fifths with water; and the ſtroke from one of theſe, under the ſame circumſtances, is equally ſtrong with that of a *Florence* flaſk held in the hand, which I have ſometimes made uſe of; though

* If of ſix men touching each other, and ſtanding upon originally-electrics, one touches the gun barrel, the whole are electrified; all theſe then muſt be conſider'd, as ſo much excited non-electric matter. From the aggregate of all theſe, not more fire is viſible upon the touch than from either of them ſingly.

† For a further account of the filings of iron, made uſe of in this experiment, ſee Art. 28. § 14. *

‖ In this experiment, and in others, wherein we aſſert, that the ſtroke is not increaſed in proportion to the quantity of electrified matter; it muſt always be underſtood, that the excited non electrics themſelves are touched, without being contained in originally-electrics, as water in the glaſs; for otherwiſe (as will hereafter be ſpecified) the effects of different quantities of matter will be very different.

the glaſs of this laſt is equally thin with that of the phial, and the quantity of water four times as much. That the ſtroke therefore is not as the quantity of water electrified, is evident from this experiment. This fact does not depend upon my judgment alone, but likewiſe upon the opinions of ſeveral learned Members of this *Society*, who have experienced the greater and leſs quantity of water.

§ 18. If a dry twig of birch, or any other wood, be run through the cork inſtead of the metallic wire, the ſtroke is not greater than is uſually felt from the gun-barrel without the application of the water. The ſtroke is likewiſe leſſen'd, if the phial is held in the hand with a glove on.

§ 19. After the gun-barrel and phial have been ſufficiently excited, which is done in a few ſeconds, the ſurcharge is diſſipated; ſo that the continuing the motion of the machine ever ſo long after the ſaturation is complete, does not increaſe the electrical force.

§ 20. The force of the ſtroke from the electrified phial does not increaſe in proportion to the dimenſions of the glaſs, or the number of globes employed. I have been ſtruck as forcibly with one phial from a globe of 7 inches diameter, as when I made uſe of, at the ſame time, one of 16 inches, and 3 of ten. I have been lately informed, that at *Hamburgh* a ſphere was employed for this purpoſe a *Flemiſh* ell in diameter, without the expected increaſe of power.

§ 21. When the phial is well electrified, and you apply your hand thereto, you ſee the fire flaſhes from the outſide of the glaſs wherever you touch it, and crackles in your hand.

§ 22. The phial may be electrified by applying the wire therein to the globe in motion; after which, if it is graſped in one hand, and the wire touched with a finger of the other, the ſtroke is as great as from the gun-barrel. If you only bring your finger near the end of the wire without touching it, you obſerve the ſame bruſh of blue flame, as from the wire hanging to the gun-barrel, before taken notice of. This inſtantly diſappears upon touching the wire, though you do not receive a ſhock, unleſs at the ſame time you graſp the phial.

§ 23. If you graſp the phial with your hand, and do not at the ſame time touch the wire, the acquired Electricity of the water is not diminiſhed. So that, unleſs by accident or otherwiſe the wire is touched, the electrified water will contain it's force many hours, may be conveyed ſeveral miles, and afterwards exert it's force upon touching the wire.

§ 24. If, when the machine is in motion, the phial is hung upon the gun-barrel, no increaſe of the ſtroke is perceived upon touching the gun-barrel with your finger, unleſs at the ſame time the phial is taken in the hand.

§ 25. If, when the gun-barrel and phial are excited, you graſp the phial with one hand, and touch the gun-barrel with a piece of any metal held in the other, the ſhock is as great in your arms as though you touched the gun-barrel with your finger; but not the leaſt ſhock is felt, if, inſtead of metal, you touch the gun-barrel with a piece of dry wood.

I have

I have felt a very great ſtroke, when I hung two phials to the gun- § 26.
barrel, and, graſping them both, brought my forehead near it. The ſhock then was ſo violent, that I ſeem'd ſtunn'd, as though ſtruck on the head with a great ſtick, and I have never ſince choſen to repeat this experiment. This increaſe of the electrical force was owing to the additional phial, whereby the points of non-electric contact were augmented.

Likewiſe if a perſon placed upon originally-electrics, graſps two § 27.
phials, as before-mentioned, and a ſecond perſon, ſtanding upon the floor, touches any part of his body, a very ſlight ſtroke only is perceived. But if the ſecond perſon, while the globes are in motion, places one of his fingers upon the hand, or any part of the naked body of the firſt, and at the ſame time touches the gun-barrel with his other hand; both feel a ſhock equal to that juſt now mention'd, but more tolerable, becauſe not felt in the head, in the arms only, and acroſs the breaſt. In this experiment, it is not neceſſary that the outſide of the glaſſes held in the hands ſhould be dry, as in the former experiments; becauſe whatever by the moiſture is communicated to the man, is ſtopped by the originally-electrics upon which he is placed. If, inſtead of his hand, you gently touch the firſt perſon's cloaths, you only perceive a ſmall ſtroke upon your finger; but if you preſs his cloaths cloſe to his body, you frequently perceive a double ſtroke; the one, ſlight from his cloaths; the ſecond, a violent ſhock from his body.

Upon ſhewing ſome experiments to Dr *Bevis*, to prove my aſſertion § 28.
that the ſtroke was, *cæteris paribus*, as the points of contact of non-electrics to the glaſs, that ingenious Gentleman has very clearly demonſtrated it likewiſe by the following experiment: he wrapped up two large round-bellied phials in very thin lead ſo cloſe as to touch the glaſſes every-where, except their necks. Theſe were filled with water, and cork'd, with a ſtaple of ſmall wire running through each cork into the water. A piece of ſtrong wire about 5 inches long, with an eye at each end, was provided, and at each end of this hung one of the phials of water by the ſmall ſtaple running through the cork. A ſmall wire loop then was faſten'd into the lead at the bottom of each phial, and into theſe loops was inſerted a piece of ſtrong wire like the former. If then theſe phials were hung acroſs the gun-barrel and electrified, and a perſon ſtanding upon the floor touched the bottom wire with one hand, and the gun-barrel with the other, he received a moſt violent ſhock through both his arms, and acroſs his breaſt.

Theſe phials may be concealed, and the ſhock be more univerſal, in § 29.
the following manner: the phials may be placed in a corner of the room, and any thing laid over them, ſo as not to touch the upper wires; then a very fine wire muſt be ſuſpended to the gun-barrel, and faſten'd to the upper ſtrong wire. A ſecond piece of ſmall wire, of a ſufficient length to reach from the phials almoſt under the gun-barrel, muſt be faſtened to the lower ſtrong wire, and this may be conceal'd under a floor-

cloth. The phials then are electrified; and if a perſon, placing his foot upon the floor-cloth over the wire which comes from the bottom of the phials, touches the gun-barrel, he receives a moſt terrible ſhock. The firſt time I experienced it, was when the phials were fully electrified, and both my feet were placed upon the wire. Upon receiving the ſtroke from the gun-barrel upon my finger, it ſeemed to me, uſed as I am to theſe trials, as though my arm were ſtruck off at my ſhoulder, elbow, and wriſt; and both my legs, at the knees, and behind near the ankles. So that, to try the effects of this experiment, you muſt be careful of not electrifying the phials too much. If a dozen or more of theſe phials, or one very large bottle, were cover'd over with thin lead in the above manner, and ſtrongly electrified, and this Electricity were diſcharged by a man at once in the manner here mention'd, I ſhould dread the conſequences.

§ 30. We muſt obſerve, that this ſhock is not felt, unleſs the wire, coming from the bottoms of the bottles, is touched; and then not, if the ſhoes are dry, and of conſequence originally-electric. In this experiment we ſee the effects of the increaſe of the points of contact; and it ſeems the more ſurpriſing to thoſe who are not acquainted with the cauſe, when the wire is concealed under a floor-cloth, that the moving of their feet only one inch, ſhould occaſion them, all other circumſtances apparently the ſame, to feel a violent ſhock, or none at all. A thick carpet, inſtead of a floor-cloth, is liable to prevent the ſucceſs of this experiment, for the ſame reaſon as dry ſhoes. This experiment may aptly enough be called, the ſpringing an electrical mine.

§ 31. If, in the former experiment, the lower ſmall wire is faſten'd to an iron rod; and if, when the phials are ever ſo ſtrongly excited, that rod is held in the hand of a man ſtanding upon the floor, and with it he touches the gun-barrel, he perceives no ſhock, for reaſons preſently to be aſſigned. But if he takes this iron rod in one hand, and touches the gun-barrel with the other, he then is violently ſtruck. We muſt here obſerve, that the violence of the ſtroke is always felt in our bodies, in proportion to the loudneſs of the exploſion, and the quantity of fire ſeen. Therefore, as both theſe are equally perceptible, whether the Electricity paſſes only thro' the iron, as in the firſt of theſe inſtances, or thro' our bodies equally with the iron, as in the ſecond; we conclude, that in both there is the ſame degree of electrical force. By the firſt of theſe methods you are capable of making others ſenſible of the electrical force, without feeling it yourſelf. This experiment, as well as the laſt, will admit of infinite variation.

§ 32. If a man, ſtanding upon an electrical cake, takes the phial ſuſpended to the gun-barrel in his hand, by theſe means he acquires ſome electrical power; for if, under theſe circumſtances, he touches the gun-barrel, he only receives a ſlight ſtroke. If then, without having had any communication with unexcited non-electrics, he touches the gun-barrel again, the globes being yet in motion, he receives no ſtroke at all.

If

If to the gun-barrel an egg, either raw or boiled, is ſuſpended by a piece of wire, and a perſon, graſping the electrified phial in one hand, brings the palm of his other near the bottom of the egg; at that inſtant he receives a ſmart ſtroke, and his hand ſeems full of a more red fire than is uſually obſerved. In this experiment the ſtroke is more confined to the hand without ſhocking the arms, than when you touch the gun-barrel itſelf; it more reſembles a ſtroke over the hand with a *ferula*. § 33.

If any number of people ſtand upon originally-electrics, and communicate with each other by any non-electric medium, eſpecially metal, they are by theſe means all equally electrified; and if a perſon ſtanding upon the floor, and holding the phial of water hanging to the gun-barrel in his hand, touches the perſon furtheſt from the gun-barrel, the whole number receives a ſhock equal to any one touching the gun-barrel ſingly. § 34.

If a number of perſons, how great ſoever, ſtand upon the ground, communicating with each other as before, the firſt of which graſps the phial, and the laſt touches the gun-barrel, the whole number receive a ſhock like the former. This, we are inform'd, M. *le Monnier* at *Paris* communicated through a line of men, and other non-electrics, meaſuring nine hundred toiſes. § 35.

Several experiments ſhew, that the electrical force always deſcribes a circuit; *e. g.* if a man holds the electrified phial in one hand, and touches the gun-barrel with the other, he feels the ſhock in no other parts of his body than in his arms, and acroſs his breaſt. So that here we ſee the electrical power darts *rectiſſimo curſu* between the gun-barrel and phial. This is more particularly demonſtrated by the following experiment, in which, though the two lines of perſons may be of any length, we only ſpecify, that each conſiſts of 4, for the ſake of perſpicuity. § 36.

Of one line, let *A* touch the gun-barrel, ſtanding upon wax, and communicate with *BCD* likewiſe ſtanding upon wax. Of the other line, let 1 take the electrified phial in his hand, and join with 2, 3, and 4, all ſtanding upon the floor. If, under theſe circumſtances, the firſt line is electrified, and 4 touches *D*, all eight are ſtruck through. If 4 touches *C*, *D*, though electrified, feels nothing, and the remaining ſeven are ſtruck; ſo that here *D* is left out of the circuit. If 4 touches *B*, only ſix feel the ſhock, and *C* and *D* feel nothing; and thus you may proceed to *A*, who muſt always neceſſarily feel, if either himſelf or any of his line is touched. If, when both lines are as before-mentioned, *D* touches 3, 4 is left out of the circuit, and the remaining ſeven feel the ſtroke. If *C* touches 2, the circuit conſiſts of five, *D*, 3, and 4 being, though under the ſame circumſtances, left out: always obſerving, however theſe circuits are diverſified, that *A*, who touches the gun-barrel, and 1, who holds the phial, are certain to feel the ſtroke. § 37. Fig. 18.

This experiment may be reverſed, the lines being as before, in the following manner, wherein likewiſe this circuit is always obſervable. Let *A* § 38.

touch

touch the gun-barrel as before, and *D* hold the wire of the electrified phial in his finger. Let 4 graſp the phial, and 1 touch *B*; then *A* feels nothing, being left out of the circuit, and the other ſeven are ſtruck. If 4 touches *C*, then *A* and *B* feel nothing, the circuit conſiſting of the remaining ſix. But it is to be obſerved, as in the former experiment, that 4, who graſps the phial, and *D*, who holds the wire, muſt of neceſſity be always in the circuit. I have been the more particular in this matter, as it demonſtrates the courſe of the electrical power to be in the moſt direct manner between the gun-barrel and the electrified phial.

§ 39. Likewiſe, if a perſon, ſtanding upon an originally-electric, touches the gun-barrel with his right hand, a piece of wire being placed round his left leg, and a ſecond perſon, ſtanding likewiſe upon the wax, takes hold of the extremity of this wire; then let another perſon, ſtanding upon the floor, and graſping the electrified phial, touch any part of the ſecond perſon's body. Upon this touch, the ſecond perſon is ſhook as uſual; but the firſt feels the ſtroke only in his left leg and right arm, the neareſt courſe of the electrical power.

§ 40. If any number of perſons communicate by pieces of wire, and if any one of them brings together the ends of the two pieces of wire in his hands, upon the gun-barrel's being touch'd, he will perceive no ſtroke. But if the ends of the wires are but a $\frac{1}{4}$ of an inch aſunder, he will be ſhaken in both his arms; becauſe then his body will become part of the circuit.

§ 41. If, when any number of perſons join hands, or communicate by any metallic medium ſtanding on the floor, one graſps the phial, and joins with the reſt; upon the gun-barrel's being touch'd by the laſt perſon of the line, the whole number are ſtruck, and he who graſps the phial, as forcibly as the reſt. But if two phials are employed, and he graſps them both, with a piece of wire of ſufficient length held between his fingers, which wire touches both phials, and it's end is taken hold of by the ſecond perſon of the line; if then the laſt perſon touches the excited gun-barrel, all in the line are violently ſtruck, except the perſon who graſps the phials; but he feels little or nothing of the ſtroke.

§ 42. The ſtroke is very violent, when a wire is put round the naked head, or under the peruke, and the perſon graſping the phial touches the gun-barrel with the ends of the wire, or if he holds the wire between his teeth.

§ 43. If a perſon, ſtanding on the electrical cakes with gold or ſilver lace upon his coat, takes hold of the gun-barrel, and another perſon graſping the electrified phial touches the bottom of the lace, the perſon electrified, if he holds down his head, feels the blow under his chin. The lace in this inſtance has the ſame effects as a piece of metal; at the end of which, if placed in the ſame manner, you would neceſſarily feel the ſtroke.

44. I now proceed to ſhew, by what ſteps, in my inquiries into the nature of Electricity, I diſcover'd that the glaſs tubes and globes had not the electrical

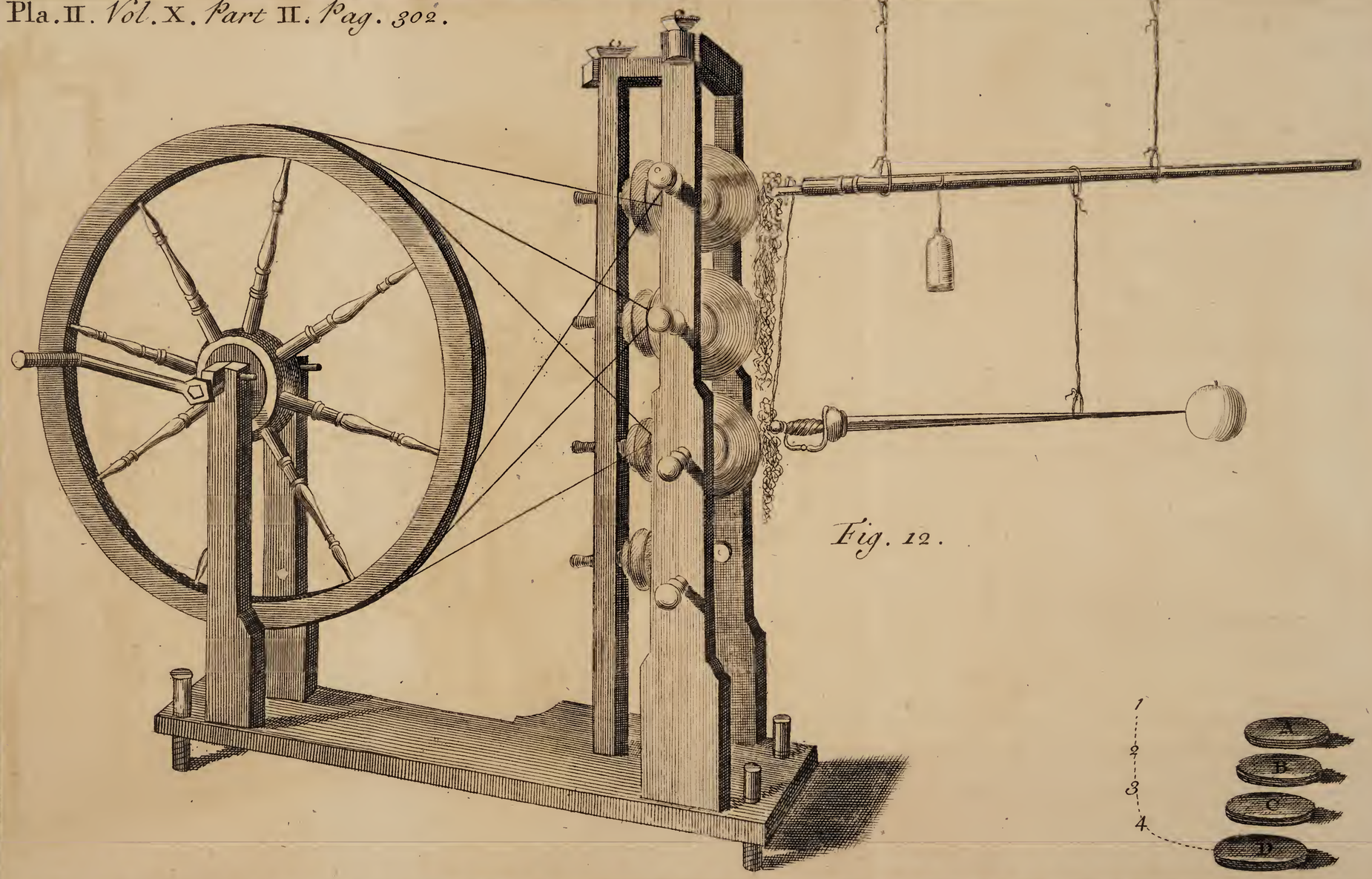


Fig. 12.

electrical power in themſelves, but only ſerved as the firſt movers and determiners of that power.

Several months ſince, I obſerv'd that, by rubbing a glaſs tube, while § 45.
ſtanding upon a cake of wax, in order, as I expected, to prevent any of the electrical power from diſcharging itſelf through me into the floor; contrary to my expectation, that power was ſo much leſſen'd, that no ſnapping was to be obſerv'd upon another's touching any part of my body. But if a perſon not electrified held his hand near the tube whilſt it was rubbing, the ſnapping was very ſenſible. This I ſhew'd to ſeveral Members of the *Royal Society*, and others, who did me the honour to viſit me. Afterwards I met with an experiment of the ſame kind, in a treatiſe publiſh'd by Profeſſor *Boſe*, intitled, *Recherches ſur la cauſe et ſur la veritable theorie de l'Electricité*, which that ingenious gentleman ſays, had given him great trouble by it's oddneſs. The experiment is, that, if the electrical machine is placed upon originally-electrics, the man who rubs the globes with his hands, even under theſe apparently favourable circumſtances, gives no ſign of being electrified, when touched by an unexcited non-electric. But if another perſon, ſtanding upon the floor, does but touch the globe in motion with the end of one of his fingers, or any other non-electric, the perſon rubbing is inſtantly electrified, and that very ſtrongly. The ſolution of this *phænomenon*, ſeemingly contrary to the already diſcover'd laws of Electricity, had terribly tormented him; but however he has given us the following, which he modeſtly calls a plauſible ſubterfuge rather than a ſolution; *viz.* that a power cannot act at the ſame time with all it's vigour, when one part of it is already employed; as a horſe, who already draws an hundred pounds, cannot draw an additional weight as freely as if he had not been loaded at all. That the hand excites the virtue already in the ſphere; therefore if the ſame power impregnates the man, there remains none for the globe. That the virtue of the globe then cannot be communicated at the ſame time to the man, by whom it is created. That he, who gives it, cannot receive it himſelf. From theſe, and ſuch-like conſiderations, it appears to him, that the man upon the ground, who holds his fingers to the globe in motion, inſtead of his diminiſhing it's electrical force, throws that force back again over the man, who excited it. That the finger in this caſe ſeems to operate as an electric *per ſe*, and drives back the electrical power.

I have ſeen an account of * Mr *Allamand*, lately printed at the *Hague*; § 46.
wherein he takes notice of this *phænomenon*. He tells us, that as part of the electrical power of the globe paſſes off by the frame, upon which the globes are mounted, into the floor, and diſſipated thereby; he conceived, that if the machine, and the man who rubb'd the globe, were placed upon pitch, to prevent this diſſipation, the fire of Electricity would be more ſtrong. But the conſequence is extremely odd and unexpected; for the contrary happens; and the electrical power is conſiderably diminiſhed, and ſometimes there is even none at all.

* *Bibliotheque Britannique pour les mois de Janvier, Fevrier, et Mars*, 1747.

I tried

§ 47. I tried this experiment ſeveral times with my machine, and the man, who turns the wheel thereof, mounted upon the electrical cakes. If the air was dry, and the machine placed at ſome diſtance from non-electrical ſubſtances, as the ſides of the room, chairs, and ſuch-like; after one or two ſmall ſnaps, the gun-barrel, ſupported by ſilk lines, and hanging in contact with the globes, would, tho' the machine were in motion a conſiderable time, attract no light ſubſtances, nor emit any fire. This induced me to conceive, that the electrical power was not inherent in the glaſs, but came from the floor of the room; and if the fact were ſo, the gun-barrel ſhould ſnap upon my touching any part of the machine. The conſequence fully anſwer'd my conjectures; for while I ſtood upon the floor, the globes ſtill in motion, I put one hand upon the frame of the machine, and touched the gun-barrel with one of the fingers of my other. Upon this, fire iſſued, and the ſnapping continued as long as I held my hand upon the machine, but ceaſed upon taking it off. This at once proved to me, that the electrical fire paſſed from the floor thro' my body to the machine. I then order'd the man to put one of his feet from the wax upon the floor; which, as ſoon as he complied with, cauſed the Electricity to ſnap at the gun-barrel, and this ceaſed upon his replacing his foot. Here I found, that the electrical power came through the man; and that, in theſe inſtances, either myſelf, or the man who touched the floor with his foot, was to be regarded as an additional part of the machine communicating with the floor. Theſe conſiderations led me to make the following experiments.

§ 48. If my conjectures were well founded, and if the electrical power, the man and the machine being placed upon originally-electrics, went through my body to the machine, a fine wire, held in my hand at a few inches diſtance, ought to be attracted by any part of the machine. This ſucceeded accordingly, but the attraction laſted a very ſmall ſpace of time, and the wire again hung perpendicularly from my finger, though the globes continued in motion. This induced me to believe, that the gun-barrel, and the other non-electrics ſuſpended in contact with the globes, would only contain a certain quantity of the electrical æther; and if this were the caſe, the attraction of the wire to the machine would be continual, if the electrical power found again a communication with the floor, as the wire was the only canal of communication between the floor and the machine. Whereupon I placed one of my fingers upon the gun-barrel, and held a wire near the machine with my other hand, and found, that as long as my finger continued upon the gun-barrel, the wire was attracted, but no longer.

§ 49. Here we find, that one cauſe of the electrical attraction is the current of the electrical æther ſetting to the machine through the wire; and this current is ſtopped from two cauſes; one, when there is no diſcharge thereof from the gun-barrel, the accumulation being complete; the other, when other currents are opened, that is, when the machine is touched in other parts.

In

In theſe, and the ſubſequent experiments, I always ſuppoſe the air very dry; for if it is not, and the ſilk lines, which ſupport the non-electrics, are wetted thereby, the electrical power will be diſcharged along them, and the wire will be conſtantly attracted, as I have frequently on purpoſe experienced; and this diſcharge is in proportion as the lines are more or leſs wetted. § 50.

If a man ſtands upon the machine placed upon originally-electrics, and the gun-barrel with the other non-electrics are ſuſpended as uſual in contact with the globes, no Electricity is obſerved in that man: but if a wire, hanging to the wainſcot of the room, touches the gun-barrel, or a man ſtanding upon the floor applies his finger thereto, the man upon the machine emits fire copiouſly; and either himſelf, or the man who turns the wheel of the machine, fires inflammable ſubſtances. But this effect is no longer obſervable, when the wire, *&c.* are removed from touching the gun-barrel. So that, in this experiment, the uſual courſe of the Electricity is inverted; and that power, which, in moſt other inſtances, is brought by the wood-work of the machine to the globes, and by them diſcharged upon the gun-barrel, is now brought by the wire to the gun-barrel, and from this the globes throw it all over, not only the machine, but any non electric in contact with it, if the Electricity is ſtopped. In this experiment, if an iron rod, ſtanding upon the floor, is inclined againſt the loops of the ſilk lines which ſupport the gun-barrel, in ſuch a manner as not to touch the gun-barrel, the electrical fire, which paſſes from the iron rod to the gun-barrel, inſtead of being ſupplied conſtantly, comes in by ſnapping ſo long as any unexcited non-electric communicates with the machine, but ceaſes upon it's being removed: and if the air is very dry, and none of the Electricity conducted down the ſilk lines, the ſnapping from the iron rod to the gun-barrel will frequently correſpond to the touching of the wooden machine with your fingers, and ſtop upon your taking them off. And this experiment will look much like magic, even to thoſe who are acquainted with the operations of Electricity; for if the perſon who turns the wheel of the machine, and ſtands upon the cakes, be properly inſtructed: upon your bidding the gun-barrel ſnap, he only puts the toe of his ſhoe upon the floor, and it ſnaps immediately, and continues ſnapping as long as he keeps it there; but if you order it to ceaſe ſnapping, he almoſt imperceptibly replaces his foot upon the cakes, and it ceaſes. This may be repeated as often and as long as you pleaſe. § 51.

Many experiments demonſtrate, that if the Electricity is not ſtopt, no ſign of it's preſence, either by fire or attraction, is obſervable in the non-electric bodies ſuſpended to the globes: that is, although ever ſo great a quantity be determined by the globes over theſe bodies, the Electricity paſſes off from them *pleno rivo* to the floor, from whence it came: but if the Electricity is ſtopt, it is then accumulated upon theſe non-electrics; but this can be done only to a certain degree, as is manifeſt from a former experiment. And if, when this power is accumulated, § 52.

ed, a man ſtanding upon the floor touches now-and-then the non-electrics with his finger, the Electricity, which is here accumulated, ſnaps, and the fire is always obſervable. But this ſnapping is not, when the electrical power paſſes off continually, as from a piece of blunt wire hung to the ſuſpended gun-barrel, and the hand of a man brought near it without touching; whereby the electrical power becomes viſible, like a fine blue cone of flame, with it's point towards the wire. When the hand is placed at a proper diſtance, the blaſt, like that of cold air, is therefrom very manifeſt. If you do not determine the Electricity by theſe means to a point, the diſſipation of it is general, and from all parts of the excited non-electric; but if you do, by bringing your hand near the wire as before-mentioned, you ſee the manner of it's being diſcharged into the floor, and ſo into the earth. Theſe facts being ſo, if my conceptions are true, that the glaſs globes circulate the electrical fire, which they receive from their friction againſt the cuſhions, or the hand of a man, and which is conſtantly ſupplied to theſe laſt from the floor; the ingreſs of the electrical fire, if the machine, *&c.* are placed upon electrics *per ſe*, ought to be viſible, as well as the egreſs under the ſame circumſtances; and this is demonſtrated by experiment. For if, while any unexcited non-electrics touch the gun-barrel, the globes being in motion, you bring your finger, or a piece of wire near any part of the wood-work of the machine, but more eſpecially the iron axis of the wheel; you obſerve the bruſh of blue flame ſet in from it to the wood-work. We always obſerve, in this experiment, that the lambent flame from the end of the wire paſſes diverging into the machine, and this continues ſo long as the gun-barrel is touch'd. So that here the office of the globes exactly tallies with that of the heart in animals; which, as long as the quantity of blood is ſupplied, propels it into the arteries, and theſe all over the ſyſtem; or that of the pump in hydroſtatics. In the ſame manner, by the attrition of glaſs tubes, the electrical power is brought from the body of the man who rubs the tube; and he is conſtantly taking in a ſupply from the floor.

§ 53. What I here call the electrical æther, is that atmoſphere which ſurrounds both excited originally-electrics, and excited non-electrics. That this is extended to a conſiderable diſtance, appears, from a fine thread, or piece of cotton-graſs ſeed, being attracted at ſome diſtance from them, as far as which, it is preſumed, this atmoſphere extends. Here indeed it is only perceived by it's effects upon theſe light ſubſtances: but at the bruſh of flame from the end of the wire before-mention'd, from ſome bran lying upon a flat piece of metal in contact with excited non-electrics, your hand being held over it, and in many other experiments, it becomes manifeſt to your feeling as a blaſt of cold wind. You feel it likewiſe in a leſs degree, when a glaſs tube is well excited, and brought near your face. If no unexcited non-electric is near, this atmoſphere ſeems to be determined equally over all the excited non-electrics in contact with the machine; but if a non-electric unexcited is brought near, the

the greateſt part of it is determin'd that way; and hereby the attraction at any other part of theſe excited non-electrics is conſiderably diminiſhed. Hence the cauſe of the repulſion of Electricity, which does not operate, until the electrical æther is ſufficiently accumulated. This electrical repulſion is ſtrongeſt in thoſe parts of the excited non-electrics, where unexcited non-electrics are brought near them; for by theſe the electrical blaſt, which otherewiſe is general, is particularly determined to the floor.

Before I proceed further, I muſt beg leave to explain what I call the accumulation of Electricity. To put a ſimilar caſe: as we take it for granted, that there is always a determinate quantity of atmoſphere ſurrounding the terraqueous globe, we conceive, when we ſee the mercury in the barometer very low, that there then is a leſs accumulated column of this atmoſphere impending over us, than when we ſee the mercury high. In like manner, when we obſerve that the electrified gun-barrel attracts or repels only very light ſubſtances at a very ſmall diſtance, or that the ſnap and fire therefrom are ſcarcely perceptible; we conceive then a much leſs quantity of electrical atmoſphere ſurrounding the gun-barrel. This power being more or leſs, we call the greater or leſs degree of the accumulation of Electricity. This is only attainable to a certain point, if you electrify ever ſo long; after which, unleſs, otherwiſe directed, the diſſipation thereof is general. The phial of water of *Muſſchenbroek* ſeems capable of a greater degree of accumulation of Electricity, than any thing we are at preſent acquainted with: and we ſee, when, by holding the wire thereof to the globe in motion, the accumulation being complete, that the ſurcharge runs off from the point of the wire, as a bruſh of blue flame. A method has been diſcover'd here by Mr *Canton*, by which the quantity of accumulated Electricity may be meaſured to great exactneſs. The manner of meaſuring is this: when the phial is ſufficiently electrified by applying the wire thereof to the glaſs globe, and which is known by the appearance of the bruſh of flame at the end of the wire, as before-mention'd; hang a ſlender piece of wire to the ſuſpended gun-barrel for this purpoſe detached from the globes. Upon your applying the wire of the electrified phial to that hanging to the gun-barrel, you perceive a ſmall ſnap; this you diſcharge by touching the gun-barrel with your finger, which likewiſe ſnaps: and thus alternately electrifying and diſcharging, you proceed until the whole Electricity of the water is diſſipated; which ſometimes is not done, under 100 diſcharges. If you do not diſcharge the Electricity every time, the ſnaps from the wire of the electrified phial to the gun-barrel are ſcarcely perceptible. In proportion to the number of ſtrokes, you eſtimate the quantity of the acquired Electricity of the water. That you could, by ſtopping the Electricity, excite non-electrics; and, by accumulating their power, make them exert more force than originally-electrics would at any point of time, was that capital diſcovery of the late Mr *Gray*; and is to be regarded as the baſis, upon which all the preſent improvements

§ 54.

of our knowledge in Electricity are founded; and till which diſcovery, although ſome of the effects of Electricity were obſerved above two thouſand years ago *, little progreſs was made.

§ 55. The electrical æther is much more ſubtil than common air, and paſſes to a certain depth through all known bodies. It paſſes moſt readily through metals, water, and all fluids, except reſinous ones; then animal bodies dead or alive, in proportion as they are more or leſs wet; then ſtones, wood, and earths. It paſſes to a certain thickneſs only thro' reſins, dry animal ſubſtances, wax, and glaſs. For this reaſon bodies are called electrics *per ſe*, or non-electrics; not only for their rubbing the Electricity from other bodies, but likewiſe as they permit more or leſs of the electrical æther to paſs through them. This æther has not only the property with air of moving light ſubſtances; but it ſeems to have another, and that is elaſticity.

§ 56. That this fluid is more ſubtil than common air, is more particularly demonſtrated by it's paſſing through ſeveral glaſſes at the ſame time; through any one of which, though ever ſo thin, air cannot paſs. It likewiſe paſſes, as I have mention'd before, through all known bodies, except originally-electrics, and even through theſe to a certain degree. It's elaſticity is proved by it's extending itſelf round excited electrics, and excited non-electrics, to a conſiderable diſtance; as well as by it's increaſing the motion of fluids. This is demonſtrated by the experiment with a ſmall glaſs ſiphon, where the elaſticity of the electrical æther overcomes the attraction of coheſion: I have frequently obſerved this experiment does not operate, unleſs the greateſt part, if not the whole electrical blaſt, is determined to the floor through the water, by bringing ſome unexcited non-electric near the long leg of the ſiphon †. The ſtream through this ſlender tube is moſt complete, when the non-electric is brought near, ſo as when the room is ſomewhat darkened, the ſtream of water appears as a ſtream of blue flame, much like that from the blunt wire. This ſtream is ſtopped, either by touching any part of the non-electrics in contact with the globes; by placing the machine and the man who turns the wheel upon electrics *per ſe*, by which the current of the electrical æther from the floor to the machine is prevented; or by removing the non-electric from the leg of the ſiphon, by which the diſſipation of the electrical æther from the excited non-electric becomes

* *Theophraſtus*, who lived above 300 years before the date of the Chriſtian Æra, takes notice of amber and the *Lyncurium*, attracting not only ſtraws, and ſhavings of wood, but alſo thin pieces of copper and iron. See *Theophraſtus* περὶ τῶν λίθων· ν'.

—Καὶ τὸ λυγκούριον—ἕλκει γὰρ ὥσπερ τὸ ἤλεκτρον. Οἱ δέ φασιν οὐ μόνον κάρφη καὶ ξύλον, ἀλλὰ χαλκὸν καὶ σίδηρον, ἐὰν ᾖ λεπτός· ὥσπερ καὶ Διοκλῆς ἔλεγεν. See *p.* 74. in the late Edit. by *J. Hill.*

† There is one inſtance, where the water will run off in a full ſtream without bringing a non-electric unexcited near the long leg of the ſiphon; and that is, by ſuſpending a phial of water, as uſual to the gun-barrel by a wire, and by letting a glaſs ſiphon through the cork into the water. When this phial is ſufficiently electrified, the water therein runs off in a full ſtream, though no non-electric unexcited is near; becauſe then the current of water through the ſiphon is the only way, by which the ſurcharge of the Electricity can be diſſipated.

general,

general. So that we find, that although we can repel light bodies from many parts of excited non-electrics at the ſame time; the whole force of the electrical current is neceſſary, to drive off ſo ponderous a fluid as water. May we likewiſe not infer the elaſticity of electrical æther, from the ingreſs of the blue flame from the end of a blunt wire held near the axis of the wheel, or any part of the wood-work of the machine, after the revolutions of the globes are ceaſed? Certainly we ſee an influx of electrical fire to all bodies, until their determined quantity is reſtored. Is not the elaſticity of this æther deducible likewiſe from the violent ſhock we feel in our bodies in the experiments with water?

There ſeems to be a quantity of this æther in all bodies. Hence the § 57.
reaſon why, though the machine is placed upon electrics *per ſe*, a ſnap or two, as I mention'd before, is obſerv'd upon touching the gun-barrel, when the machine has been ſome time in motion: but after theſe no more is perceiv'd, if the ſilk lines are very dry, and the electrical ſupporters of the machine are of a requiſite thickneſs. As ſoon as any non-eletcric unexcited touches the machine, this loſs is immediately reſtored. As the electrical æther, as has been ſpecified, is an elaſtic fluid, wherever there is an accumulation thereof, there is an endeavour by the neareſt unexcited non-electric to reſtore the *equilibrium*. The reſtoring of this *equilibrium* I take to be the cauſe of the attraction of excited glaſs tubes and globes, as well as that of excited non-electrics; for here the blaſt of electrical æther conſtantly ſets in from the neareſt unexcited non-electrics towards thoſe excited, and carries with it whatever light bodies lie in it's courſe. This ſetting in of the current of electrical æther towards excited non-electrics is likewiſe very perceptible to your feeling as a blaſt of cold wind; if when you are electrified, you hold your hand over a plate with ſome bran in it, by which blaſt the bran is carried againſt your hand. Theſe light ſubſtances are again repell'd by the blaſt from the excited bodies, as ſoon as they come in contact, and ſometimes before. The ſucceſſions of theſe alternate attractions and repulſions are extremely quick, ſo that ſometimes your eye can hardly keep pace with them. And if you put a glaſs globe of about an inch in diameter very light and finely blown into a plate of metal, and hang another plate over it; electrify the upper one, and bring the other under it, and you will find the ſtrokes from the alternate attractions and repulſions * almoſt too quick for your ear. I have ſeen a *German*, who travelled with a ſmall electrifying machine, who, by a proceſs of this ſort, made two ſmall bells ring. One of the bells was ſuſpended to an electrified wire, which was conducted without touching along the ſides of the room; at about an inch diſtance, detached from this wire, a little clapper was hung by a ſilk line; at an equal diſtance from this laſt was

* The following is an argument of the velocity likewiſe, with which theſe little globes are attracted and repelled. If they are let fall from the height of ſix feet or more upon a wooden floor, or a plate of metal, they are rarely broke; but by the attractions and repulſions of them between the plates, though at the diſtance only of $\frac{1}{6}$ of an inch, they are frequently beat in pieces.

hung

hung another little bell, which communicated with the ſides of the room. As ſoon as the machine was in motion, the electrified bell attracted the clapper, which immediately by the repulſive blaſt was blown off to the unexcited bell. By the time the ſecond bell was ſtruck, the former attracted again; and this jingling of the two bells continued not only during the motion of the machine, but ſeveral ſeconds after it was ſtopped. This was occaſioned by the ſmall volume of the clapper being able to convey away only a ſmall quantity of the electrical æther at each ſtroke; by which it was ſome time before the *equilibrium* was reſtored.

§ 58. To demonſtrate likewiſe, that the reſtoring this *equilibrium* is not imaginary, I ſhall mention an experiment of Mr *Wilſon*, who has taken great pains in theſe inquiries. Take two plates of any metal, very clean and dry, whoſe ſurfaces are nearly equal; hang one of them to any excited non-electric, and bring under it upon the other a whole leaf of ſilver. When, which you find upon application, the ſilver leaf is attracted, lower the bottom plate; if it is too low, you will obſerve the leaf ſilver jump up and down; if too high, it will only be attracted in part, and thereby diſſipate the electrical power. But if you get it at the proper diſtance, which will very eaſily be found upon trial, the ſilver will be perfectly ſuſpended at right angles with their planes, like the *trapezium* of the Geometers, and touch neither of the plates; it will be extended likewiſe to it's utmoſt dimenſions. You frequently obſerve, both at the top and bottom of the ſilver, the electrical fire. The ſame effect is produced, if you reverſe the experiment, by electrifying the bottom plate, and ſuſpending the other over it. Now I conceive, that the ſpace occupied by this leaf of ſilver, is that where the *equilibrium* of the electrical æther is reſtored; for if you take away the under plate, thro' which from the floor the flux of this æther is furniſhed, or if that plate be placed upon an electric *per ſe*, by which this flux is prevented likewiſe, the ſilver leaf is blown away.

§ 59. No body can be ſuſpended in *equilibrio* but from the joint action of two different directions of power: ſo here, the blaſt of electrical æther from the excited plate blows the ſilver towards the plate unexcited. This laſt, in it's turn, by the blaſt of electrical æther from the floor ſetting through it, drives the ſilver towards the plate electrified. We find from hence likewiſe, that the draught of electrical æther from the floor, is always in proportion to the quantity thrown by the globes over the gun-barrel; or the *equilibrium* by which the ſilver is ſuſpended, could not be maintained. I once found, that a gentleman, at that time an invalid, whoſe ſhoes were perfectly dry, and of conſequence originally electrics, and who was employed to hold the non-electric plate through which the æther was to come from the floor; this gentleman, I ſay, did not furniſh a ſufficient quantity, becauſe of the dryneſs of his ſhoes, to maintain the *equilibrium*; and the ſilver was blown away. But upon employing another to this office, whoſe ſhoes were more wet, the æther came readily through him, and the ſilver was ſuſpended. I have likewiſe

likewiſe found a wooden pole, very dry, not conduct this æther faſt enough to keep the ſilver ſuſpended. It may be imagined, that it is poſſible for the ſilver to be ſuſpended, without ſuppoſing a flux of the electrical æther from the neareſt unexcited non-electric, as well as from the excited one; that is, by the ſimple electrical attraction. But to obviate this, it muſt be remembered, that the electrified gun-barrel both attracts and repels light ſubſtances at the ſame time. Can this attraction and repulſion be conceived without the operation of the electrical æther both to and from the gun-barrel at the ſame time? Does not this point out an afflux as well as an efflux? Are not the electrical repulſions as ſtrong at leaſt as the attractions? Do not we ſee light bodies, either between excited originally-electrics, or excited non-electrics, and unexcited non-electrics, dart like a ball between two rackets of equal force? It may be ſaid perhaps,

1. That the ſuſpended ſilver may only ſerve as a canal of communication, which diſcharges the Electricity from the excited non-electric to the un-excited one; and that when an originally-electric is placed between the lower plate in this experiment and the floor of the room, that then the ſilver is attracted only, until the lower plate is ſaturated with Electricity, and no longer. This is as much as ſaying that this effect ariſes from Electricity, without mentioning in what manner.

2. That this effect is produced by the electrical attraction, which gives the ſilver a direction towards the excited non-electric, but that it is kept down near the unexcited one by the force of gravity. Was this the cauſe, the action of gravity would operate as much thro' originally-electrics as through non-electrics.

But I am able to prove the afflux experimentally, as well as the efflux, § 60.
in the following manner. When the ſilver lies ſtill, though the motion of the globes is continued, between the two plates, one ſuſpended to the gun-barrel, and the other placed upon an electrical cake, a perſon ſtanding upon the floor needs only bring a ſmall glaſs ſiphon in a veſſel of water, and apply the long leg thereof near the plate placed upon the wax; for upon this the ſilver is immediately ſuſpended; and the water, which before only dropped, now runs in a full ſtream, and appears luminous *. Does not, in this caſe, the current of the water point out the direction of the current of electrical æther?

When the machine, *&c.* are placed upon originally-electrics, if a § 61.
man, ſtanding likewiſe upon an originally-electric, touches the gun-barrel while the globes are in motion, he will receive a ſnap or two; after which, though the motion of the globe is continued, he will perceive

* This experiment is more elegant, if the upper plate, attracting the ſilver, is ſuſpended high enough for a perſon ſtanding upon an originally-electric, conveniently to bring the other plate under it with one hand, and to hold a pewter plate in the other. If the originally-electric is ſufficiently thick, the ſilver will not be ſuſpended; but if the glaſs ſiphon in a ſmall veſſel of water is brought very near the pewter plate, the water runs into the plate, and the ſilver is immediately ſuſpended.

no

no more fire from the gun-barrel. While in this poſture, if he touches the wood-work of the machine with one hand, and applies a finger of his other near the gun-barrel, at that inſtant he receives the electrical ſtrokes. Theſe continue as long as he touches the machine, but ceaſe upon his removing his hand therefrom. Here we ſee a circulation of part of this man's electrical fire, which operates in the following manner. Firſt; The man, by applying one of his hands to the machine, becomes a part thereof; and, by the motion of the globes, part of the electrical fire, inherent in his body, is driven upon the gun-barrel; but it is inſtantaneouſly reſtored to him again, upon his touching the gun-barrel with his other hand. Thus he continues communicating the fire with one hand, and having it reſtored to him with the other, as long as he pleaſes. If, inſtead of touching the machine or gun-barrel, he holds his finger near either or both of them, you ſee the fire go out, and return back, as in a former experiment.

§ 62. It may be perhaps imagined, if one man touches the machine, himſelf and the machine both being placed upon the wax, and if another, ſtanding upon the floor, conſtantly, or by turns, touches the gun-barrel, that by theſe means the man upon the originally-electrics might be diveſted of all his electrical fire, by conſtantly continuing the motion of the globes, as he receives then no ſupply from the floor. But the contrary proves true; and, after a conſiderable time, the ſtrokes from the gun-barrel are as ſtrong as at firſt. But here we muſt obſerve, that the gun-barrel ſuſpended will not contain probably at one time $\frac{1}{1000}$ part of the whole quantity of this man's electrical fire: therefore I conceive, that, as ſoon as this man has parted with any portion of his neceſſary, his determined quantity, to the gun-barrel by the motion of the globes, he has it reſtored to him upon any unexcited non-electric's touching the gun-barrel, by having the uſual courſe of the Electricity * inverted.

§ 63. We ſee, from many experiments, that dry wood does not conduct Electricity ſo well as that which is wet; and that the man ſtanding upon the floor, who rubs the globes, excites the Electricity ſtronger than the cuſhions. This I had reaſon to conceive was owing not to any other difference, than that of his being more moiſt, and, of conſequence, more readily conducting the Electricity from the floor. Therefore I order'd my machine, and even the cuſhions to be made damp, by cauſing wet cloths to be placed upon ſeveral parts thereof; and found then, that the Electricity was equally ſtrong, as when the globe was rubbed by the hand.

§ 64. It remains now, that I endeavour to lay before you a ſolution why our bodies are ſo ſhocked in the experiments with the electrified water; the difficulty thereof I confeſs ſeemed unſurmountable, until I had made the following diſcoveries.

1. That the Electricity always deſcribed a circuit between the electrified water and the gun-barrel.

* For a further account of this matter, ſee Art. 28. § 7.

2. That

2. That the electrical fire came from the floor of the room.

3. That it would not pass from the floor quick enough for the person to be shaken, if his shoes were dry.

4. That the force was increased in proportion to the points of contact of non-electrics with the glass containing the water.

Then the solution of this phænomenon became more easy, which I take the liberty to offer.

1. I have endeavoured to prove by experiment that a quantity of Electricity is furnish'd from the nearest unexcited non-electrics, equal to that accumulated in excited originally-electrics and excited non-electrics.

2. This being so, when the phial of water held in one hand of a man is highly electrified, and he touches the gun-barrel with a finger of his other; upon the explosion which arises herefrom, this man instantaneously parts with as much of the fire from his body, as was accumulated in the water and gun-barrel; and he feels the effects in both arms, from the fire of his body rushing through one arm to the gun-barrel, and from the other to the phial. For the same reasons, if, in the experiment with the electrical * mine, a man places his right foot upon the lower small wire, and touches the gun-barrel with his left arm, the electrical force is only felt in that leg and arm.

3. As much fire as this man then parted with, is instantaneously replaced from the floor of the room, and that with a violence equal to the manner in which he lost it.

4. But this flux of electrical æther, either from the floor to the man, or from the man to the water, is prevented for reasons sufficiently obvious, if the glass containing the water be thick; if the points of non-electric contact are few; if the man is placed upon originally-electrics; or (which is the same thing) if the soles of his shoes are dry.

5. As we find that the Electricity passes at least equally quick through dense mediums, which are non-electrics, as through those which are more lax and spongy; may we not therefore conclude, that the cause why we feel most pain at the joints of our arms, and in the tendons of our heels †, arises from the texture in the tendons and tendinous ligaments of those parts?

From a due consideration of the *phænomena* before us, I take the li- § 65.
berty of proposing the following queries:

1. Whether or no the effects we observe, in bodies being drawn to and driven from either excited originally-electrics, or excited non-electrics, are to be attributed to the flux of electrical æther?

* See more of this in Art. 28.

† This pain in the heels is felt only in the experiment with the electrical mine; and it is not perceptible only when you touch the lower small wire with your foot, but likewise if you stand upon non electrics, which touch this wire. It has been strongly felt by a person standing upon a pedestal of *Portland* stone near ten inches in height, and upon one of metal more than two feet. I am of opinion, that no mass of metal, of dimensions however great, would in the least prevent the progress of the electrical power from the water in the phials to the body of the man.

2. Whether or no, that, which from it's being firſt diſcover'd in amber, we call Electricity, electrical æther, electrical power, *&c.* is any other than elementary fire?

3. Whether or no this fire does not appear in different forms, according to it's different modifications? Does it not, when diffuſed under a large ſurface, appear to affect us as air? When brought towards a point, does it not become viſible, as lambent flame? When nearer ſtill, does it not explode, and become the object alſo of our feeling as well as of our hearing? Altho' it does not affect our ſkin with the ſenſation of heat; does it not, by it's lighting up inflammable ſubſtances, ſhew itſelf to be truly fire?

4. Whether or no this fire is not connected intimately with all bodies at all times, though leaſt of all, probably, with pure dry air? Have we not found and ſeparated it from water, flame, even that intenſe one of oil of turpentine, ſmoke, red-hot iron, and from a mixture 30 degrees colder than the freezing point?

5. Have we not proved it's ſubtility, from it's paſſing through all known bodies?

6. May we not infer it's elaſticity likewiſe from it's exploſions, from it's increaſing the motion of fluids, as well as from it's effect in the concuſſion of our bodies, when we diſcharge it after we have accumulated it in water?

7. May not the electrical machine, from it's uſes, be denominated a fire-pump, with equal propriety as the inſtrument of *Otto Guerick* and Mr *Boyle*, that of the air?

8. Does not the power we are now maſters of, of ſeeing the ſeparation of fire from bodies by motion *, and of ſeeing it reſtored to them again, and even after that motion has ceaſed, cauſe us rather to incline to the opinions of *Homberg* (*a*), *Lemery* the younger (*b*),

* The ſetting in of the fire to the glaſs tubes and globes has always, in theſe experiments, been viſible both from the hands and cuſhions, by which they were rubbed. But as, till now, this fire was conſidered as coming from the glaſs, that, obſerved upon the hands and cuſhions, was always believed to be ſo much loſt by running down the inſtruments of friction into the floor. I endeavoured to prevent this loſs, by ſtanding upon originally-electrics; and found, to my great ſurprize, that ſo far from increaſing the electrical power, by ſtopping what I conjectured was ſo much loſs, I could excite then no Electricity at all in the tube and globes. This diſappointment, which, I afterwards found, had occurred to Meſſ. *Boſe* and *Allamand*, was the foundation of my diſcovering the ſource of the Electricity, and the manner of it's ingreſs to the machine.

(*a*) *Homberg* du ſouphre principe. *Mem. de l'Acad. Royale des Sciences*, 1705. La matiére de la lumiére eſt la plus petite de toutes matiéres ſenſibles—elle paſſe librement au travers et par les pores de tous les corps, que nous connoiſſons —Que tout l'univers eſt rempli de la matiére de la lumiére—J'ai mieux donnè à notre ſouphre principe le nom de matiére de la lumiére, que celle du feu, quoique ce ſoit proprement la même choſe.

(*b*) *Lemery* le fils. *Mem. de l'Acad.* 1709. p. 527. La matiére de feu doit être regardée, comme un fluide d'une certaine nature, et qui a des proprietez particulieres, qui le diſtinguent de tout autre fluide. Pag 8.—Qu'une matiére beaucoup plus ſubtile et plus agitée, qui remplit tous les vuides de l'univers, et ne trouve point les pores ſi étroits, qui ne lui laiſſent un libre paſſage, coule inceſſamment dans les lieux où elle eſt enfermée, et entretient ſon mouvement.

s'Graveſand

s'*Graveſand* (*c*), and *Boerhaave* (*d*), who held fire to be an original, a diſtinct principle, formed by the Creator himſelf, than to thoſe of our illuſtrious countrymen, *Bacon* (*e*), *Boyle* (*f*), and *Newton* (*g*), who conceived it to be mechanically producible from other bodies?

9. Muſt we not be very cautious, how we connect the elementary fire, which we ſee iſſue from a man, with the vital flame and *calidum innatum* of the Ancients; when we find, that as much of this fire is producible from a dead animal as a living one, if both are equally replete with fluids?

10. Whether or no it is not highly probable, that by increaſing the number and ſize of the phials of water in a certain manner, you might not inſtantly kill even large animals by the electrical ſtrokes (*h*).

I cannot conclude theſe papers, without congratulating that excellent § 66.
Philoſopher and learned Member of this Society the Abbé *Nollet* of *Paris*. This gentleman, almoſt two years ſince, in a letter to Profeſſor *Boſe* (an extract of which this laſt publiſhed with a work (*i*) of his own) without the knowledge of ſeveral experiments ſince diſcover'd; at leaſt none of his diſcoveries have yet fallen into my hands, did declare his opinion, (*k*) that the Electricity did not only proceed from the electrified bodies, but from all others about them to a certain diſtance; (*l*) that the Electricity, as well from bodies electrified, as from

(*c*) s'*Graveſand Philoſoph. Newton Inſtitutiones, cap.* 1. Ignis in corpora omnia quantumvis denſa & dura penetrat.——Corporibus ſeſe jungit——ignem ad certam diſtantiam a corporibus attrahi—nulla novimus, quæ ignem non continent—non ignis æque facile corpora omnia intrat——corporibus contentus in his a corporibus circumambientibus retinetur.—Motu celerrimo ignem affici poſſe.

(*d*) *Boerhaavii Elementa Chem. de igne*, p. 187. & ſeq.—Ipſe ignis—ſemper præſens exiſtit in omni loco—imo vero in omni corpore, etiam rariſſimo, vel ſolidiſſimo, æqualiter diſtributus hæret.—Haud ergo potui detegere, quod in rerum natura ſit vel ullum ſpatium ſine igne.

Ibid. p. 283. Huc uſque conabar—tradere ea, quæ veriſſima addiſcere potui de natura illius ignis, quem elementalem appellant philoſophi. Illum ſcilicet, ita conſiderando, prout creatus ipſe in rerum (natura) exiſtet ſeorſum, extra reliqua omnia creata, quæcunque demum ſint, corpora.

(*e*) Vide tractatum *De forma calidi*.

(*f*) *Mechanical Origin of Heat and Cold*, ſect. 2.

(*g*) See Queries at the end of his *Optics*.

(*h*) Monſ. *le Monnier* at *Paris* killed birds by theſe; and with me, a linnet and a rat, much more than half-grown (the largeſt I was then able to procure) have been ſtruck dead.

(*i*) *Recherches ſur la Cauſe, et ſur la veritable Theorie de l'Electricité.* Wittembergue, 1745.

(*k*) *Voyez Nollet dans les Recherches, &c. du M. Boſe*, pag. xlv.—La matiére electrique vient non ſeulement du corps électriſé, mais auſſi de tous ceux qui ſont autour de lui, juſques à une certaine diſtance.

Ibid. p. xlix.—Si vous pouvez vous convaincre comme moi, que la matiére qui va au corps électrique vient primitivement de tous le corps environnans, de l'air même, vous aurez bien plus de facilité à expliquer tous les autres efféts.

(*l*) *Ibid.* p. xlvi. La matiére electrique, tant celle qui ſort du corps électriſé, que celle qui vient des environs à ce même corps, ſe meut plus facilement dans les corps denſe que dans l'air même.

thoſe which were not, paſſed more readily through denſe mediums than air; *(m)* that the Electricity is preſent in all bodies; *(n)* that this matter always tends to an *equilibrium*, and endeavours to occupy thoſe ſpaces in bodies, which have not their neceſſary quantity: all which aſſertions may now be proved by experiments.

§ 67. You ſee, Gentlemen, by my aſſerting, that what we have hitherto called electrical *effluvia*, do not proceed from the glaſs, or other electrics *per ſe*, I differ from *Cabeus*, *Digby*, *Gaſſendus*, *Brown*, *Des Cartes*, and very great names of the laſt as well as the preſent age. My differing from them would be preſumption indeed, were I not induced thereto, by obſervations drawn from a ſeries of experiments carefully conducted, to which many of you have been witneſſes, and to whom I may therefore appeal, for taking what may ſeem ſo extraordinary a ſtep. I have conſtantly had in view that excellent maxim of Sir *I. Newton* laid down in his *Optics*, that, "as in Mathematics, ſo in Natural Philoſophy, the "inveſtigation of difficult things by the method of analyſis ought ever "to precede the method of compoſition. This analyſis conſiſts in mak-"ing experiments and obſervations, and in drawing general concluſions "from them by induction, and admitting of no objections againſt the "concluſions, but ſuch as are taken from experiments, or other certain "truths. For hypotheſes are not to be regarded in Experimental Phi-"loſophy. And although the arguing from experiments and obſerva-"tions by induction be no demonſtration of general concluſions; yet it is "the beſt way of arguing which the nature of things admits of, and "may be look'd upon as ſo much the ſtronger, by how much the in-"duction is more general.——By this way of analyſis we may proceed "from compounds to ingredients, and from motions to the forces pro-"ducing them; and, in general, from effects to their cauſes, and from "particular cauſes to more general ones, till the argument ends in the "moſt general." I am deſirous, that what is contain'd in theſe papers, you will be pleaſed to regard rather as the rude outlines of a ſyſtem, than as a ſyſtem itſelf; which, I am in hopes, men of better heads and more leiſure will proſecute: and if hereafter, from being poſſeſſed of more obſervations than we at preſent are maſters of, any opinions in theſe papers ſhall be found erroneous, I at all times ſhall be willing readily to retract them.

Extracts of two letters from the Rev. Hen. Miles, *D. D. F. R. S. to Mr* Hen. Baker, *F. R. S. concerning the effects of a*

10. Being determined on making ſome experiments in Electricity with other bodies beſides glaſs, a little before the Holidays I procured a ſtick of the beſt black ſealing-wax, of about an inch in thickneſs, and of a convenient length; and exciting it with white-brown paper, or clean dry flannel (I know not which is beſt) I made the following trials.

I attempted to kindle common lamp-ſpirits, both by attraction and repulſion, the electrified perſon ſtanding on a cake of bees-wax, and ſuc-

(m) Ibid. p. xlvii.

(n) La même. Cette matiére tend à l'équilibre, et s'empreſſe de remplir les eſpaces, qui ſe trouvent vuides des parties de ſon eſpece.

ceeded.

ceeded.——I made trial, at the ſame time, with my glaſs tube, and, I think, kindled the ſpirits more eaſily. Perhaps, from ſome circumſtances hereafter to be mentioned, this may, *cæteris paribus*, be generally expected.

cane of black ſealing-wax, *and a cane of* brimſtone, *in electrical experiments.* N°. 478. p. 27. Jan. *and* Feb. 1746. *Dated* Jan. 15. 1746. *Read* Jan. 23. 1745-6.

I was then minded to repeat that experiment of the late ingenious and induſtrious Dr *Deſaguliers*, and others; by which it appears, that when any light body is put into a ſtate of repulſion by vitreous Electricity, it is in a ſtate of attraction in reſpect of reſinous Electricity, and ſo *è contra*. This I found conſtantly to hold good. —I made this trial with a down-feather, which was tied to the end of a pendulous thread, which thread was tied to a ſilk line, faſtened horizontally to the oppoſite ſides of the room, and alſo with a ſmall piece of writing paper, of about the ſame dimenſions as the feather. Here I found the feather would retain the *effluvia* (whether of the tube or cane) about five or ſix minutes longer than the paper would; that is, the feather remained ſo much longer in a ſtate of repulſion The time in which the paper was in a ſtate of repulſion, after many trials, I found to be about 20′, more or leſs; at about which time the paper would indeed ſomewhat ſenſibly decline the tube, *&c.* but in a moment would be attracted by them; and if I ſtaid longer, I could not perceive any repulſive force remaining.

I ought to tell you, that when I had, by ſeveral trials, found out about what time the *effluvia* would be quite diſſipated, I forbore making any trials till then, leſt that, by bringing the tube or cane near the body of trial, I might communicate freſh *effluvia*, and perpetuate the ſtate of repulſion longer than it would otherwiſe have been; ſo that, in the laſt trials I made, I never came near with the tube, *&c.* till full 20′ after the body of trial was put into a ſtate of repulſion. I obſerved not any material difference of time between the diſſipation of the *effluvia* of the glaſs tube, and thoſe of the wax cane, when the ſame body of trial was made uſe of for both: if there was any difference, I think the vitreous *effluvia* were the moſt laſting.

I made another trial with the cane and tube in a dark room; being led to it from a ſuſpicion I had, that the *effluvia* from the wax cane were groſſer, and more in quantity, than thoſe from the glaſs tube; and, upon exciting both as quick as I could in ſucceſſion, I found the luminous *effluvia*, when I brought my forefinger near the wax, to proceed in a much greater quantity to the cane from the tip of my finger, than they did on the ſame trial with the tube of glaſs. And I ſeveral times obſerved a ſmall globular ſpot of fire to appear firſt on my finger, from which iſſued regular ſtreams in form of a comet's tail.

When I made uſe of the glaſs tube, as the quantity was leſs, ſo the ſparks were finer, leſs in thickneſs and in length, but much more active; nor did they proceed ſo regularly towards the tube, nor make ſo regular an appearance (being ſeldom, if ever, altogether regular, as the others); frequently breaking in pieces, as if by colliſion, or not altogether unlike the ſparks from a brand in a wood fire, which has lain long without being

being ſtirred. Another difference I remarked was, that the reſinous *effluvia* were more deeply coloured than the vitreous.

Q. Whether it be not probable, that the reſinous *effluvia* are more unctuous or ſulphureous than the vitreous; and becauſe not ſo active and nitrous, leſs apt to kindle inflammable ſpirits, as I think I found them to be?

I intreat I may not be conſidered as pretending, in the above trials, to eſtabliſh laws, but, as plainly relating matters of fact. Perhaps future trials may not confirm theſe.

I think it not a circumſtance too impertinent to be mentioned, that the trials relating to repulſion were made in a ſmall room, and near a fire; the air pretty moiſt.

Extract of the ſecond letter dated Jan. 22. 1745-6.

I am dubious whether I did not expreſs myſelf in a manner liable to be miſunderſtood, when I ſaid to this purpoſe, *that I would not be underſtood to eſtabliſh laws by the fore-mentioned experiments, but only to relate facts; and that future experiments might not confirm theſe.* I did not intend this ſhould extend to that experiment, which proves the different nature of vitreous and reſinous *effluvia*; which I preſume, may be conſidered as invariable inherent properties; ſo that bodies, put into a ſtate of repulſion by the one, will be attracted by the other, *&c.* But the other *phænomena*, as depending on changeable circumſtances, the temperature of the air, the degree in which the electric bodies may chance to be excited, the quantity of *effluvia*, and perhaps others to us unknown; the other *phænomena* (I ſay) depending on ſuch like circumſtances may be variable.

I beg leave to inform you, that I have been making trial with a ſtick of ſulphur of the common ſort, which I made of a convenient ſize, by caſting it into a coffin of paper, the inſide being of writing paper: this, being excited, attracted the bunch of threads with great power, and kindled common ſpirits as quick as ever I knew it done. This was after night, and I ſaw not what the day-light afterwards diſcovered, that the inſide round of paper adhered to the ſulphur, and it had made it's way thro' the paper, which concealed the colour of the paper, and it's adherence, till next day; however it performed as above.——This was broken, by an attempt to ſtrip the paper off the ſtick by a too officious perſon, without my knowledge.——I then caſt another with the ſame ſulphur, and an addition of freſh, melted together in a wooden mould, which came out ſmooth and well; but was perſuaded, againſt my own judgment, to put a gun-rammer into the middle of the mould, to ſtrengthen it; which ſtick anſwer'd that end; but, as I fear'd it would turn out, the ſulphur tho' of a great thickneſs round the ſaid gun-ſtick, could by no means be excited to any tolerable degree. I therefore made a third, as the firſt, which has the paper on it as before, but it performs exceeding well: having ſuffered myſelf to be electrified with it, upon the approach of a perſon's finger to mine, I had by far the moſt painful ſenſation I ever

yet

yet felt in any of theſe experiments.—I believe a glaſs tube might be beſt of all for a mould (but mine are of too ſmall a bore), if one could be aſſured it would not break.

11. I am under no doubt, but that experiments with ſulphur are capable of being improved, and hope ſhortly to make it appear. I am loth to venture my glaſs tubes of flint for a mould, but intend to procure one of common glaſs; having lately had the misfortune of loſing my beſt, in ſo odd a manner, that I believe you will excuſe me if I trouble you with the account.

Extracts of two letters from the ſame, containing ſeveral Electrical Experiments. *Ibid.* p. 53. *Dated* Feb. 4. 1745-6. *Read* Feb. 13. 1745-6.

I had been uſing it but a little time in the evening; and, before I laid it up, having by me a round ruler ſmall enough to go into the bore, when it was covered with a roll or two of brown paper, it came into my head to excite it, by rubbing it a little on the inſide with the ſaid ruler and paper; but not finding any effect of it, after a few minutes trial, not ſo much as to attract the ſmalleſt thread, I laid it in my window in my ſtudy on a parcel of papers and pamphlets, where it uſed to be put; and next morning, as we were at breakfaſt, I heard a ſnap, and, on turning my head, found about two inches of my tube broke off very regularly. Upon this I took it, and placed it againſt a cupboard-door, erect, in a pocket of leather, that had been nailed up againſt the door for ſuch a purpoſe. The upper end was tied to two thongs of leather, but not tight, only to prevent it's ſtirring: thus it continued ſafe till I went to bed; but, in the morning, upon opening the ſaid door, I was ſurpriſed to find my tube in ſhivers, except about three inches, as if it had been broke with a ſmart blow of a hammer. The cupboard is over the fire-place, and ſo near it, that I think it impoſſible it ſhould ever have been quite cold; and the window where it was firſt put is ſo near the fire, and it's being laid on the ſeat of the window, a foot below the ſaſh, it could not be much affected with the air from thence.——The weather was froſty, but the tube from firſt to laſt never out of the room; and I am ſure never had any blow.

The ſtick of brimſtone I laſt made, with which I kindled lamp-ſpirits ſo readily, as I informed you before, was ſet up in the foremention cupboard in an erect poſture, has loſt all it's electric virtue, and cannot be made to attract a down-feather, or a fine thread.——This I know not how to account for, unleſs it be, that the expoſing it to the air, by it's not being wrapped up in any thing, may have deprived it of it's power: for, if I miſremember not, *Stephen Gray* uſed to keep his ſulphur conic bodies, caſt in wine-glaſs, in a box, and wrapt up in flannel; however I ſhall attempt to recover it again.——The cupboard is ſmall, and never cold.—My ſtick of wax kept in my deſk, not wrapt, will attract a thread at any time, without rubbing at all.

Laſt night, having ſeveral gentlemen with me, who were deſirous of ſeeing me ſet fire to ſome ſpirits of wine, I was willing to try whether I could not kindle the ſame with an icicle; but, not being able to get one, I attempted it with a thick piece of ice, and immediately ſucceeded, in

preſence

preſence of 7 or 8 perſons; and I think the ſparks of fire from the ice, when the finger of a non-electric perſon was brought nigh it, were as large and as powerful as any I ever ſaw; ſo that I am ſatisfied the power of them is no ways diminiſhed by the coldneſs of the ice: and I doubt not, but that, if the ice be kept from melting and dropping into the ſpirits, ice will kindle them as readily as any other ſubſtance: the ſpirits were ſuch as we uſe for the tea-kettle lamp, and far from being of the beſt ſort.

One circumſtance more I will mention, and releaſe you.——By accident one of the gentlemen approaching the electrified perſon with his hand near his ſhoulder, the ſaid gentleman felt a very pungent ſtroke on his fleſh, thro' his coat and waiſtcoat, which were both cloth. This was repeated ſeveral times, and in every one's opinion (on whom trial was made) the repulſive ſtroke was as ſmart as it is wont to be on the end of the finger, when nothing intervenes; and the ſenſation continued as long. I know not whether this has been before taken notice of; if it has, your goodneſs will excuſe my impertinence.

Extract of the ſecond letter. Since my former, I made an other trial, and ſucceeded with all the eaſe imaginable; the ſpirits kindling the very moment of my approaching them with the lump of ice, which was an inch and $\frac{1}{4}$ thick. After this I took a clamp of iron, ſuch as is uſed for heating box-irons for ſmoothing linen-cloths; and having heated the ſame red-hot applied it to the ſpirit, as I ſtood on the cake of wax electrified, holding the ſame in a pair of tongs.

I did not, I confeſs, expect much from this trial; and the event was, that I could not kindle the ſpirits, during the time the redneſs continued in the clamp; but, as ſoon as that diſappeared, and it began to look blackiſh, the ſpirits were kindled as uſual.

I ſhall not draw any concluſion from a ſingle trial; perhaps ſome reaſons might be aſſigned, why the red-hot iron did not kindle the ſpirits, provided one were ſure this would always be the caſe; and if the experiment were repeated with the ſame conſequence a good many times, one would venture to ſay, that the heat of the iron contributed no power of inflaming to the *effluvia*.

My tube I have uſed of late is not made of the fine flint-glaſs, but ſuch as common wine glaſſes are made of.

I have got me a tube made of common green glaſs: this is exceeding light, in compariſon with others; and may be excited with double the time and pain required for the others, but yet not without warming it at the fire; though this ſeems powerful enough to attract the bunch of threads, yet I am not able to kindle any ſpirits with it.

I have made theſe trials, that I might be able to determine which kind of glaſs afforded the greateſt quantity of *effluvia*, or at leaſt the ſtrongeſt, as near as might be; which may not be altogether unuſeful to be known.

12. Mr

12. Mr *l'Allamand* incloſed ſome mercury in a tube cloſe-ſtopp'd; and, when he afterwards rubb'd this tube, it gave a great deal more light than when the ſame had no mercury in it.

When this tube has been rubb'd, after raiſing ſucceſſively it's extremities, that the mercury might flow from one end to the other, one ſees a light ſerpenting all along the tube; that is to ſay, the mercury, as it runs along, is all luminous.

Mr *l'Allamand* then made the mercury run in the ſame manner along the tube without rubbing it, and it ſtill gave ſome light, but much leſs than before. This laſt experiment perſuaded him, that the friction of the mercury againſt the glaſs might electrify that glaſs, in the like manner as the rubbing of the hand. And he has been confirmed in the ſame notion by another experiment: He brought ſome down near to the tube, and then made the mercury run from one end to the other; and the down was attracted, as the mercury in it's motion paſſed by it.

Theſe experiments he has repeated, and varied ſeveral ways; and they have led him to conclude, that the *phoſphorus* of the barometer, known this great while, is not ſo properly a *phoſphorus*, as the effect of the mercury electrifying the tube of the barometer.

Mr *l'Allamand* has put mercury into exhauſted tubes, and, when theſe are rubb'd, they give much more light than before; there then come out from them on all ſides rays of very lively light. I have alſo ſeen at *Leyden*, at Mr *Muſſchenbroeck*'s the mechaniſt's, an exhauſted globe of glaſs, which, when rubbed with the hand, ſeemed all filled with a very bright fire.

Several perſons have obſerved, that when they had been electrified, their pulſes beat a little faſter than before. I have even myſelf felt, after having been electrified a pretty while together, a ſenſation all over my body: but within theſe few weeks, ſome perſons have felt very ſharp pains upon their being electrified.

There is an experiment that Mr *l'Allamand* has tried; he electrified a tin tube, by means of a glaſs globe; he then took in his left hand a glaſs full of water, in which was dipped the end of a wire; the other end of this wire touched the electrified tin tube: He then touched, with a finger of his right hand, the electrified tube, and drew a ſpark from it, when at the ſame inſtant he felt a moſt violent ſhock all over his body. The pain has not been always equally ſharp, but he ſays, that the firſt time he loſt the uſe of his breath for ſome moments; and he then felt ſo intenſe a pain all along his right arm, that he at firſt apprehended ill conſequences from it; tho' it ſoon after went off without inconvenience.

It is to be remarked, that in this experiment he ſtood ſimply upon the floor, and not upon the cakes of reſin. It does not ſucceed with all glaſſes; and tho' he has tried ſeveral, he has had perfect ſucceſs with none but thoſe of *Bohemia*. He has tried *Engliſh* glaſſes without any effect. That glaſs with which it beſt ſucceeded was a beer-glaſs.

Mr *Muſſchenbroeck* the profeſſor has repeated his experiment, holding n his hand a hollow bowl exceeding thin, full of water; and he ſays he

Part of a letter from Mr Trembley, *F. R. S. to* M. Folkes, *Eſq*; P. R. S. *concerning the light cauſed by* quickſilver *ſhaken in a glaſs tube, proceeding from* Electricity. *Ibid* p. 58. *Dated* Hague, 4 Feb. 1745. N. S. *Read* Feb. 13. 1745-6.

experienced a moſt terrible pain. He ſays, the glaſs muſt not be at all wet on the outſide.

Part of a letter from the Rev. Dr Miles, *F. R. S. to Mr* Hen. Baker, *F. R. S. concerning* electrical fire. *Ibid* p. 78. *Dated* Feb. 15. 1745-6. *Read* Feb. 20. 1745-6.

13. You query, whether that ſubtil fire which kindles warm'd ſpirit of wine, be reſident in the body from which it evidently iſſues, and be kindled occaſionally? or, whether it comes from the excited tube pervading inſtantaneouſly the body it is applied to? or, laſtly, whether there are certain principles in the air, which are thus agitated into an extemporaneous lightning? Theſe queries are certainly very comprehenſive and important; I wiſh I were able to return you ſomewhat more ſatisfactory than ſuppoſitions.

I incline to think the electrical and luminous *effluvia* to be the ſame, and not diſtinct ſubſtances. Mr *Haukſbee* ſeems to diſtinguiſh them, intimating, that no luminous matter would be communicated from an excited cylinder of wax to his finger, when brought near to the cylinder, though it attracted light bodies; but it is to be obſerved, that this cylinder of wax was only a coat of wax, of about half an inch thick, on a wooden cylinder of four inches diameter: now I have always found my ſtick of wax, which conſiſts of nothing elſe, to emit luminous *effluvia* very plentifully, and rather in a greater degree than the glaſs tube.

If we conclude with the *Engliſh* philoſophers, that fire is mechanically producible from other bodies, by colliſion, attrition, *&c.* or, according to Sir *I. Newton*, by putting the ſulphureous particles of bodies into a very ſtrong vibratory motion; by which means they become hot and lucid, *i. e.* affect us with ideas of light and heat; on this ſuppoſition may we not conclude, that the action on the glaſs tube, when it is rubbed, by putting the parts of it into ſuch a vibration, and, conſequently, agitating violently the ſulphureous particles therein, may heat and kindle them? And may it not alſo be ſuppoſed, that when the air is in a due ſtate, nitrous or other particles in the air may contribute to the kindling them? or, perhaps, rather that ſubtil, active, elaſtic ſubſtance, which Sir *I. Newton* ſuppoſes to be the cauſe of the refraction, *&c.* of light, and which communicates heat to bodies, and is univerſally diffuſed? Theſe *effluvia*, being thus agitated and conveyed by a non-electric body intervening, in a due quantity, to the vapour of the warmed ſpirit, may be ſuppoſed to kindle them, without exciting any originally-reſident fire in the body immediately communicating with them; the luminous *effluvia* from the finger, or ice, *&c.* when brought near the inflammable body, being, as far as we can perceive, of the very ſame kind with thoſe which proceed from the tube; or there is nothing appearing in them which may lead us to ſuſpect they are not the very ſame, tho' in a greater quantity than what can come from the part of the tube you approach with the end of your finger.

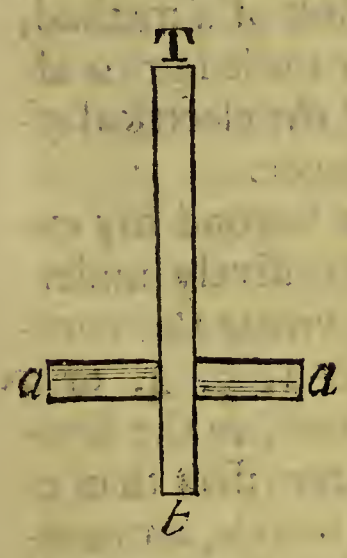
If we conclude with ſome of the foreign philoſophers, *Boerhaave*, *Homberg*, *Lemery*, *s'Graveſand*, &c. that fire is equally diffuſed throughout the univerſe by the Creator, pervading the interſtices of all bodies, and that there is no fire mechanically produced *de novo*; then, may we not conclude,

clude, that whereas, by attrition of the glaſs tube, there is produced a a very quick and ſtrong vibration of its parts, which muſt neceſſarily affect the fire contain'd in the vacuities, by compreſſion and relaxation; ſo that, as *Boerhaave* expreſſes it, there muſt be, in the bodies thus agitated, and in the fire contained in its pores, an exceeding great motion excited, and, together herewith, the ſurrounding fire from both theſe cauſes, muſt be agitated, and ſo much the more violently, the nearer it is; may we not conclude, that its force will be hereby ſufficiently increaſed to kindle the ſpirit to which it is convey'd?

In this, as in the former hypotheſis, I would not exclude the elaſtic *materia ſubtilis* from being ſuppoſed the having an influence on the *effluvia*. Whichſoever of the two *hypotheſes* we embrace, you may perceive, that I incline to think, that the kindling fire rather proceeds from the excited tube; I am very ſenſible I am in a great meaſure groping in the dark; but hope future experiments will caſt a light on this obſcure ſubject.

A letter from the ſame, concerning the Electricity of water. *Ibid* p. 91. *Dated* Feb. 20. 1745-6. *Read* Feb. 27. 1745-6.

14. I this afternoon, on reflecting afreſh on Monſieur *l'Allamand*'s experiment, reſolved to make the following trial, tho' I was in no doubt what the iſſue would be: I took my tin tube, which has two arms to it, directly oppoſite one to another; and at that diſtance from one end of the tube, which is equal to the length of one of the arms, as you may perceive by the figure in the margin (not to trouble you with the uſe it was made for, at preſent): this I ſuſpended by a ſilk line from the ceiling of the room, letting it hang down of a length convenient for my purpoſe. I then took a china baſon, holding better than a quart, and, having nearly filled the ſame with water, I ſtood on the wax cake, with this baſon of water in my hand, ſo near the pendulous tube, that I could apply the baſon to it with convenience: then, having ſuffered myſelf to be electrified, I held the baſon ſo under the tube, that the lower end dipped an inch more or leſs in the water: upon this, a perſon approached one end of one of the arms with the ſpirit of wine in a ſpoon, and it was immediately kindled with vehemence; and at the ſame time I received on one of my fingers that held the baſon a pungent ſtroke; and that ſtroke was given the very inſtant of time the ſnap was at the ſpoon, or any other object that was applied. The wind was then S. and hard rain, as moſt part of the day; and yet, if one were diſpoſed to indulge imagination, the *effluvia* ſeemed to act more ſtrongly than is uſual. I think there can be no doubt, but that water is as good a *medium* of communication to the *effluvia*, as any ſubject whatever; for, that all thoſe which came to the ſpirit were conveyed to the tube by the water, I am certain; ſince the tube dipped in the centre, and was then motionleſs; ſo that it never came ſo near the baſon as to receive any *effluvia* from it.



T, *t*, the tube.
a, *a*, the arms.
At *t* the baſon of water was held.

A letter from —— to Mr John Ellicot, *F. R. S. of weighing the ſtrength of electrical* effluvia. N°. 479. p. 96. Mar. *and* Apr. 1746. *Read* Mar. 6. 1745-6.

15. As you were the only perſon who ever ſhewed me any electrical experiments, and have been ſo kind, according to your wonted candour, to aſſiſt me freely upon this and all other like occaſions; I think it proper to give you this firſt account of what I have thought of towards gaining a farther inſight into the nature, power, and laws of Electricity.

From the time I ſaw thoſe experiments at your houſe about 3 years ago, I had little or no opportunity of making any myſelf, until within this month; when, having got ſome good utenſils, I repeated, or imitated moſt of the trials I had heard of, with ſucceſs. And particularly having heard, that Mr *Gray* gave an account of balls cauſed to move round one another by means of electrical *effluvia*, I was very deſirous of ſeeing ſo delightful a ſight. And though I was diſappointed in my expectation of a circular motion, yet I found it eaſy to make two balls act upon each other, in a very entertaining manner, for a long time; and that with ſuch a conſtancy and regularity, as to the effect, that I apprehend one may thence deduce a gauge or ſtandard, whereby to meaſure electrical powers, and compare the quantities and ſtrength of the virtue infuſed into, or remaining in, non-electrical bodies after given times, *&c.*

This, together with a great deſire to be able to eſtimate and compare the effects of experiments with ſome certainty, and to do ſomething more than amuſe myſelf and friends with the ſeveral ſurpriſing *phænomena* which thoſe experiments produce, led me, about 10 days ago, to think of a method, which, for aught I know, is quite new, and ſeems to promiſe fair to afford much new light: it is to try or weigh the ſtrength of the electrical *effluvia*, virtue, or power, by cauſing it to act upon a balance.

I found, the firſt day, that this method anſwered even beyond my expectation; ſo that ſeveral non-electrical balls placed ſucceſſively underneath one of the ſcales, and then imbued with electrical virtue the common way, would preſently cauſe that ſcale to deſcend 2, 3, 4, or 5 inches, and ſeem to cleave, for 10 or more ſeconds of time, to the ſeveral bodies ſo placed underneath, ſome having much greater effect than others. Whence it appeared, that there was a ſufficient latitude for comparing very different forces, if any ſuch there were. At the next and only opportunity I have had ſince (my *apparatus* being made more commodious), I uſed flat inſtead of globular bodies, and then I found the effects far more conſiderable; ſome of them, whoſe upper ſurface was about 3 inches ſquare, having attracted and held down one ſcale, when there were about 200 grains weight in the other.

Though I am tempted to communicate ſome things, which I have already obſerved by this means, with much delight, I reſerve them at preſent for a farther examination; deſiring in the mean time, that you will communicate or divulge this in ſuch manner as you think proper (only concealing my name), that others, who may have an inclination, may purſue and improve the hint. And, for the eaſe of ſuch, I muſt add, that the ſtrings of that ſcale which is to be acted on, muſt be long, and non-electrical, and, I think, thick; that there may be a ready paſſage

for

for the electrical virtue to run off as faſt as it is received. Inſtead of a braſs ſcale-pan, I uſed a flat piece of cork, filed very ſmooth and even, eſpecially on the under ſurface. The other ſcale needs no alteration, provided the ſtrings be made of ſilk, as uſual, and ſhort enough to keep that ſcale out of the reach of the electric virtue, which is to act upon the former. If the beam were three or four feet long, the ſtrings of both ſcales might be of a length, which would make it leſs troubleſome to put in and take out weights.

I mounted the attracting bodies upon ſmall taper ſticks, about $2\frac{1}{2}$ feet, whoſe thicker ends had a foot which ſtood upon 2 cakes of bees-wax full 10 inches thick in all.

I forbear to deſcribe the pretty little ſimple inſtrument you furniſhed me with at my firſt ſetting out; I leave that to yourſelf; only, as it has no name, I take the liberty to call it an *electrical needle*. Every body, who delights in ſuch matters, will thank you for it, if it were only for the amuſement it will furniſh for ſo many hours, after being but once well ſeaſoned, or tinctured with electrical *effluvia*.

But, I think, this little inſtrument, and the balance together, cannot fail of informing us farther concerning the properties of Electricity: ſuch as, how far it agrees and diſagrees with magnetiſm; whether it paſſes through the ſubſtance, or only along the ſurface of bodies; whether it proceeds in any, and what particular direction, or has any particular tendency; in what particular bodies the moſt of it may be collected and retained; and how long; how far the figure, ſize, denſity, or colour of bodies may be concerned; whether, as theſe *effluvia* may be felt, heard and ſeen, they may likewiſe be weighed; and many other matters, which will occur to the diligent obſerver.

16. On my making uſe of one of my boxes filled with pitch, wax, *&c.* for the perſon to be electrified to ſtand upon, after uſing it a little while ſucceſsfully, I got the man who aſſiſted to wipe the ſurface of the pitch, *&c.* with a dry clean cloth, ſuſpecting, from the place it had ſtood in, ſome dampneſs might lodge thereon. This being done, for my ſatisfaction I ſet up the box on one ſide, and held a thread of trial at a proper diſtance, and found it to attract and repel the ſame: but, on ſetting it down, and ſtanding upon it, by no means could it be made appear that I was electrified, or any other perſon who ſtood thereon afterwards. I thereupon took another box of the ſame ſort, but made uſe of it without wiping it, and it performed well. This I have not yet repeated, but intend to do it.

Part of two letters from the Rev. Hen. Miles, *D. D. F. R. S. to Mr* Hen. Baker, *F. R. S. containing ſome* Electrical *Obſervations*, *Ibid.* p. 158. *Dated* Mar. 20. 1745-6. *Read* April 17. 1746.

In a pint-bottle of flint-glaſs I have ſome ſmall pieces of braſs leaf, and the bottle hermetically ſealed. Upon trying whether the excited tube would much affect the ſaid leaf, I was at firſt diſappointed in my expectations; for tho' the tube was ſo well excited, as that, upon bringing it near the bottle, ſtrong and loud ſnaps were given, there was hardly any ſenſible motion in the braſs leaf, till I thought of warming the bottle at the fire; and then there was a conſiderable one, tho' not what I expected before I made

made any trial. But I ſuſpect the bottle to be too thick; for, on trying a common flaſk, which we ſealed in the fire, the leaf which I had put in was very ſtrongly both attracted and repelled a great many times.

One odd circumſtance I will tell you, and detain you no longer: Upon my lifting up the tube haſtily by chance, I obſerved the leaf to be powerfully attracted by the ſides of the bottle or flaſk next to the tube; this put me on trying purpoſely what the effect would be, if, when a perſon held either in his hand ſideways, ſo as the neck was parallel with the horizon; I took the excited tube, and moved it up and down towards and from the floor, at 3 or 4 inches from the bottle, ſucceſſively, as faſt as I could, without hazarding my ſtriking againſt it; upon which the braſs leaf was as ſucceſſively attracted and repelled, or ſeemed to follow the motion of the tube, or was affected, as it would have been if I had beat the air upon it, tho' in a very inferior degree, as you will ſuppoſe; and thus it would be, if the tube was held at a greater diſtance; and in the flaſk, I carried my hand ſo as that the tube deſcribed a circle about it, at the diſtance of 6 or 7 inches, the whole of the leaf would be put into a conſtant, regular gyration, which would hold as long as I could well continue the motion. This ſeemed to me ſtrange, that if I brought the tube near, and removed the ſame ſlowly, no motion (eſpecially in the bottle) was obſerved, or what was next to none; and yet that this ſudden motion of the tube ſhould produce ſuch an effect; but I think it may be thus accounted for: while the tube is held near the bottle, *&c.* for any time, the leaf-braſs is kept in a ſtate of repulſion; and therefore, under that confinement in the bottle, is motionleſs; but on my ſudden withdrawing the tube, the ſide of the glaſs oppoſite the leaf ſerves as an attractive to it, while the ſide on which it lay repels it; and thus, by the motion of the tube mentioned, there is a conſtant ſucceſſion of attraction and repulſion.

The ſecond letter, dated Apr, 16. 1746.

It may be hardly worth while to tell you, that I fired common ſpirit of wine, at the diſtance of 25 feet, the *effluvia* being conveyed by 3 perſons and 2 laths of deal, tyed together thus: the perſon to be electrified immediately ſtanding on a cake of wax, and holding one end of the lath, another perſon ſtanding about the middle of the diſtance on another cake, and ſupporting the lath, and a third perſon at the further end, who held the other end of the lath, and fired the ſpirit; and ſometimes held the ſpoon, while a fourth perſon fired them by repulſion. In this experiment, inſtead of common thread, I uſed ſilver and gold twiſt, or what, I think, the ladies call plate; and I have reaſon to think this much better than the former.

I am ſo far from being of Abbé *Nollet*'s mind, that I think no ſort of glaſs is proof againſt the effects of a moiſt air. I conclude this from Mr *Watſon*'s experiments and my own.—I told you before where I kept my tube; and I can aſſure you, I find as great a difference as can well be in the ſame tube, between what it is one day and the next, even when I have ſeen no great reaſon to expect, from any ſenſible change in the

air

air, it ſhould be ſo. But whence ariſes that we call moiſtneſs in the air? I have many times known, that the wind being N. and N. E. and tho' it has rained all day inceſſantly, the air has been as dry (ſo far as I could judge from natural hygrometers, and from my tube) as in a fair day; and than ſome fair days, drier, by the ſame indications.

I begin to think, that, by careful practice, the glaſs tube may be brought to be a good hygrometer for the air. I wiſh the theory of the air were more diligently and accurately conſidered: certainly it has been neglected; ſo Mr *Locke* thought, a little before he died; and ſaid, the imperfect diſcourſe of Mr *Boyle*'s, which was printed after his deceaſe, was the beſt account we had. And what has been done ſince?

I was going to tell you (for I write in a hurry, that I may not loſe the conveyance which offers), that I believe cuſhions, the caſe hair-cloth, and the ſtuffing of horſe-hair, may be made to anſwer inſtead of wax-cakes. I have one not 3 inches thick in the middle, even when it is not compreſſed, which will do well.

17. When I heard of Mr *Muſſchenbroeck's* experiment *, I tried the ſame; but I found great convulſions by it in my body. It put my blood into great agitation; ſo that I was afraid of an ardent fever; and was obliged to uſe refrigerating medicines. I felt a heavineſs in my head, as if I had a ſtone lying upon it. It gave me twice a bleeding at my noſe, to which I am not inclined. My wife, who had only received the electrical flaſh twice, found herſelf ſo weak after it, that ſhe could hardly walk. A week after, ſhe received only once the electrical flaſh; a few minutes after it ſhe bled at the noſe.

Extract of a letter from Mr John Hen. Winkler, *Gr. & Lat. Litt. Prof. publ. Ordin. at* Leipſick, *to a friend in* London; *concerning the effects of* Electricity *upon himſelf and his Wife.* N°. 480. p. 211. May *and* June 1746. *Dated* Leipſick, April 22. May 3. 1746. *Read* May 29. 1746.

I read in the news-papers from *Berlin*, that they had tried theſe electrical flaſhes upon a bird, and had made it ſuffer great pain thereby. I did not repeat this experiment; for I think it wrong to give ſuch pain to living creatures. I therefore take, inſtead of men or brutes, a piece of metal, and I put it upon a ſtand under the electrical pipe, which pipe propagates the Electricity. To this metal is faſtened an iron chain, which goes about the bottle with water, in which the braſs wire is put, which wire is faſtened to the electrical pipe.

When then the Electrification is made, the ſparks that fly from the pipe upon the metal are ſo large and ſo ſtrong, that they can be ſeen (even in the day time) and heard at the diſtance of 50 yards. They repreſent a beam like lightning, of a clear and compact line of fire; and they give a ſound that frightens the people that hear it.

18. While ſo many gentlemen are labouring to find out the uſes of Electricity, it has been my fortune to diſcover one, at leaſt, of the inconveniencies attending that property in glaſs. And as it is ſuch whereby vaſt numbers, very likely, have been, and may hereafter be, greatly prejudiced, I deſire you will mention what follows to the *Royal Society*; to the end that it may be publiſhed, if they think proper, for the benefit of others, and particularly of thoſe who uſe the ſea.

A letter to Mr Benj. Robins, *F. R. S. ſhewing that the* Electricity *of glaſs diſturbs the* Mariners

* That with the gun barrel ſuſpended as the iron bar. See above, Art. 5.

Having

Compaſs, *and alſo nice* Balances. *Ibid.* p. 242. *Dated* June 10. 1746. *Read* June 12. 1746.

Having lately had occaſion to compare together two compaſſes of a different make, the one having a bare needle, as uſual, and the other a chart, in the manner that mariners compaſſes are commonly made, I happened to wipe off with my finger ſome duſt, which lay upon the glaſs of the former; and thereby put the needle, which was before at reſt, into a violent diſorderly motion, partly horizontal, and partly vertical, or dipping. After ſeveral repetitions of the ſame thing, I found that the glaſs, by ſo ſlight a touch, was at that time excited to Electricity, ſo far as to diſturb the needle extremely.

The ſame glaſs being rubbed a very little more with a finger, a bit of muſlin, or of paper, would attract either end of the needle, ſo as to hold it to the glaſs, for ſeveral minutes, far out of the due direction, according to what part of the glaſs was moſt excited.

And when the needle has for ſome time adhered to the glaſs, and afterwards dropt looſe, and made vibrations, thoſe vibrations would not be biſſected, as uſual, by that point where the needle ſhould reſt, but either be made all on one ſide, or be very unequally divided, by means of ſome remains of electrical virtue in that part of the glaſs which had attracted the needle; until at length, after 15 minutes or more, all the Electricity being evaporated, the magnetical power took place.

The cure for this inconvenience, is to moiſten the ſurface of the glaſs: even a wet finger will do it immediately and effectually.

I need not ſuggeſt, that the ſame quantity of friction will not at all times have the ſame effect upon theſe glaſſes, any more than it will upon the electrical tubes; but take the liberty to hint, that I have reaſon to believe that glaſs does, at ſome times, become in ſome degree attractive without any friction at all; and may poſſibly be excited by great concuſſions in the air, ſuch as thunder, or the diſcharge of great ordnance, *&c.* and, if ſo, may thereby diſturb the compaſs.

I muſt however obſerve, that the mariners compaſs is much leſs dangerouſly moved by wiping or exciting the glaſs than the other; by reaſon that the excited part of the glaſs attracts that part of the chart which lies neareſt, juſt underneath, without giving it ſo much verticity, as it does to the other ſort of compaſs with a bare needle. And farther that the deeper, or the farther diſtant the needle hangs below the glaſs, the leſs diſturbance it is likely to receive, by wiping, rubbing or otherwiſe exciting the cover.

I ſhall make no farther reflections upon theſe facts than to obſerve, firſt, that all the minute, irregular, reciprocating variations which have been obſerved in the directions of dipping and horizontal needles, as mentioned in ſome of the *Tranſactions*, may probably have been cauſed by the glaſſes which covered the inſtruments made uſe of: and, ſecondly, that the flat pieces of glaſs, often placed under the ſcales of an eſſay-balance, are likewiſe very capable of attracting, and making even the lighter ſcale preponderate, where the whole matter weighed is ſo very ſmall. I have not tried this laſt, but do remember, that Mr *Ellicot*, a

Member

Member of your *Society*, did ſome years ago ſuſpect, if not find it certain, that ſuch pieces of glaſs did diſturb his balance, and had given him a vaſt deal of trouble, upon a ſuppoſition, that the beam itſelf was defective.

19. It ſeems to me that a glaſs ball, which has oftentimes been employed for violent diſtillations, and other chymical operations, does ſend forth the Electricity incomparably more ſtrong than any other glaſs, which never ſince it's making had been expoſed to a violent fire. As I am the firſt that has mentioned this notable circumſtance, be pleaſed to let me have the honour of this improvement in the Philoſ. Tranſ.

Extract of a letter from Mr Prof. Geo. Matthias Boſe *of* Wittemberg, *to Mr* W. Watſon, *F. R. S.* 1749. *Dated*

on the Electricity of glaſs, that has been expoſed to ſtrong fires. N°. 492. p. 189. Apr. &c. March $\frac{1}{12}$, 1748-9. *Read* April 6. 1749.

20. The electrifying glaſs uſed by M. *le Monnier* is an oblong ſpheroid, whoſe diameter from pole to pole is 4 or 5 inches longer than that at the equator, which is about 12 inches. Each of theſe poles is terminated in a ſtem, or portion of a hollow cylinder, about 3 inches in length, and one in diameter, ſpirally emboſſed on the outſide into a large male ſcrew: to each of theſe male ſcrews is adapted a female ſcrew of wood, cloſed at one extremity with a piece of ſteel, excavated in the center, to receive the ſteel pivots upon which the electrifying glaſs turns. Theſe female ſcrews of wood are ſo formed at their open extremity, that they graſp and cover as much at the poles, as nearly renders what appears of the glaſs ſpheroid a perfect ſphere: this with a deſign, that the wood may fix the more effectually, and embrace the electrifying glaſs. From the exterior ſurface of one of theſe wooden female ſcrews, a circular ledge riſes, and projects to the height of about 2 inches; the *ambitus* of which ledge is excavated, to receive the cord that turns the electrifying glaſs. This is what they uſe here inſtead of our tubes, and with ſurpriſing effects, ſuch as greatly ſurpaſs what you have yet ſeen in *England*. The electrifying ſpheroid is turned by means of a wheel about 4 feet in diameter, with the ſame motion, and exactly in the ſame manner, as the ſpindle is turned round by the ſpinning-wheel: allowing a due proportion to the frame, upon which the glaſs ſpheroid is mounted, that it may anſwer to the wheel that turns it. The ſides of this frame, which ſtand perpendicular to the horizon, are near as ſtrong and as large every way, as the poſts of an ordinary cloſet-door; and, with the ledges that join them at top and bottom, form a rectangular parallelogram. The front of this frame is provided with ſilken loops, conveniently diſpoſed in ſeveral places, to bring to, and fix at a contact with the electrifying glaſs, wires, threads, packthread, or whatever elſe is to be electrified. Into one ſide of this frame, at about half it's height, the pivot that receives one of the poles of the glaſs ſpheroid is fixed; the other pivot, on the oppoſite ſide, is a round long bar of iron, ſcrewed into and paſſing through the poſt, in order to fix, or give liberty of removing the

Extract of a letter from Mr Turberville Needham *to* M. Folkes, *Eſq; P. R. S. concerning ſome new electrical Experiments lately made at* Paris. N°. 481. p. 247. Oct. &c. 1746. *Dated* Paris, *July* 4. N. S. 1746. *Read* Oct 23. 1746.

electrifying glaſs. This bar of iron, for the conveniency of turning it, has another in the nature of a leaver, which paſſes through it's extremity at right angles with it. The whole machine is mounted upon a floor of boards, wheel, frame, glaſs, *&c.* and employs two men, the one to turn the wheel, the other to ſit behind the glaſs ſpheroid, and apply the concave of each hand to it's lower convex ſurface; for it is by this friction that the Electricity is excited.

When the electrifying glaſs has been ſome little time in motion, the perſon who deſires to be electrified, applies the extremities of the nails of one hand, and ſtands not upon cakes of wax, as in *England*, but within the area of a ſquare drawer or box about five inches deep, and filled with five parts pitch, four of reſin, and one of bees-wax: I will not call it a compoſition, for they are not mixed, but diſpoſed in the following manner; the pitch is placed next to the ſides of the box, and riſes almoſt to a level with them, the reſin in the middle is level with the pitch, and the wax forms a thin ſurface, covering both to a level with the box it ſelf; however, I ſuppoſe this to be in itſelf very indifferent, and that any one body of the electrics *per ſe* would anſwer equally.

Exp. I. The perſon electrified by this machine not only emits fire from all parts of his body, upon the touch of another, with more vigour, and in a much more ſenſible manner, than when electrified by a common tube; but fires alſo ſpirits of wine with ſuch eaſe, that when the ſpirits have been once but ſimply ſet on fire by a match or paper lighted, and the flame has been inſtantly blown out, they will, with that ſmall degree of heat they have acquired, take fire upon his touch 10 or 20 times ſucceſſively, without failing once.

I am told here, that they have frequently attempted in vain to fire ſpirits with a common tube of glaſs; ſo that I believe the uſe of the tube has been more improved in *England* than in any other place: but it is a downright ſlavery, and in it's effects many degrees inferior to this machine. I ſhould have thought, as this ſo much exceeds in ſtrength the common tube, that many glaſs ſpheroids, acting at once upon the ſame body, would have conſiderably increaſed the effect; but M. *de Buffon* tells me, that M. *le Monnier* had found, upon trial, that they anſwered not his expectations; ſo that it might ſeem there is a *ne plus ultra* in the intenſity of Electricity, as well as in the heat, which is communicated to boiling water.

Exp. II. If the perſon electrified holds a ſword in one hand, the chamber being darkened, a continual flame iſſues out at the point, in ſmell and colour reſembling the fumes of *phoſphorous*, and near as ſtrong as that of an enameller's lamp: with this difference, that when any other of the company applies a hand, even to the very point, where the concentred rays begin to diverge, it burns not, nor is any otherwiſe ſenſible to the feeling, than as a continual blaſt of wind.

Exp. III. This is performed with a ſquare bar of iron, about 4 feet in length, and $\frac{1}{4}$ an inch in thickneſs; to one extremity of which is adapted, by the

the help of a ſcrew, another piece of iron beat flat, like the end of one of the legs of a pair of tongs. This flat piece of iron being ſcrewed in, the bar is placed parallel to the horizon upon a wooden ſtand, and the ſtand within the area of the drawer or box, upon the pitch, reſin, and bees-wax, as above. The extremity of the bar, oppoſite to that, which carries the flat piece of iron, is covered with 3 or 4 folds of linen, to prevent any damage that might happen to the glaſs ſpheroid, in hitting againſt it by accident, while it revolves round it's axis; and the ſame extremity is moreover, for further ſecurity, placed at the diſtance of about $\frac{1}{4}$ of an inch from the glaſs itſelf, the effect being the ſame in every reſpect, as if in contact. The operator then orders the bar to be electrified by repeated revolutions of the glaſs ſpheroid, as above; and places one finger upon the middle of the bar, to prevent the communication of the Electricity from one end to the other, till he has covered the flat piece of iron with as much ſaw-duſt as it will carry. Some other of the company, in the mean while, takes up, on the point of a knife likewiſe, a quantity of ſaw-duſt, and holds it under the flat piece of iron, at about an inch diſtance. The effect is, that when the operator takes off his finger, the ſpheroid ſtill continuing to revolve, the ſaw-duſt above is all repelled and blown off, and that under attracted upwards. If, inſtead of ſaw-duſt, you place upon the flat piece of iron a ſmall ſquare tin box filled with water, or any other veſſel made of a matter non-electric *per ſe*, particularly metalline, and endeavour to draw off the water by a capillary ſiphon: the water, in that caſe, will fall drop by drop, as uſually; but the inſtant the bar is electrified, it will run in one continual ſtream; which, if the chamber be darkened, will alſo appear luminous. This play of the water may again be ſtopped at pleaſure, by the application of one finger to the bar, as above. If the flat piece of iron be unſcrewed and removed, the Electricity runs out at the extremity of the bar, to the eyes, in the appearance of a blueiſh flame; to the ſmell, like fumes of *phoſphorus*; and, to the feeling, like a blaſt of wind; as in the experiment of the ſword.

The moſt ſurpriſing of all, is that of Mr *Muſſchenbroeck*, improved by M. *le Monnier*. A muſquet-barrel open at both ends, is ſuſpended parallel to the horizon, by ſilken threads within reach: and at the breech end, about 3 inches from the extremity, is hung, by a ring of iron worked into the barrel itſelf, a ſmall iron chain about $\frac{1}{2}$ a foot in length. A glaſs phial, reſembling in ſize and ſhape a common vinegar-crewet, is then prepared, full of water and well corked, with an iron wire running through the cork almoſt to the bottom, and emerging ſome two or three inches above it, out of the top of the phial. The head of this wire is bent, to catch in the loweſt link of the chain; and is there to be ſuſpended, when it has been electrified. From the mouth of the barrel, which is pointed in a line parallel to the equatorial plane of the revolving ſpheroid, comes a long iron wire, inſerted into the barrel itſelf, as far as $\frac{1}{3}$ of it's length, and thence proceeding till it touches the glaſs ſpheroid; to a contact with Exp. IV.

which it is determined by one of the ſilken loops I mentioned above in the deſcription of the *apparatus*. Every thing being thus diſpoſed, the gun-barrel is to be electrified by repeated revolutions of the glaſs ſpheroid; which is to be in a continual contact with the long wire that proceeds from it. The phial is, at the ſame time, to be electrified by the operator, who takes hold of the body of the bottle, and applies to the electrifying ſpheroid the bent extremity of that wire, which paſſes from near the bottom of the phial through the cork, as I deſcribed above. The operator muſt take care not to touch the wire itſelf, while he endeavours to electrify the phial; otherwiſe he would be in the caſe of one, who ſhould aim to electrify himſelf, without ſtanding upon ſome one of the bodies, that are electrics *per ſe*. When the phial is ſufficiently electrified, which will be done in 8 or 10 revolutions of the ſpheroid; for I would not have any one be too free in beſtowing ſuch an efficacy upon it by too long an application, as might perhaps, occaſion his receiving a more violent ſhock than he would be willing to feel, particularly if the glaſs ſpheroid has been any time in action, and is much heated thereby; the phial is then, I ſay, to be ſuſpended by the iron chain, the glaſs ſpheroid continuing ſtill to revolve about it's axis, and to electrify the gun-barrel: the perſon then who has courage enough to ſuffer the experiment, for ſo I muſt expreſs myſelf, graſps the bottom of the electrified phial with one hand, and with the other touches the gun-barrel. At that inſtant, a great part of the nervous ſyſtem receives a ſhock ſo violent, that it would force the ſtrongeſt man to quit his hold, and turn him half-round.

I remember, among others of us, that tried the experiment, was a boy of about 14: I aſked him, what he thought of it; he told me, that he imagined, the inſtant he touched the gun-barrel, his arms had been broke ſhort off at the elbows, and that he had been cut into two parts juſt below the breaſt; another of the company, with a ſort of pun, termed it being broken upon the wheel. In effect, ſo far the boy was in the right, that the ſhock in the arms ſeems to extend no farther than the elbows, and that of the body no lower than the breaſt, without affecting however in the leaſt the head, or ſeeming to reach beyond the outward expanſion of the nerves: yet is it not to be termed a pain; for there is not the leaſt ſenſe of that ſort in it, but a mere ſudden convulſionary motion, or rather a ſhock, which ſurpriſes much, and is indeed an uneaſy, though not a painful ſenſation.

In this experiment, it is very remarkable, how greatly the force of the communicated Electricity is augmented, by the application of the electrified phial: but the moſt ſurpriſing circumſtance attending the uſe thereof, and which, I believe, is, among all the bodies that are ſuſceptible of Electricity, peculiar to this alone, is, that it loſes not entirely it's efficacy under ſeveral minutes; and I am told, that in a froſt it will retain it for 36 hours together.

M. *de Buffon*, who informed me that M. *le Monnier* was the firſt who diſcovered this particular, has alſo aſſured me, that this ſame gentleman had frequently-

frequently electrified the phial at home, and brought it in his hand through many ſtreets from the college of *Harcourt*, to his apartments in the king's garden, without any very ſenſible diminution of it's efficacy. The uſe of the electrified phial may be diverſified many ways: among others, are ſuch as follow.

When the phial has been ſufficiently electrified as above, the whole company join hands; the operator at one extremity of the line graſps the bottom of the electrified phial, and the perſon at the other extremity touches the wire, which riſes above the cork. At that inſtant, the whole company receives a ſhock, reſembling that in the experiment of the gun-barrel, but not ſo ſtrong; for it ſeems not at all to extend beyond the elbows. Exp. V.

This is the experiment, which abbé *Nollet* performed upon 180 of the guards, before the king, who were all ſo ſenſible of it at the ſame inſtant of time, that the ſurprize cauſed them all to ſpring up at once; as it will indeed force any perſon to do that ſubjects himſelf to the trial; though the convulſionary motion itſelf, as I obſerved before, reaches not beyond the elbows: but the greater or leſſer effect depends entirely upon the longer or ſhorter application of the phial to the electrifying ſperoid; and I am credibly informed, that when due precautions have not been taken in this particular, ſome perſons have received ſuch violent ſhocks, as have benumbed, and impaired, to a certain degree, the uſe of their arms for a day or two, before they perfectly recovered themſelves. I can aſſure you, however, from my own experience, that, with the precautions I have already taken notice of, there is no manner of danger, though at the ſame time a ſufficient efficacy may be communicated to the phial, to gratify any one's curioſity: and in this particular I have been the more prolix, leſt any bad conſequence ſhould happen to the unexperienced.

Another experiment with the electrified phial conſiſts, firſt, in placing a wire fixed in a pedeſtal, erect in a baſon of water, the head of which wire is bent, and riſes ſome 3 or 4 inches above the level of the water; and then, in touching the ſurface of the water with one hand, and the ſtanding wire with the wire of the electrified phial, which is graſped by the other hand, as in the preceding experiments. The effect of this is much more violent than that of the laſt experiment, and I think, exceeds even the ſhock of the gun-barrel; ſo that here the utmoſt precaution muſt be uſed, not to electrify the phial too much. Exp. VI.

I obſerved particularly upon the trial of this, that the operator, who appeared to be very expert, and quite familiarized with every former effect, ſhewed however ſome apprehenſion, and was unwilling to lead the way, as he had done in all the other experiments.

If the electrified phial is held in the hand, and the chamber is darkened, the wire inſerted in it is perceived to emit a ſtream of fire at it's extremity without any diſcontinuance; but if it is ſuſpended by a ſilken thread, the fiery eruption inſtantly ceaſes. Exp. VII.

This,

This, as a perſon would be apt to imagine, gives ſome inſight into the reaſon of it's retenſion of Electricity; the ambient glaſs and ſilken thread being in the number of the electrics *per ſe*, which have a power of determining to, and confining in, any other kind of body, a communicated Electricity, though they are not ſuſceptible of it themſelves. Yet, as the *French* obſerve very well, there are ſo many of what they term *bizarreries*, or unaccountable *phænomena*, in the courſe of electrical experiments, that a man can ſcarce aſſert any thing, in conſequence of any experiment, which is not contradicted by ſome unexpected occurrence in another: at leaſt, this is my preſent thought of the matter; and I am the more confident in advancing it, ſince that I have learnt your friend M. *de Buffon* is of the ſame opinion, for whoſe judgment I have the greateſt deference. I remember he told me one day, when I had the honour of waiting upon him, that he thought the whole ſubject of Electricity, though illuſtrated with ſo great a variety of experiments, very far from being yet ſufficiently ripe for the eſtabliſhment of a courſe of laws, or indeed of any certain one, fixed and determined in all it's circumſtances. An inſtance of this, among others that are or may be found out, will appear in the following experiment.

Exp. VIII. If the non-electrified phial is placed upon a glaſs ſalver, it acquires from the revolution of the ſpheroid no Electricity, though it's wire is in contact with it all the time; unleſs the finger of ſome one in the company is approached very near to the phial itſelf: but, in that caſe, it receives it viſibly from the finger; inſomuch that, if the chamber is darkened, you will ſee the electrical fire ſtreaming out of the finger, and entering into the water, through the body of the glaſs phial, which is thereby immediately impregnated with it; and this, though the hand ſhould be placed even under the glaſs ſalver itſelf.

Here we ſee an example, where an electric *per ſe* is ſo far from terminating or excluding the power of Electricity, that it is even made a *medium* of communication in circumſtances where the wire, which is a non-electric *per ſe*, refuſes to perform it's expected office. When I ſpeak of the power of Electricity in this caſe, I would not be underſtood of the power of attracting light bodies, which is well known to be ſcarce ſenſibly interrupted by a glaſs *medium*, as appears in the common experiment of an electrified tube, acting upon leaf-gold, in a cryſtal bottle: though even this, if duly conſidered, might create ſome difficulty; but I would only be underſtood of that communicated virtue, which renders non-electrics *per ſe* electrical. In one word, the ſingularity of this experiment is, that, by the addition of the glaſs ſalver, the wire and the water, both of them non-electrics *per ſe*, ſhould not be in the leaſt affected without the approach of the hand, and ſhould then receive the electrical fire from it through a glaſs *medium*; notwithſtanding they are in the very ſame circumſtances, that a man is in, or any other non-electric *per ſe*, placed upon a cake of wax and in contact with the electrifying ſpheroid. Now, that in this experiment the glaſs ſalver has a conſiderable effect, is very clear. For, if the phial is placed upon the table, or upon a ſtand, without the

ſalver,

ſalver, a few revolutions of the ſpheroid will with eaſe communicate a ſtrong Electricity to it; particularly if any one touches the table or ſtand it is placed upon: and to know whether any degree of Electricity has been communicated or not, the phial is to be brought to the teſt of any of the preceding experiments.

If the electrified phial is placed upon a table, and any light body is ſuſpended by a ſilver thread, within the diſtance of about 2 inches from the phial, what I ſaw was a ſmall braſs bell of a lap-dog's collar, the phial will attract that light body to it with force, if anyof the company touch the wire of the phial; but if the phial itſelf is touched, it will repel it with a force equal to its attraction in the former caſe. Exp. IX.

This experiment conſiſts in the communication of the electrical fire from the glaſs ſpheroid to many perſons at once, as in *England*, from a tube; with this only difference, that the company do not here join hands, but are united to each other by taking hold of iron chains, which ſurpriſingly increaſe the force of the communicated Electricity: for it is to be obſerved, that, whenever the communication is carried on by a metallic *medium*, the effects are much the more ſenſible. Exp. X.

This experiment is no other than what has been frequently tried in *England*, the attraction of leaf-gold by a hollow wooden globe, to which Electricity is communicated, by a packthread of a very great length ſuſpending it; after it has been conducted over ſilken threads croſſing the chamber at ſeveral diſtances, in a ſort of ſpiral, conſiſting of as many turns as the place will admit. Exp. XI.

I had almoſt forgot to take notice of two particulars, which were the conſequences of ſome of the preceding experiments, and may in ſome meaſure ſerve to illuſtrate them: the one regards the communication of Electricity; the other, it's ſurpriſing force.

At the grand convent of the *Carthuſians* here in *Paris*, the whole community formed a line of 900 toiſes, by means of iron wires of a proportionable length, between every 2; and, conſequently, far exceeding the line of the 180 of the guards above-mentioned. The effect was, that, when the two extremities of this long line met in contact with the electrified phial, the whole company, at the ſame inſtant of time, gave a ſudden ſpring, and all equally felt the ſhock, that was the conſequence of the experiment.

The other *phænomenon* was the reſult of a late experiment of abbé *Nollet*'s. He fixed, at the two extremities of a braſs ruler, two ſmall birds, a ſparrow and a chaffinch: this ruler had a handle or pedeſtal faſtened to the middle of it, for the convenience of holding it. When both the gun-barrel and the phial had been ſufficiently electrified, as in the 4th experiment, he applied the head of the ſparrow to the ſuſpended phial, and the head of the chaffinch to the barrel. The conſequence, upon the firſt trial, was, that they were both inſtantaneouſly ſtruck lifeleſs, as it were, and motionleſs, for a time only, and they recovered ſome few minutes after: but, upon a ſecond trial, the ſparrow was ſtruck dead, and, upon examination,

nation, found livid without, as if killed with a flash of lightning, moſt of the blood-veſſels within the body being burſt by the ſhock. The chaffinch revived, as before.

Extract of a memoir concerning the communication of Electricity; *read at the public meeting of the* R. Acad. *of* Sciences *at* Paris, Nov. 12 1746 *by M.* le Monnier *the younger, M. D of that* Academy, *and F R. S. communicated by the Author to the* Pre. *of the* R. S. *Ibid* p. 290. *Read* Dec 11. 1746.

21. The author of this memoir propoſes therein to examine theſe 3 queſtions; that is to ſay, how is this electric virtue to be communicated to ſuch bodies as have it not, and which are not capable of acquiring it by bare friction only? How is the electric matter propagated? And, laſtly, in what proportion is it diſtributed?

As to the firſt, the author obſerves, that this electric virtue is no other way to be communicated, but by the near approach of a body already actually poſſeſſed of the ſame: That the rule laid down by M. *du Fay*, *that bodies never receive Electricity by communication, unleſs they are ſupported by bodies electric in their own nature*, does not always take place, and that it is ſubject to great exceptions. For, firſt, in the *Leyden* experiment, the phial filled with water is ſtrongly electrified by communication, even when carried in the hand, which is not a body electric by nature. Secondly, all bodies that are electrified by means of a phial of water fitted to a wire, and which has already received a great degree of virtue by communication; all ſuch bodies, I ſay, placed in any curve line, connecting the exterior wire, and that part of the bottle which is below the ſurface of the water, acquire Electricity, without being placed upon reſin, ſilk, glaſs, or the like.

Thus one may give a violent concuſſion in both the arms to 200 men all at once, who holding each other by the hand, ſo form the curve juſt mentioned, when the firſt holds the bottle, and the laſt touches the wire with the end of his finger; and this, whether theſe perſons actually touch each other's hands, or whether they are connected by iron chains, that either dip in water, or drag upon the ground; whether they are all mounted on cakes of reſin, or whether they only ſtand on the floor; in all which caſes the experiment equally ſucceeds.

Electricity has in this manner been carried through a wire of the length of 2000 toiſes, that is to ſay, of about a *Paris* league, or near 2 *Engliſh* miles $\frac{1}{2}$, tho' part of the wire dragged upon wet graſs, went over charmil hedges or paliſades, and over ground newly ploughed up.

Thirdly, the water of the baſon in the *Thuilleries*, whoſe ſurface is about an acre, has been electrified in the following manner: there was ſtretched round half the circumference of the baſon an iron chain, which was intirely out of the water: the two extremities of this chain anſwered to thoſe of one of the diameters of the octogon: an obſerver, placed at one of theſe extremities, held the chain with his left hand, and dipped his right at the ſame time into the water of the baſon; whilſt another obſerver, at the oppoſite ſide of the baſon, held the other end of the chain in his right hand, and a phial well electrified in his left: he then cauſed the wire of his phial to touch an iron rod, fixed upright in a piece of cork that floated near the edge of the baſon; at that inſtant both obſervers felt a violent ſhock in both their arms. This ſame fact

was

was again confirmed, by experiments made upon two baſons at the ſame time, that it might appear diſtinctly, that the electrical *effluvia* did really paſs along the ſuperficies of the water.

Fourthly, it has been confirmed, by repeated compariſons, that a bar of iron placed in the above-mentioned curve, does not at all acquire more Electricity, when it is ſuſpended in ſilken lines, than when it is held in the bare hand. Whence it appears, that, in this caſe, the contiguous non-electric bodies do neither partake of, nor abſorb in any way, the Electricity that has been communicated.

Beſides many ſtrong exceptions to the rule laid down by M. *du Fay*, the author adds another yet ſtronger, and indeed directly contrary to that rule; which is, that the ſame phial of water, fitted with it's wire, receives either no virtue at all, or at leaſt none that is ſenſible, ſo long as it is either placed upon a ſtand of glaſs that is very dry, or that it is ſuſpended by a ſilken thread, whilſt it's wire reſts upon the globe; and that, to make it receive the virtue, the part of the phial which is below the ſurface of the water, muſt communicate with ſome body that is not electric; as is evident, when it is touched, whilſt it reſts on the ſtand of glaſs, with the finger, for it then inſtantly becomes electric: and the ſame will alſo happen when it is touched with a peice of metal; but not when it is touched with a tube of glaſs that is dry.

The electrical reſts produce here upon the bottle an effect ſo contrary to M. *du Fay*'s rule, that, if one places a phial, perfectly well electrified, and which throws out the pencil of fire copiouſly, upon a dry ſtand of glaſs, or upon a line of ſilk; it's light immediately goes out, and it's Electricity is as it were laid to ſleep. One may then ſecurely approach the finger to it's wire, and there will come no electrical ſparks from it. The author has even drawn out of it entirely both the wire and the cork, and has kept it half an hour in his pocket, without deſtroying the Electricity. But one muſt only, in this caſe, touch the wire, and not the phial itſelf; for in touching the two at the ſame time, one returns to the *Leyden* experiment; but when one touches the phial only, the Electricity revives in the wire, and the pencil of fire diſplays itſelf again, provided one has not ſtaid too long: but if the wire only is touched, the body of the bottle becomes ſtrongly electric, and draws to it, from a conſiderable diſtance, any light ſubſtances.

This laſt caſe gives room to an experiment that looks at firſt like magic: there was hung up a little tinkling bell by a ſilver wire, at the height of 8 or 9 feet, and there was placed upon a glaſs ſtand well dried, a phial newly electrified; the centre of the bell, and that of the phial, were nearly in the ſame horizontal line; but the bell was between 6 and 7 inches from the ſurface of the phial. Every thing being in this ſtate, the bell remained quite ſtill, if the ſtand was very dry; but the inſtant one either approached a finger, or any other non-electric body, to the wire of the phial, the bell leaped to it: and one might

begin again, and repeat the experiment 20 times together, without having any occaſion to new-electrify the phial.

With regard to the propagation of Electricity, the velocity with which the electrical matter is conveyed, has been found too great to be yet determined with any exactneſs.

The author made an experiment with an iron wire of 950 toiſes in length, and he was not able to obſerve, that there paſſed ſo much as a quarter of a ſecond of time, between the wires receiving the Electricity at one end, and his feeling the ſhock in both his arms at the other; which infers a velocity at leaſt 30 times as great as that with which ſounds are propagated.

In ſeeking what might be the force which ſhot forward the electric matter, with ſo much rapidity, through the length of the wire, he at firſt thought it might be performed by the exploſion of the ſpark of fire, which is perceived when the electrified phial is brought into contact with the wire conducting the electric matter; but the following experiment ſoon convinced him he was miſtaken.

He diſpoſed horizontally a wire folded in two, upon lines of ſilk; the whole length of this wire was of 1319 feet, and the two parallel halves were at the diſtance from each other of about ſix feet: the Electricity was then communicated by means of a phial, and it preſerved itſelf in the wire for ſeveral minutes, by reaſon of the ſilken lines upon which the ſame was ſupported: a finger was then brought to one of it's extremities to take away the virtue; and in the ſame inſtant it ceaſed alſo at the other extremity of the wire: ſo that, in this caſe, the matter in queſtion returned to the finger, that is to ſay, marched backward, with the ſame velocity with which it was before ſhot forwards: the electric matter therefore now came towards the exploſive ſpark, for this ſpark appeared upon the finger as ſoon as it approached the end of the wire to take away it's Electricity, and therefore it is not this ſpark which ſhoots forward the electric matter with ſo great a velocity.

The laſt part of the memoir concerns the proportion in which the electric matter is communicated to bodies of the ſame nature. And here the author firſt eſtabliſhes, that it is not communicated to homogeneous bodies, in proportion to their maſſes or quantities of matter, but rather in proportion to their ſurfaces. Yet all bodies having equal ſurfaces do not receive equal quantities of Electricity: thoſe receive the moſt, whoſe ſurfaces are extended the moſt in length. Thus a ſquare ſheet of lead receives a much leſs quantity of Electricity, than a ſtrip of the ſame metal with a ſurface equal to that of the ſquare ſheet: inſomuch that the only way to increaſe in any body it's faculty of receiving the electric virtue, is continually to increaſe it's length.

Obſervations upon ſo much of the preceding Article,

22. The world is much obliged to M. *le Monnier* for the many diſcoveries he has made of the power of Electricity; though the reaſon of my troubling you with this paper at this time, is my differing with that gentleman

gentleman in the concluſions which he deduces from ſeveral of the experiments contained in his memoir.

as relates to the communicating the electric virtue *to* non-electrics, *by* Wm Watſon, *F. R. S.* N°. 482. p. 388. Jan. *and* Feb. 1746. *Read* Jan. 29. 1746-7.

One of the queſtions propoſed to be examined is, " in what manner " the electric virtue is to be communicated to ſuch bodies as yet have " it not, and which are not capable of acquiring it by bare friction " only?" M. *le Monnier* obſerves hereupon, " That no other manner " is known, by which the electric virtue may be communicated, beſides " the near approach of a body actually poſſeſſed of the ſame: that the " rule laid down by M. *du Fay*, *that bodies never receive Electricity by* " *communication*, *unleſs they are ſupported by bodies electric in their own* " *nature*, *does not always take place*; *and that it is liable to great excep-* " *tions*: for, firſt, in the *Leyden* experiment, the phial filled with water " is ſtrongly electrified by communication, even when carried in the " hand, which is not a body electric by nature."

To this I anſwer, that M. *du Fay*'s rule is confirmed by all the experiments yet made public, and even by that of *Leyden* quoted by our author, or what is uſually called that of Profeſſor *Muſſchenbroeck*. For, in this experiment, is not the non-electric water contained in and ſupported by the glaſs phial, which is electric in it's own nature? It's being carried in the hand is no more than it's being placed on any other non-electric body, and therefore is no proof againſt the general poſition. It is well known, that if the phial is made non-electric by wetting it's outſide, ſo as not to leave ſome inches perfectly dry, between it's mouth and that part which is wetted, the water and phial part with the Electricity as faſt as they receive it, unleſs it is ſtopped by another electric *per ſe*. But of this I treated at large, in a paper I lately did myſelf the honour to communicate.

Secondly, our author mentions, " that all bodies, which are electri- " fied by means of a phial of water fitted to a wire, and which has al- " ready received a great deal of virtue by communication; all bodies, " he ſays, placed in any curve line, connecting the exterior wire and " that part of the bottle, which is below the ſurface of the water, ac- " quire Electricity without being placed upon reſin, ſilk, glaſs, or the " like: that thus a violent concuſſion may be given to 200 men all at " once; who holding each other by the hand ſo form the curve juſt " mentioned, when the firſt holds the bottle, and the laſt touches the " wire with the end of his finger; and this equally, whether they are " all mounted upon cakes of reſin, or ſtand upon the floor: that the " Electricity has in this manner been carried through a wire of the " length of 2000 toiſes, or near $2\frac{1}{2}$ *Engliſh* miles; part of which wire " dragged upon wet graſs, went over hedges, paliſado's, and over land " newly ploughed up."

The experiments in the ſecond argument do no ways invalidate M. *du Fay*'s rule; for the ſucceſs of them depends upon keeping whatever forms the curve line mentioned by our author, whether it conſiſts of men or wire, in a non-electric ſtate: and if whatever forms this curve

line acquires any degree of Electricity more than it's original quantity, which it is well known may be done, by being placed upon originally electrics, the effect of the ſhock is proportionably leſſened. Thus if a man, ſtanding upon electrics *per ſe*, applies his hand to the phial of water, ſuſpended by a wire to the electrified gun-barrel as uſual, this perſon will acquire Electricity, which will be ſufficiently perceptible in him, by his attracting light ſubſtances held near his body, or by his firing inflammable ones, when properly preſented to him; if, I ſay, a perſon thus electrified, by applying one of his hands to the phial, touches the electrified gun-barrel with a finger of his other, let the phial be ever ſo ſtrongly electrified, he feels but a ſlight ſtroke; and this ſtroke is greater or leſs, in proportion to the difference of the accumulation of Electricity in the body of the man, and that of the water in the phial. Thus we know from experiment, that though a conſiderable quantity of the Electricity, in impregnating the phial of water therewith, pervades the glaſs, yet the loſs thereof this way is not equal to what comes in by the wire: therefore we will, for the ſake of a more eaſy method of explanation, ſuppoſe, that the phial, when electrified in the moſt perfect manner, contains a quantity of Electricity equal to 10; that the man's body, by ſtanding upon wax, and touching the phial with one of his hands during it's Electrification, contains a quantity equal to 7: upon his touching the gun-barrel with a finger of his other hand, he will receive a ſmall ſtroke only equal to 3, the difference of the Electricity of the water and that of his body: and if he touches the gun-barrel again without removing his foot from the originally electric, the ſtroke will be ſcarcely perceptible, on account of his body being nearly of the ſame degree of Electricity with the water in the phial. So that here we ſee that the violence of the ſhock, to be felt by whatever forms the curve line, depends upon it's being, in the moſt perfect manner, free from any degree of Electricity more than the original quantity, which is contrary to the opinion of our author.

Thirdly, M. *Monnier* tells us, "That the water of the baſon of the "*Thuilleries*, whoſe ſurface is about an acre, has been electrified in the "following manner:

"There was ſtretched round half the circumference of the baſon an "iron chain, &c."

The water of the baſon in this experiment was no more electrified than the wire which dragged along the ground, *&c.* was in the former. When I was firſt informed, without being acquainted how, that an acre of water had been electrified, I was amazed, and told the gentleman who acquainted me therewith, that if my idea of Electricity was in the leaſt true, ſuch an effect could not be produced, without electrifiing the whole terraqueous globe from a larger maſs of matter. And indeed, when I heard M. *le Monnier*'s paper read I eaſily ſaw the deception: ſo that, inſtead of electrifying the whole quantity of water contained in the baſon,

the

the Electricity paſſed only through ſo much of it as formed a line between the iron rod faſtened in the floating cork, and the hand of that obſerver which was dipped in the water.

Theſe experiments ſtill more and more eſtabliſh the account I lately laid before you of the Electricity's always deſcribing the ſhorteſt circuit between the electrified water and the gun-barrel; or (which is the ſame thing) the wire of the electrified phial. And this operation reſpects neither fluids or ſolids, as ſuch, but only as they are non-electric matter. Thus this circuit, in the preceding experiment between the phial and the wire, conſiſted of the two obſervers, the iron chain, the line of water, and the iron rod in the floating cork.

Fourthly, M. *le Monnier* mentions, "That it has been confirmed, by "repeated compariſons, that a bar of iron, placed in the above-men-"tioned curve, does not at all acquire more Electricity when it is ſuſ-"pended in ſilken lines, than when it is held in the bare hand: whence "it appears to him, that, in this caſe, the contiguous non-electric bodies "do neither partake of, nor abſorb in any way, the Electricity which has "been communicated."

The curve line before-mentioned, let it conſiſt of whatever non-electrics it will, unleſs the whole thereof be properly ſupported, the communicated Electricity cannot be accumulated: ſo that the ſuſpending one part thereof in ſilk lines cannot be ſuppoſed to produce any effect.

This gentleman further obſerves, "That the phial of water fitted to "it's wire does not receive the leaſt degree of Electricity, if it's wire, ſuſ-"pended by a ſilk line, is applied to the globe in motion, or if that phial "is placed upon a dry glaſs ſtand." This M. *le Monnier* takes to be directly contrary to M. *du Fay*'s rule; eſpecially as the phial cannot be replete with Electricity, unleſs, while it is exciting, ſome non-electric body touches the phial below the water.

That the phial of water receives no degree of electricity in this caſe, is not ſtrictly true: it receives as much as any other maſs of matter of the ſame bulk would, under the ſame circumſtances. For we find, that we cannot highly electrify the water, unleſs the Electricity from the globe be directed through the water and phial to the non-electric in contact; in which paſſage a great quantity thereof is accumulated, by it's not pervading the glaſs ſo faſt as it is furniſhed by the wire; and therefore we find, that when the water will contain no more, the ſurcharge runs off by the wire: ſo that this experiment, no more than thoſe which precede, contradicts M. *du Fay*'s opinion; the thinneſs of the glaſs permitting it, not wholly, but partially, to ſtop the Electricity. This matter is explained further under experiment the firſt.

I differ from this ingenious author with reluctance, inaſmuch as I greatly honour him, not only for his diſcoveries upon the ſubject of Electricity, but alſo for the pleaſure and improvement I received in my reading his learned and curious obſervations in Natural Hiſtory, made in the ſouthern parts of *France*, where he accompanied M. *Caſſini de Thury* in meaſuring

meaſuring a degree of the meridian. Theſe obſervations are publiſhed with M. *Caſſini*'s book: but as the reverſe of ſeveral of the opinions delivered in his memoir is experimantally found to be true, and as the diſcovery of truth, and carefully ſeparating it from deception, ſhould be the only aim of our philoſophizing, I take the liberty of laying before you my opinion thereon.

Part of a letter from Mr John Browning, *of* Briſtol, *to Mr* Henry Baker, *F R. S. Dated* Dec. 11. 1746. *concerning the effect of* Electricity *on* Vegetables. *Ibid.* p. 373. *Read* Jan. 22. 1746-7.

23. Having an operator at *Briſtol* with a good electrifying machine, I was deſirous to electriſe a tree, and therefore ſent him the following for that purpoſe; *laurus tinus*, *leucoium majus flore pleno ferrugineo*, and *ſtæchas citrina Cretica*. Theſe were not choſen with any deſign; their being the leaſt plants I had, was the only reaſon.

I promiſed myſelf the pleaſure of ſeeing their leaves ereeted when electriſed, but was diſappointed, (whether it's being the dormant ſeaſon of the year for all plants, might not be ſome hindrance, I cannot determine); neither did the leaves flag on their being touched. However, I was agreeably recompenſed by a ſtream of fine purple blue coloured light, much reſembling an amethyſt, that iſſued from the extremity of each leaf upwards, of an inch in length, when the finger, or any other non-electric, approached near it. This colour I attribute to the watry particles in the earth, having often obſerved the very ſame colour iſſuing from the long leg of a ſyphon. On putting my finger on the gun-barrel to ſtop the Electricity, the leaves of each tree had a trembling motion, which remained for ſome little time, and immediately ceaſed on withdrawing my finger from the barrel, and admitting the Electricity. This conſtantly happened, as I put my finger on or off the barrel.

The *ſtæchas* plant has a very long hoary leaf, and bears it's bloſſom on a very ſmall, ſlender, and almoſt naked ſtem, riſing near a foot above the body of the plant. This ſtem had a motion given it, when any non-electric was brought within about two inches of it's ſummit, much like the vibration of the *pendulum* of a clock; which vibrating motion was parallel with the breech of the gun, quite contrary to the ſame kind of motion I had before obſerved in a needle, hanging perpendicularly by a thread at the end of a gun; the needle always vibrating in the direction of the gun. The motion of the plant and needle always continued as long as the glaſs globe was excited.

I was alſo deſirous to be ſatisfied, whether Electricity could be propagated without mutual contact, by ſuſpending another gun in ſilk cords, about 2 inches from contact, and the Electricity was near as ſtrong in the ſecond gun as in the firſt. At the diſtance of between 3 and 4 inches, it was much abated, and ſo it gradually diminiſhed, as the diſtance increaſed to near 6 inches, where it would ſcarce attract a thread of trial.

I prevailed on a man to be let blood, and then placed him on a cake of pitch, but could not be ſenſible of any increaſe of velocity in his blood, by being electrized, as has been aſſerted.

I had almoſt forgot to mention, that the ſtrokes I received from the electrified garden-pots were more violent and painful to my fingers than from any other body I ever experienced.

Mr

Mr *Baker*, ſince his receiving the above account, has had an opportunity of electrifying a myrtle-tree, of between 2 and 3 feet in height, growing in a pot at the ſeat of the Duke of *Montague* at *Ditton*; in preſence of his Grace, of the *Preſident* of the *Royal Society*, and ſeveral other curious gentlemen; who found, that whenever the hand, or other non-electric body, was brought near the leaves, ſtreams of fine purple fire iſſued therefrom, together with a conſiderably cold air; and that the leaves would be attracted at ſome diſtance, and move vigorouſly towards a non-elctric body.

24. Since I have read the *Tranſaction* * with reſpect to the ſparkling lady, who could communicate a kind of electrical fire to her garments, I can give you an inſtance nearly like it, of a lady who was ſurprized at ſuch an appearance from a flannel petticoat, which ſhe happened to ſhake in the dark. But at laſt, we found that new flannel, after ſome time wearing, would acquire this property; but that it loſt it by being *waſhed*.

Extract of a letter from Mr Benj. Coke, *F.R.S. to Mr* Peter Collinſon, *F. R. S. concerning the property of* Iſle of Wight.

new Flannel *ſparkling in the dark*. N°. 433. p. 457. Mar. &c. 1747. *Dated* Newport, Jan. 13. 1746-7. *Read* March 19. 1746-7.

25. I fancy at laſt this ſparkling of the flannel, and ſuch-like bodies, will be found to be quite electrical: and it is poſſible, I conceive, that the acid ſteams of the ſulphur, burnt under the extended flannel in the time of bleaching, may unite themſelves with the oil (with which hair, as well as horns, are found by analyſis to be replete), and form an animal ſulphur, which, upon friction, vibration, or any nimble agitation of theſe hairs, may become luminous.

Part of two letters from the ſame, concerning the ſparkling of Flannel, *and the* Hair *of* Animals *in the dark.* N°. 488. p. 394. June 1748. *Dated* May 19. 1748. *Read* June 23. 1748.

And that ſomething like this may be in the caſe, ſeems not improbable; ſince it hath been obſerved, that this appearance hath happened moſt conſpicuous in froſty weather; in which ſeaſon there is generally not only a greater purity of the air, and abſence of moiſture, but all hairy and horny ſubſtances (and hairs, you know, are but ſmall horns) are more elaſtic, and conſequently ſuſceptible of, and capable of exciting, the ſtrongeſt vibrations. And, on the contrary, the lixivial ſalts uſed in waſhing may deſtroy the ſulphureous acid, and diſcharge the oil; whence the hairs will become more flexible and limber, and be rendered leſs fit for exciting the electrical fire. And the ſame may happen when flannel is much worn, and by that means filled with the alcaline *effluvia*'s, which go off from moſt (of the higher order of) animals by tranſpiration; which may diſſolve the animal ſulphur, weaken the ſpring of the hairs, and ſo render the phænomenon more difficult.

It ſhould have been mentioned, that the flannel had been worn but few days; and that it was immediately upon ſhaking the under-coat from that

The ſecond letter, dated Newport, June 1. 1748.

* Art. 6.

which

which was worn above it, that the ſparks were emitted; and that their appearance was in a broad ſtreak almoſt contiguous, attended with a crackling or ſnapping, like what may be obſerved on moving the finger nimbly along over the prime conductor, when excited in the electrifying machine; of which the lady was able to form a compariſon, having afterwards ſeen ſome experiments of that ſort.

This appearance returned at the ſame time, and on the ſame occaſion, 2 or 3 nights after, but more languid, till it was quite loſt.

A lady, who was informed of this, leſſened the ſurprize (which had been thought almoſt ominous) by aſſuring, that ſhe had ſeen the ſame phænomenon often in new flannel, but never in any that had been long worn or waſhed: and that the flannel being rendered damp with ſea-water, and afterwards dried, would heighten the flaſhing, which ſhe imputed to the ſulphur uſed in bleaching. However that be, I ſhall only obſerve, that theſe ſparklings had the crackling criterion of electrical fire; and that hair and wool, as well as ſilk, are electrics *per ſe*, and unctuous and ſulphureous bodies more electric than others of the ſame denſity.

Dr *Wall* hath obliged the public with a curious diſſertation on a ſimilar ſubject, which I gueſs would be particularly entertaining while you are on this ſpeculation.

Bartholin ſuppoſes unctuous *effluvia* to have a great ſhare in theſe appearances: his words are theſe, which I chuſe to quote; the book * *De Luce Animalium* being not very common: "Imo quod admirationem "excedit, collectæ oleaginoſi effluvii reliquiæ, longo interjecto tem-"pore, in ſcintillas reſolvuntur: ſi enim faſcias vel tænias ſerico textas, "ſed uſu detritas, leviter excutiamus, igniculi ſuſcitantur ſcintillæ;" ——and quotes a paſſage out of *Geſner De Herbis Lucentibus*, to confirm his opinion.

The ſame writer tells us, that *Theodore Beza* was to be ſeen in the dark, "ob fulgorem externum circa oculorum orbes;"—— but whether this light proceeded from the ball of the eyes, or hairs of the brows or lids, he does not mention.——Nor does that learned author ſo exact in ſome other circumſtances, in other examples of this ſort, as could be wiſhed. However, I think what he ſays of the Duke of *Mantua* deſerves a remark.——"Quicquid ſit, pro vero habendum eſt quod de *Carolo* "*Gonzaga Mantuæ* duce conſtans fama tulit, levi per totam cutem facta "*frictione* flagrantes ſpecies exire ſolitas."——But here alſo it were to be wiſhed he had let us know whether this great man, of a moſt illuſtrious family, had not ſome particular hairy or ſcaly texture or covering to his ſkin.

By this, I gueſs, you are excited to know how this author, who lived about 100 years paſt, ſolves theſe appearances, of which he had profeſſedly written. Take it in his own words.——

* *Tho. Bartholinus* De Luce Hominum & Brutorum, *lib.* iii. *Hafniæ* 1669, 8°.

"*Ariſtoteles*

" *Ariſtoteles* (l i. m. cx.) docebat——quod omnis natura ejus ſit eſ-
" ſentiæ procreatrix, qualis ipſa eſt—enimvero ſunt ad conſervationem
" ſpeciei omnis, ejuſdem ſingulæ particulæ, vim ſe diffundendi obtinue-
" runt, & ſpargendi, per individua multiplicata, ita ne lux primæva &
" naturalis, ſingulari numinis conſilio, elementorum mixtioni addita,
" mole minor intercidat, & extinguatur cum ſpeciei non revocando caſu,
" eo modo conſervari debuit, quo ſerventur omnia, per inſitam naturæ
" potentiam ſui generativam, *&c.*"

26. In a book which I publiſhed laſt year in the *German* tongue, when I was ſpeaking of *Muſſchenbroeck*'s experiment, and deſcribing the increaſe of it in glaſs veſſels, I made mention of a machine which made ſeveral ſparks to appear and crackle, but did not at that time give any figure of it. This *electrical pyrorganon*, as I call it, is repreſented in *fig.* 14. Through the middle of a metalline ring *a b*, filled with pitch, is fixed the little metalline cylinder *c d*. The diameter of the ring is a *Paris* inch and 4 lines. The cylinder appears on both ſides at the diſtance of an inch. The diameter of the cylinder muſt not be leſs, for fear the Electricity communicated to the cylinder ſhould be diminiſhed by touching the metalline ring. To this ring is ſoldered a metalline fork, at which the cochleated ſtyle is let into a wooden cylinder *e f*, the lower extremity of which *f*, is ſo formed, that paſſing through a fiſſure of any plank, it may be faſtened by a ſcrew. Such a metalline cylinder, with a cochleated ſtyle fixed into the pitch with which the ring is filled, I call, for brevity, an *electrical cylinder*. The *electrical pyrorganon* is compoſed of 4 ſuch cylinders. The cylinders are ſo placed, as to have a ſufficient ſpace between them to collect the electrical ſparks. When I would excite them, I place the *pyrorganon* near ſome piece of metal *a b*, ſuſpended on ſilken threads and faſtened, leaving the neceſſary ſpace between the metal and the cylinder *c*. To the laſt cylinder *f g*, I faſten at *g* a wire *h*, reaching to a metalline veſſel *i*, full of water. When theſe things are ſo diſpoſed, as ſoon as the oblong metal *a b* is electrified, the electrical ſparks flaſh out in the 4 ſpaces, and ſhine in proportion, as the glaſs balls communicate more or leſs Electricity, by *rotation* or *friction*

Deſcription and figures of an electrical machine, by Jo. Hen. Winkler, *Prof* Leipſ. *and F. R. S.* N°. 483. p. 497. Mar. &c 1747. *Dated* Mar. 31. *Read* May 7 1747. Fig. 13.

Fig 14.

Since the publication of the book above-mentioned, I have conſtructed 2 *electrical pyrorgana*, one of which has the form of a winged wheel, and the other with it's ſparks gives the figure of *Charles's* Wain.

The conſtruction of the winged wheel is as follows: into a hollow orb or wheel of wood *d d d d*, are fixed 6 wooden wings *c d*. The diameter of the whole wheel with it's nave, is 13 inches, and that of the nave 6 inches. The wings *c d*, which are 10 inches long, have fiſſures, in which 3 electrical cylinders may be moved to and fro', and faſtnened. Near the faſtening, at the wings in the orb *d d d d*, are made angular holes, in each of which another electrical cylinder is placed. Thus in the winged wheel appear 6 rows, each conſiſting of 4 electrical cylinders, keeping the diſtances between them which are moſt convenient for exciting the electrical ſparks. In the extremity *c* of each wheel is faſtened a metalline

Fig 16.

inſtrument *g f*, conſiſting of 3 parts, the extremes of which are joined to the middle *g* in a right angle.

Now that theſe metalline inſtruments may be rightly applied, in the poſterior ſide of the wing at the end *c*, for I call that the anterior, in which the electrical cylinders ſparkle, a metalline button being faſtened, leaves ſome ſpace between itſelf and the poſterior ſide of the wing. The middle of the button is furniſhed with a ſcrew *g*. In the above-mentioned ſpace is inſerted the ſhorter part of the metalline inſtrument. The other extremity *h*, is near the fourth cylinder; but is ſo far diſtant from it, as is neceſſary for the electrical flaſh to be excited between that part of the inſtrument and the cylinder. Hence the other ſhorter part may be moved at will, and faſtened under the button. To the buttons is applied, and wound about the poſterior parts of the wings, a wire *i k*, to which, when the electrical ſparks are to be excited, in the place *x*, another metal *y* is added, which reaches to a metalline veſſel *s*, filled with water. The Electricity, as ſoon as it is communicated to the firſt cylinder, paſſes to all the reſt.

Hence it is neceſſary, in order to excite the ſparks, that the metalline inſtruments *g f* ſhould always remain free from Electricity, which is done by the metal *y*, joined to the wire *i k*, that conducts the Electricity to the water. For between 2 bodies endued equally with Electricity, no ſparks appear. But the Electricity is communicated to the firſt cylinder by the metalline hammer *a*, faſtened to the metalline axis *b c*, which may be turned in the round holes of 2 wooden columns *d e*, by the handle *f*. The apparatus of this hammer is ſhewn in *fig.* 17.

Fig. 17. The columns *d e* reſt upon the piece of wood *m n*, which has a ſtyle fixed into the lid *g*, which covers the glaſs veſſel *h*, faſtened to it with pitch. This glaſs veſſel, therefore, is neceſſary, that the Electricity given to the metalline axis *b c*, may be preſerved by means of ſome wire hung to it.

The bottom of the glaſs veſſel *h* is joined with pitch to the wood, to which a ſtyle is added, which may be inſerted into a longer hole of the column *i k*, ſo as to be faſtened by a ſcrew *l*, after the glaſs veſſel has acquired it's due height, which is when the axis *b c* is in the middle of the nave of the wheel *d d d d*. The electrical cylinders are to be placed in the orb and rings *c d*, in ſuch a manner, that the hammer may have a ſufficient diſtance from the firſt electric cylinder to which it approaches, and the cylinders from each other, to excite the cylindrical ſparks.

Fig. 16. In the ſquare baſe *m n o p*, *fig.* 16. on which ſtands the column *i k*, is a fiſſure, in which, when the hammer *a* appears ſufficiently through the nave of the wheel *d d d d*, the column *i k* is faſtened by a ſcrew. The metalline fork is applied to the wheel *d d d d*, and fixed into the wooden column *q r*, which is faſtened in like manner in the fiſſure of the ſquare baſe *m n o p*. The metalline axis *b c*, protended through the nave of the wheel, is 21 inches high above the ſquare baſe: when therefore, on the axis *b c*, having acquired the Electricity, the hammer approaches to any

firſt

firſt electrical cylinder, 5 ſparks appear in a ſtrait row almoſt at the ſame time.

On turning the axis *b c*, thoſe rows of ſparks appear in a circle. The ſparks are ſo bright as to be ſeen by day-light at the diſtance of 100 feet. The man who turns the axis by it's handle, ought to ſtand upon a ſubſtance that does not propagate Electricity, for fear of diſſipating and loſing the Electricity.

Fig. 15. repreſents the electrical 7 ſtars, or *Charles*'s Wain: in the table *a b c d*, which may be elevated or depreſſed in the fiſſure of the column *e f*, 9 electrical cylinders may be ſo placed, that 7 ſparks may appear in the ſame order in which thoſe 7 ſtars appear in a clear night. A wire *h*, which receives the Electricity, is added to the metalline cylinder when theſe ſparks are to be ſhewn. In the extremities of the third and fourth, the bent wire *i* is faſtened, that the Electricity may reach to the fifth and the reſt of the cylinders. To the ninth cylinder is applied a wire *k*, reaching to the water in the metalline veſſel *l*, that the Electricity, being diſtributed through all the cylinders, may be propagated as far as is neceſſary into a ſubſtance which does not preſerve it. Fig. 15.

Theſe obſervations, though extending no farther than to delight the eye, I have ventured to offer to your illuſtrious Society, who have diſcovered a wonderful power of nature to be concealed in ſuch entertainments.

27. In the paper I did myſelf the honour ſome time ſince to communicate to the *Royal Society*, I took notice, that, among the many other ſurpriſing properties of Electricity, none was more remarkable, than that the electrical power, accumulated in any non-electric matter contained in a glaſs phial, deſcribed upon it's exploſion a circuit through any line of ſubſtances non-electrical in a conſiderable degree; if one end thereof was in contact with the external ſurface of this phial, and the other end upon the exploſion touched either the electrified gun-barrel, to which the phial in charging was uſually connected, or the iron hook always fitted therein. This circuit, where the non electric ſubſtances, which happen to be between the outſide of the phial and it's hook, conduct Electricity equally well, is always deſcribed in the ſhorteſt manner poſſible; but if they conduct differently, this circuit is always formed through the beſt conductor, how great ſoever it's length is, rather than through one which conducts not ſo well, though of much leſs extent. *A collection of the electrical experiments communicated to the* R. Society *by* Wm Watſon, *F. R. S. read at ſeveral meetings between* Oct. 29. 1747. *and* Jan. 21. *following*. N°. 485. p. 49. Jan. 1747-8.

It has been found, that in proportion as bodies are ſuſceptible of having Electricity excited in them by friction, in that proportion they are leſs fit to conduct it to other bodies; in conſequence whereof, of all the ſubſtances we are acquainted with, metals conduct beſt the electrical powers; for which reaſon the circuit before ſpoken of is formed through them the moſt readily. Water likewiſe is an admirable conductor; for the electrical power makes no difference between ſolids and fluids as ſuch, but only as they are non-electric matter.

In order to give an idea of what is underſtood by this circuit, we will mention an example or two, from which all the other may naturally be

deduced. If a perſon ſtands upon a dry wooden floor with a coated phial ever ſo highly charged in one of his hands, and if another perſon, without touching the firſt, ſtands but ſix inches from him, and touches the iron hook of the phial, neither of them are ſhocked; becauſe the floor between them, tho' the diſtance is ſo ſhort, will not conduct the Electricity ſufficiently quick. But if theſe two perſons tread upon a piece of wire laid between them, they each of them feel the electrical commotion in that arm, which touches the phial and hook, and in that foot which treads upon the wire; the wire here conducting the Electricity quick enough, which the dry floor would not. The circuit is here formed by the coated phial, it's hook, ſo much of the bodies of theſe two perſons as formed a curve line between the wire, the phial, and hook, and the wire between theſe perſons. If theſe perſons ſtand upon, or touch with any part of their bodies any non-electrics, which readily conduct Electricity, the circuit is completed, and the effect is the ſame: and this is occaſioned by the ſhort ſpace of time, in which the loaded phial is diſcharged, when any matter of what kind ſoever readily conducting Electricity happens to be between the coated phial and it's hook, and is ſo connected as to communicate with both upon the diſcharge of the phial.

M. *le Monnier* the younger at *Paris*, in an account tranſmitted to the *Royal Society*, takes notice of his feeling the ſtroke of the electrified phial along the water of two of the baſons of the *Thuilleries* (the ſurface of one of which is about an acre) by means of an iron chain which lay upon the ground, and was ſtretched round half their circumference.

Upon theſe conſiderations it was conjectured, as no circuit had as yet been found large enough ſo to diſſipate the electrical power as not to make it perceptible, that if the non-electrical conductors were properly diſpoſed, an obſerver might be made ſenſible of the electrical commotion quite acroſs the river *Thames*, by the communication of no other medium than the water of that river. But as perhaps, in what relates to Electricity leſs than in any other part of natural Philoſophy, we ſhould draw concluſions but from the facts themſelves, it was determined to make the experiment.

The making this experiment drew on many others, and as the gentlemen concerned flatter themſelves that they were made with ſome degree of attention and accuracy, they thought it not improper to lay a detail of all the operations relating thereto, before the *Royal Society*.

In order to try whether or no the electrical commotion would be perceptible acroſs the *Thames*, it was abſolutely neceſſary that a line of non-electric matter, equal in length to the breadth of the river ſhould be laid over it ſo as to touch the water thereof in no part of it's length; and the bridge at *Weſtminſter* was thought the moſt proper for that purpoſe, where the water from ſhore to ſhore was ſomewhat more than 400 yards.

Accordingly on *Tueſday July* 14, 1747. to ſee the ſucceſs and aſſiſt in making the experiment, there met *M. Folkes*, Eſq; *Pr. R. S.* the R. Hon. the E. *Stanhope*, *Rich. Graham*, Eſq; *Nich. Mann*, Eſq; and myſelf,

myſelf, with proper perſons to execute what was required of them in the various parts of theſe experiments.

A line of wire was laid along the bridge, not only through it's whole length, but likewiſe turning at the abutments, reached down the ſtone ſteps on each ſide of the river low enough for an obſerver to dip into the water an iron rod held in his hand. One of the company then ſtood upon the ſteps of the *Weſtminſter* ſhore holding this wire in his left hand, and an iron rod touching the water in his right: on the ſteps facing the former upon the *Surry* ſhore, another of the company took hold of the wire with his right hand, and graſped with his left a large phial almoſt filled with filings of iron coated with ſheet-lead, and highly electrified by a glaſs globe properly diſpoſed in a neighbouring houſe. A third obſerver ſtanding near the ſecond dipped an iron rod held in his left hand into the water, and touching the iron hook of the charged phial with a finger of his right hand, the Electricity ſnapped, and it's commotion was felt by all the three obſervers, but much more by thoſe upon the *Surry* ſhore. The third obſerver here was no otherwiſe neceſſary, than that the river being full, the iron was not long enough to be fixed in the mud upon the ſhore, and therefore was in want of ſome ſupport. The experiment was repeated ſeveral times, and the electrical commotion felt acroſs the river; but the gentlemen preſent being much moleſted in their operations by a great concourſe of people, who many times broke the conducting wire, and otherwiſe greatly incommoded them, and the evening growing too dark for the obſervers on different ſides of the water to ſee each other, they were prevented from diverſifying the experiments, as was intended, and only conſidered theſe trials as a ſtill further encouragement for them to proſecute the inquiry at a more favourable opportunity.

Early therefore on *Saturday* morning *July* 18, there met upon *Weſtminſter-Bridge* the *Preſ.* the R. Hon. the Lord *Charles Cavendiſh*, *Rich. Graham*, Eſq; Dr *Bevis*, and myſelf, with proper aſſiſtants, At the preceding meeting, the electrical machine's being placed at ſome diſtance from the water being found inconvenient, the following alteration was made in the diſpoſition of the *apparatus*.

A room up two pair of ſtairs in a commodious houſe neareſt the bridge on the *Surry* ſhore was provided, in which was placed the electrical machine with the gun-barrel ſuſpended in ſilk lines. From this room on account of it's height, the ſignals on both ſides of the river were eaſily obſervable. The coated phial before-mentioned with it's iron hook was placed upon the ſeat of the window of this room, and communicated with the gun-barrel by the means of a piece of iron wire. One extremity of another wire was likewiſe fixed into the bottom of the leaden coating of the phial, whoſe other extremity reached therefrom over the bridge to the ſteps upon the *Weſtminſter* ſhore, the body of the wire being placed as much as poſſible upon the parapet of the bridge. One or more obſervers took each other by the hand, the firſt of which muſt neceſſarily

ceſſarily take the wire in his left hand, and the laſt, upon the proper ſignal given, either dip his right hand into the water, or (which makes the poſture more agreeable) a rod of metal held therein. Another wire having no communication with any of the former, was let down from the before-mentioned room, and down the ſteps upon the *Surry* ſhore: one extremity of this wire was held in the hand of an obſerver ſtanding upon theſe ſteps, who dipped an iron rod held in his other hand into the water: to the other extremity of this wire was faſtened a ſhort iron rod, with which, when the electrified phial was ſufficiently charged, and the ſignal given, the gun-barrel was to be touched.

The gentlemen, by this diſpoſition of the *apparatus*, propoſed to examine principally theſe 3 queſtions: firſt, whether or no the obſervers ſtanding on each ſide of the river would perceive the electrical commotion, each putting an iron rod into the water? Secondly, whether or no the obſervers on both ſides of the river would feel the electrical commotion, when the obſerver ſtanding upon the *Weſtminſter* ſhore removed the iron rod held in his hand out of the water? Thirdly, whether or no the electrical power was perceptible to the obſervers on both ſides of the river, if the obſerver upon the *Weſtminſter* ſhore dipped his hand into a pail of water, which had no communication with the water of the *Thames*.

It was determined firſt, upon proper ſignals, to diſcharge the electrified phial in the manner before-mentioned, the obſervers on each ſide of the river holding the iron rods in the water, and this experiment was to be repeated 3 times. This was attempted accordingly; and although the obſerver on the *Surry* ſhore was each time ſmartly ſtruck, the *Preſident*, who obſerved with the utmoſt attention upon the *Weſtminſter* ſhore, gave the ſignal that he felt nothing. The company was ſurpriſed at this want of ſucceſs in the experiment; but, upon examining the wire, which was laid over the bridge, it was found to have been broken by ſome accident, after it had paſſed over about $\frac{1}{4}$ part of the bridge. The wire being refitted, it was agreed to make the ſame experiment ſix times more: this was done accordingly, and the electrical commotion was felt each time by the obſervers on both ſides of the water, but much ſmarter by thoſe on the *Surry* ſide. It was then thought proper to repeat this experiment 3 times more upon the ſignal's being given: but, in making the firſt of theſe, the obſerver in the room with the machine, diſcharged the electrified phial, before the obſerver upon the *Surry* ſhore had dipped his iron rod into the water, and therefore no effect was perceived by the obſerver on the oppoſite ſhore. The electrified phial therefore was again diſcharged 3 other times, and the commotion felt by the obſervers on both ſides of the river.

To examine the ſecond queſtion, no other alteration was neceſſary in the whole *apparatus*, than that the obſerver upon the *Weſtminſter* ſhore ſhould not dip either his hand, or the iron rod held therein in the laſt experiments, into the water of the river. The electrified phial then was

diſcharged

diſcharged 3 times without it's effects being in the leaſt perceived by the obſervers upon the *Weſtminſter* ſhore; thoſe indeed on that of *Surry* felt the ſhock as before.

In examining the third queſtion, the *apparatus* was in all other reſpects the ſame as in the laſt; except that the obſerver upon the *Weſtminſter* ſhore had a pail of water placed upon a wooden table, which ſtood upon the ſtone ſteps, and into which he was to put his right hand upon the ſignal's being given. This was accordingly done, and the electrified phial being diſcharged 3 times, the electrical commotion was felt as before by the obſerver upon the *Surry* ſhore; but not in the leaſt by him on the *Weſtminſter* ſide, who held his hand in the pail of water.

In all theſe experiments, except in one before-mentioned, where the iron rod was not in the water, it was found, that whether the obſervers on the *Weſtminſter* ſhore, upon the diſcharge of the electrified phial, did or did not feel it's effects, they were always perceived not only in the arms of thoſe upon the *Surry* ſhore, who formed a line between the extremity of the wire there, and the water of the river; but by any other perſon, who ſtanding upon the ſtone ſteps, even where they were not wet, touched the wire with his hand. They were likewiſe felt by a perſon upon the *Weſtminſter* ſhore, ſtanding upon the wet ſtone ſteps, who did not form part of the line between the extremity of the conducting wire and the water, otherwiſe than by touching the wire with his fingers.

As was before-mentioned, the obſervers upon the *Weſtminſter* ſhore did not feel the effects of the diſcharged phial near ſo ſtrong as thoſe on that of *Surry* in the firſt ſett of theſe experiments. When a line was there formed by the joining hands of two or more perſons, the firſt of which, on account of the ſituation, held the conducting wire in his left hand, and the laſt touched the water with an iron rod held in his right, the effects were moſt ſenſible in the left arm of him who held the wire: they were indeed manifeſtly felt by them all; but this feeling was not great enough to be called a ſhock, but, as was very properly expreſſed by one of the company, it reſembled the pulſation of a large artery.

From the examination of the firſt and ſecond queſtions it appeared, that the obſervers upon the *Weſtminſter* ſhore were not ſenſible of the effects of the Electricity, unleſs their bodies deſcribed part of the circuit before ſpoken of; and this circuit here conſiſted of part of the gun-barrel of the electrifying machine, the wire going from this gun-barrel to the iron hook, the phial itſelf, the tail wire of this coated phial which reached therefrom acroſs the bridge and down the ſteps on the *Weſtminſter* ſhore, the line of obſervers between this wire and the iron rod which dipped in the water there, this iron rod, a ſuppoſed line of water drawn quite acroſs the *Thames*, the obſervers with their iron rod on the *Surry* ſhore, the iron wire going from the right hand of the laſt of theſe up into the room where the electrifying machine was placed, and the ſhort iron rod to which one extremity of this wire was joined, and with which,

in making the exploſion, the gun-barrel was touched. The length of this circuit, through which the Electricity was propagated was at leaſt 800 yards, more than 400 yards of which was formed by the ſtream of the river.

From the examinntion of the third queſtion it appeared, that the electrical commotion would not be felt from the obſerver dipping his hand in water only, unleſs that water was ſo diſpoſed as to become part of the circuit; and this experiment was made, leſt the contrary might be ſurmiſed.

The obſervers upon the *Weſtminſter* ſhore not feeling the electrical commotion equally ſtrong with thoſe of *Surry*, was judged to proceed from other cauſes beſides that of diſtance. For it muſt be conſidered, that the conducting wire was almoſt thoughout it's whole length laid upon *Portland* ſtone ſtanding in water. This ſtone, being in a great degree non-electric, is of itſelf a conductor of Electricity: and this ſtone ſtanding in water, no more of the Electricity was tranſmitted to the obſervers on the *Weſtminſter* ſhore than that proportion, wherein iron is more non-electric, and, conſequently, a better conductor of Electricity than ſtone. This was made more manifeſt, from obſerving, that whether the conducting wire upon the bridge was broke or no, and, conſequently, whether the obſervers upon the *Weſtminſter* ſhore felt the electrical commotion or no, not only the obſervers upon the *Surry* ſhore, who with their wire formed part of the line, felt the ſhock in their arms; but thoſe perſons who only ſtood upon the ſtone ſteps there, and touched the wire with their fingers, felt the electrical commotion in the arm of that hand which touched the wire, and down their legs. From whence, and from the perſon before ſpoken of feeling the electrical commotion ſtanding upon the wet ſtone ſteps of the *Weſtminſter* ſhore, though not forming part of the line, but only touching the wire with his fingers, it was concluded, that, beſides the large circuit before ſpoken of, there were formed ſeveral other ſubordinate circuits between the ſame ſteps of the *Surry* ſhore, and the bridge by means of the water; whereby that part of the electrical power, felt by the obſervers upon the *Surry* ſide of the river, and not by thoſe on the *Weſtminſter* ſide, was diſcharged.

Dr *Bevis* having obſerved, and which was likewiſe tried here, that however well an electrified phial was charged, it's iron hook would not fire the vapours of warm ſpirit of wine held in a ſpoon and applied thereto, if the perſon who held the phial, and he who held the ſpoon did not take each other by the hand, or have ſome other non-electrical communication between them, it was therefore thought proper to try the effects of Electricity upon ſome warm ſpirit of wine through the large circuit beforementioned. Accordingly the obſervers being placed as before both upon the *Weſtminſter* and *Surry* ſhores, no other alteration was made in the beforementioned *apparatus*, than that the wire which connected the gun-barrel with the iron hook of the coated phial being laid aſide, the coated phial itſelf was charged at the gun-barrel, and then brought in the hands of

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an obſerver near the warm ſpirits in the ſpoon, which was placed upon the ſhort iron rod before-mentioned, which was connected with the wire which went to the obſervers upon the *Surry* ſhore. Upon preſenting properly the iron hook of the charged phial to the warm ſpirit, it was inſtantly fired, and the electrical commotion felt by the obſervers on both ſides of the river.

It was then thought proper to try the effects of the charged phial upon the warm ſpirit, when the wire was divided which was laid over the bridge: upon preſenting the iron hook to the ſpirit, a ſufficient ſnap was given to the ſpoon to fire the ſpirit, but nothing ſo ſmart as in the former experiment, where the large circuit was completed.

It was then tried, what the effect would be upon the ſpirit, if the charged phial was diveſted of it's long wire which lay over the bridge, and was only held in the hand of an obſerver; whilſt the ſpoon with warm ſpirit was placed in contact of the iron rod before-mentioned, to which the wire was connected, which went to the obſervers upon the *Surry* ſhore; and the ſpirit was fired with much the ſame degree of ſmartneſs as in the laſt experiment.

In theſe and all the ſubſequent operations, wires were made uſe of to conduct the Electricity preferable to chains, as it before by great numbers of experiments had been fully proved, that whatever difference there was in the bulk of the conductor, that is to ſay, whether it were a ſmall wire, or a thick iron bar, the electrical ſtrokes communicated thereby were equally ſtrong: and it had been further obſerved, beſides the difficulty of procuring chains of a requiſite length for the preſent purpoſes, that the ſtroke at the gun-barrel, when the Electricity was conducted by a chain, was *cæteris paribus*, not ſo ſtrong, as when that power was conducted by a wire. This was occaſioned by the junctures of the links of the chain not being ſufficiently cloſe, which cauſed the Electricity in it's paſſage to ſnap and flaſh at the junctures, where there was the leaſt ſeparation; and theſe leſſer ſnappings in the whole length of the chain leſſened the great one of the gun-barrel.

Encouraged by the ſucceſs of theſe trials, the gentlemen were deſirous of continuing their enquiries, and of knowing whether or no the electrical commotions were perceptible at a ſtill greater diſtance. The *New River* near *Stoke-Newington* was thought moſt convenient for that purpoſe; as at the bottom of that town, the twinings of the river are ſo circumſtanced, that from a place which we will call *A* to another *B*, the diſtance by land is about 800 feet, but the courſe of the river is near 2000. From *A* to another place, which we will call *C*, in a right line is 2800 feet, but the courſe of the water is near 8000 feet.

Accordingly, on *Friday July* 24, 1747, there met at *Stoke-Newington* the *Preſ.* of the *R. Soc.* the R. Hon. the Lord *Ch. Cavendiſh*, the Rev. Mr *Birch*, *James Burrow*, Eſq; *Peter Daval*, Eſq; Mr. *George Graham*, *Wm Jones*, Eſq; *James Lever*, Eſq; Mr *Newcombe*, *Charles Stanhope*, Eſq; Mr *Trembley*, and myſelf, who were of the *Royal Society*, and Dr

Bevis. To this gentleman the company were much obliged, not only for his great readineſs in aſſiſting in all the operations, but likewiſe for the uſe of his electrifying machine, which from it's ſize was conveniently portable. This machine was now placed in a room up one pair of ſtairs in a houſe near *A*, and the ſignals from thence might eaſily be perceived by the obſervers both at *B* and *C*.

It was propoſed, firſt to try the electrical commotion by the ſame obſervers as at *Weſtminſter-bridge*, from *A* to *B*, the diſtance as before-mentioned being about 800 feet by land, and 2000 by water, in order, if poſſible, to determine the difference of the ſtrength of the Electricity felt there, and at the ſtone bridge at *Weſtminſter*; the difference of the length of the 2 circuits being about 400 feet in favour of that of the *New River*.

To make the experiment, an iron wire was faſtened to the coating of the glaſs phial before-mentioned, and conducted from one of the windows of the room over the *New River* without touching the water; and from thence to *B*, laying in it's whole length upon the graſs in the meadows, except where it paſſed over a hedge. At *B*, when the exploſion was to be made, one or more obſervers were to take the extremity of this wire in one hand, and touch the water of the river as before with an iron rod held in the other. Another wire was let down from the other window of the room; one extremity of which was joined to the ſhort iron rod mentioned in the former experiments, the other was held in the hand of an obſerver at *A*, whoſe other hand held an iron rod dipped into the river.

It was abſolutely neceſſary that theſe wires ſhould touch each other in no part of their length, otherwiſe the before-mentioned circuit would upon the exploſion be completed from their firſt contact.

When every thing was thus diſpoſed, and the ſignals given, the charged phial was exploded 8 times, and the electrical commotion every time ſmartly felt by the obſervers both at *A* and *B*. Whether the line of obſervers at *B* conſiſted of one or more, they were always ſtruck, and that more ſharply than at *Weſtminſter-bridge*, under the ſame circumſtances. One of the obſervers, taking the wire in his hand, without having any communication either with any of the other gentlemen or the water of the river, felt the ſhock in his feet.

It was then thought proper to make right exploſions without any other alteration in the *apparatus* than that the obſervers at *B*, ſhould ſtand in the meadow at ſome diſtance from the water, without having any communication therewith other than that furniſhed by the ground. This was accordingly done, and the ſtroke felt little if at all leſs than thoſe laſt mentioned. But the electrical ſtrokes being felt ſmartly at the diſtance of at leaſt 20 feet from the water, occaſioned a very perplexing difficulty, as it was impoſſible by this experiment to determine with any certainty, whether or no the electrical circuit was formed throughout the windings of the river, or much ſhorter by the ground of the meadows. The experiment plainly ſhewed, that the meadow-ground with the graſs thereon

conducted

conducted the Electricity better than ſtone; as it muſt be remembered, that the obſervers upon the ſtone ſteps upon the *Weſtminſter* ſhore felt not in the leaſt degree the electrical commotion, when their iron rod was not in the water, and themſelves ſtood upon the dry ſtone ſteps. But this effect was ſuppoſed to be owing to the meadow-ground here being encompaſſed on two ſides by the *New River*, and on the other by a wet ditch, by both which it was generally well moiſtened. To ſolve therefore this difficulty, a ſeries of experiments were executed, of which hereafter.

The gentlemen then determined to examine whether the electrical commotions were perceptible from *A* to *C*; a diſtance not leſs than 2800 feet by land, and near 8000 by water.

To execute this, to the former wire, which was already conducted to *B*, another was added, which there croſſed the river without touching the water; and reached almoſt to *C*, where the firſt of a line of gentlemen held, as before, the wire in one hand, and the laſt dipped the iron into the water. The wire from the machine to *A* was as before. Upon the ſignal's being given, the charged phial was exploded 10 times, and it's effects plainly though but faintly perceived each time by ſome or other of the obſervers, but never by them all. The electrical commotion was always felt by that obſerver who held the extremity of the wire, but never by him who held the iron rod in the water. It was in one experiment felt by the obſerver who held the wire, not felt by the next, who held the hand of the former, and yet plainly perceived by the third, who joined the ſecond. Thoſe who did not themſelves feel the electrical commotion here, did as at *B* ſee the involuntry motions of thoſe who did. The obſervers at *A* felt the ſhocks in the ſame degree, whether the other obſervers were ſtationed at *B* or *C*.

This experiment further demonſtrates the diſtance to which the electrical power may be conveyed: but the ſame difficulty occurs here as in the laſt; to wit, whether the circuit was completed by the ground, or by the water of the river?

Theſe ſame operations, which ſhewed at how great a diſtance the electrical commotion was perceptible, ſolved likewiſe 3 queſtions of a ſubordinate nature.

Firſt, Whether or no, *cæteris paribus*, any difference occurred in the ſucceſs of the experiment, if the long wire, inſtead of being joined to the coating of the phial, was faſtened to the ſhort iron rod, which, upon touching the gun-barrel, occaſioned the exploſion; and if the ſhort wire, which only went to the obſerver at *A*, a diſtance from the machine not more than 30 feet, was joined to the coating of the phial? Upon trial no difference * was found.

* No difference is obſerved when the electrical circuit is propagated through ſubſtances which readily conduct Electricity; if they conduct it in a leſs degree, the electrical commotion is moſt perceptible to the obſerver, who holds the wire, which comes from the charged phial.

Secondly, Whether or no, *cæteris paribus*, any difference in the electrical commotion would be perceived, when that power paſſes through the arms of two obſervers, whoſe bodies made part of the circuit, ſtanding in the room near the electrifying machine; one of which takes the extremity of the wire that goes to the obſerver at *A* in one hand, and touches the gun-barrel with the ſhort iron rod held in his other hand? The other obſerver takes the extremity of the wire which goes to *B* or *C* in one hand, and touches the coating of the charged phial with his other. In ſeveral trials, where each of theſe obſervers frequently changed ſtations, no difference in point of ſtrength was obſerved in the electrical commotion.

Thirdly, Whether or no theſe two obſervers laſt-mentioned received the ſhock at the ſame time? They were ſeen to be both convulſed in the ſame inſtant.

July 28. 1747, there met again at the ſame place, to proceed further in theſe enquiries, the *Preſident*, the R. Hon. the Lord *Ch. Cavendiſh*, the Rev. Mr *Birch*, Sir *Francis Daſhwood*, Baronet, *Peter Daval*, Eſq; Mr *Ellicott*, Mr *George Graham*, *Richard Graham*, Eſq; Mr *Robins*, Mr *Short*, Dr *Wilbraham*, and myſelf, who were of the *Royal Society*, and Dr *Bevis*.

The electrical commotion was firſt tried from *A* to *B* before-mentioned, the iron wire in it's whole length being ſupported, without any where touching the ground, by dry ſticks placed at proper intervals of about 3 feet in height. The obſervers both at *A* and *B* ſtood upon originally-electrics, and, upon the ſignal, dipped their iron rods into the water. Upon diſcharging the phial, which was ſeveral times done, they were both very much ſhocked, much more ſo than when the conducting wires lay upon the ground, and the obſervers ſtood thereon, as in the former experiments. The ſame experiment was tried with the obſerver at *A*, inſtead of the iron rod, dipping a narrow ſlab of *Portland* ſtone into the water about 3 $\frac{1}{2}$ feet in length; when the ſhock was felt, but not ſo ſevere as through the iron rod. This demonſtrated, as was before ſuggeſted, why the electrical commotion was not felt ſtronger by the obſervers upon the weſtern ſhore of the *Weſtminſter-bridge*; *viz.* that *Portland* ſtone ſtanding in water will conduct Electricity very conſiderably.

The gentlemen then tried what would be the effect, if the obſerver at *B* ſtood upon a cake of wax, holding the wire as before, and touched the ground of the meadow with his iron rod at leaſt 150 feet from the water; and if the obſerver uſually placed near the river at *A*, had his wire carried 150 feet over the river, as the former, ſtood upon an originally-electric, and touched the ground with his iron rod. Upon the exploſion of the charged phial, which was ſeveral times done, both the obſervers were ſmartly ſtruck: this demonſtrated, that in theſe inſtances the moiſt ground of the meadows made part of the circuit. The obſervers were diſtant from each other about 500 feet.

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The obſervers then, ſtationed as in the laſt experiment, ſtood upon the wax cakes as before, without touching the ground with the iron rods, or any part of their bodies, and the charged phial was exploded 4 times. Theſe were not at all felt by the obſerver next to *B*, and without the greateſt attention, would not have been perceived by him next to *A*; and then only in ſome of the trials, the feeling of the Electricity was like that of a ſmall pulſe between the finger and thumb of that hand which held the wire. The loaded phial was again diſcharged 4 times more, without any other alteration in the diſpoſition of the *apparatus*, than that the obſerver next to *B* ſtood upon the ground; when the electrical commotion was perceived by that obſerver, though not ſo ſharp as when the other obſerver at the ſame time ſtood upon the ground. The obſerver next to *A* felt the tingling between his finger and thumb, as before.

The gentlemen were deſirous of trying the electrical commotion at a ſtill greater diſtance than any of the former, through the water, and where, at the ſame time by altering the diſpoſition of the *apparatus*, it might be tried, whether or no that power would be perceptible through the dry ground *only* at a conſiderable diſtance. *Highbury-barn* beyond *Iſlington* was thought a convenient place for this purpoſe, as it was ſituated upon a hill nearly in a line, and almoſt equidiſtant from 2 ſtations upon the *New River*, ſomewhat more than a mile aſunder by land, though following the courſe of that river, their diſtance from each other was 2 miles. The hill between theſe ſtations was of a gravelly ſoil; which, from the late continuance of hot weather without rain, was dry, full of cracks, and conſequently was as proper to determine whether or no the Electricity would be conducted by dry ground to any great diſtance, as could be deſired. This hitherto had not been attempted; the meadows in the inſtances before quoted conducting the Electricity, was ſuppoſed to be owing to the moiſture of the ground. The ſtreets of *London*, when very dry, had been found to conduct it ſtrongly about 40 yards, and the dry road at *Newington* about the ſame diſtance. Accordingly, on *Wedneſday*, *Aug.* 5, 1747, there met at *Highbury-barn* the R. Hon. the Lord *Ch. Cavendiſh*, the Rev. Mr *Birch*, Mr *George Graham*, *Rich. Graham*, Eſq; *N. Mann*, Eſq; Mr *Short*, *Daniel Wray*, Eſq; and myſelf, who were of the *Royal Society*, and Dr *Bevis*.

The electrifying machine being placed up one pair of the ſtairs in the houſe at *Highbury-barn*, a wire from the coated phial was conducted upon dry ſticks as before, to that ſtation by the ſide of the *New River*, which was to the northward of the houſe. The length of this wire was 3 furlongs and 6 chains, or 2376 feet. Another wire faſtened to the iron bar, with which, in making the exploſion, the gun-barrel was touched, was conducted in like manner to the ſtation upon the *New River* to the ſouthward of the houſe. The length of this wire was 4 furlongs 5 chains and 2 poles, or 3003 feet. The length of both wires, excluſive of their turnings round the ſticks, was 1 mile, 1 chain, and 2 poles, or 5379 feet.

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For the more conveniently deſcribing the experiments made here, we will call the ſtation to the northward *D*, and the other *E*.

At this diſtance the gentlemen propoſed to try, firſt, whether or no the electrical commotion was perceptible, if both the obſervers at *D* and *E*, ſupported by originally-electrics, touched the conducting wire with one hand, and the water of the *New River* with an iron rod held in the other? Secondly, whether or no that commotion was perceptible, if the obſerver at *E*, being in all reſpects as before, the obſerver at *D*, ſtanding upon wax, took his rod out of the water? Thirdly, whether or no that commotion was perceptible to both obſervers, if the obſerver at *D* was placed upon wax, and touched the ground with his iron rod in a dry gravelly field at leaſt 300 yards from the water?

As from the ſituation of the ground, trees, *&c.* neither of the ſtations could be ſeen by each other, or by the obſerver at the electrifying machine, it was agreed to diſcharge a gun as a ſignal to get ready, and to do the ſame, as near as might be, half a minute before each exploſion.

In theſe experiments, as well as the former, the coated phial was each time charged as high as it could be; ſo that if the difference of the ſhock to the obſervers was conſiderable, it was owing to other cauſes more than to the phial's being differently electrified.

To try the firſt propoſition, 8 exploſions were made with the obſervers at *D* and *E*, touching the water, and ſtanding upon wax, with their iron rods in the water. The firſt 2 of theſe were felt but weakly by the obſerver at *D*; but in the other 6 he was ſtrongly ſhocked. The obſerver at *E* felt nothing of the firſt 6 exploſions; when, upon examination, the wire was found broken by ſome accident; but this obſerver was ſtrongly ſhocked by the 2 laſt. The obſerver at *D* being ſhocked in 4 of theſe exploſions, while in theſe 4 the obſerver at *E* felt nothing, was owing to the circuits being formed by the ground between the obſerver at *D* and the broken wire. Upon account of the wire's being broken, the gentlemen tried 3 more exploſions, when the obſervers at both ſtations felt the electrical ſhock.

To try the ſecond propoſition, 4 exploſions were made with the obſerver at *D*, ſtanding upon an originally-electric, and taking his iron rod out of the water, the obſerver at *E* as before. In each of theſe the obſerver at *D* felt a ſmall pulſation between his finger and thumb of that hand which held the wire. The obſerver at *E* felt each of theſe as ſtrong as before. This being different from the obſervations made in the experiments of the laſt trials at our former ſtations *A* and *B*, and many others; where *B* in the ſame circumſtances with *E* here felt the electrical commotion only in a ſlight degree, was owing, as we were afterwards informed, to the impertinent curioſity of the ſervants of the gentlemen, and other voluntary obſervers, who, by touching the wire which went from the coated phial to the obſerver at *D*, felt the ſhock in their arms and ankles, and formed ſubordinate circuits to *E*. The preventing theſe people from touching the wires, was impoſſible; as great part of them could be ſeen neither

neither by the obſervers at the ſtations, nor by thoſe at the houſe, and their being more than a mile long.

The 4 other exploſions were made without any other alteration in the *apparatus*, than than that the obſerver at *D* ſtood upon the ground about 4 yards from the water without any communication therewith. The obſerver at *E* felt the ſhocks in his arms as before; but the obſerver at *D* ſtanding upon the ground was ſhocked in the elbow and wriſt of that arm which held the wire, and in both his ankles.

To try the third propoſition, 8 exploſions were made with the obſerver at *D* ſtanding upon an originally-electric with his rod in the water of the river as before; but the obſerver at *E* was placed in a dry gravelly field about 300 yards nearer the machine than his laſt ſtation, and about 100 yards diſtant from the river. He there ſtood upon the wax, holding the conducting wire in one hand, and touched the ground with an iron rod held in the other. The ſhock was each time felt by the obſerver at *D*, but ſenſibly weaker than in the former trials; but the obſerver at *E* felt them all equally ſtrong with the former; the 4 firſt in his arms, when he ſtood upon the wax, and touched the ground with his iron rod; the other 4 in his arm and ankles, when he ſtood upon the ground without the iron rod.

In ſome of theſe experiments, the obſervers at *D* felt a tingling as ſoon as they laid hold of the conducting wire. This was conjectured to be owing to the Electricity, which conſtantly runs off while the coated phial is filling, and preferably by the wire, as the beſt conductor.

From the ſeverity of the ſhock, the gentlemen, in ſome of theſe trials, did not chooſe to have the Electricity paſs through their bodies: but, as it was neceſſary for them to be ſenſible of the different degrees of the electrical commotions, they bound the conducting wire round one of their thumbs, and touched the iron rod with the fore-finger of the ſame hand; when the electrical commotion was felt only in ſo much of the finger and thumb of that hand, as completed the circuit.

By the experiments of this day, the gentlemen were ſatisfied, that the dry gravelly ground conducted the Electricity as ſtrongly as water; which though otherwiſe at firſt conjectured, they now found not to be neceſſary to convey that power to great diſtances; as well as that, from difference of diſtance only, the force of the electrical commotion was very little if at all impaired. They were convinced of the truth of the firſt of theſe facts, not only from both obſervers feeling the electrical commotion in the 8 laſt experiments, when the obſerver at *E* was at ſuch a diſtance from the water, but alſo from the obſerver at *D* feeling the ſhock ſo ſtrong in 4 of the firſt 6 exploſions, when the conducting wire to *E* being broke at about 100 yards diſtance from the houſe, that obſerver felt nothing.

In this laſt inſtance the circuit was formed from the phial by the obſerver at *D* and his wire, a line of ground which reached from the ſtation at *D* to the broken wire that lay upon the ground, and ſo much of this wire as reached to the ſhort iron rod, which touched the gun-barrel in

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making the exploſions. This induced the gentlemen to conclude (as from many experiments it was manifeſt, that when the intervening ſubſtances conduct Electricity equally well, the circuit was performed in the ſhorteſt manner poſſible), that when the obſervers holding their iron rods in the river at *D* and *E* were both ſhocked, the Electricity was not conveyed by the water of the river, being two miles in length, but by land, where the diſtance was only one mile; in which ſpace that power muſt neceſſarily paſs over the *New River* twice, through ſeveral gravel-pits, and a large ſtubble-field. So that, admitting the Electricity did not follow the track of the river, the circuit from *D* to *E* was at leaſt 2 miles; *viz.* ſomewhat more than one mile of wire, which conducted the Electricity from the houſe to the ſtations, and another mile of ground, the ſhorteſt diſtance between thoſe ſtations. The ſame inference was now drawn with regard to the experiments at *A*, *B*, and *C*, in the *New River* before recited; *viz.* that as in all of them the diſtance between the obſervers was much greater by water than by land, the Electricity paſſed by land from one obſerver to the other, and not by water.

From the ſhocks which the gentlemen received in their bodies, when the electrical power was conducted upon dry ſticks, they were of opinion, that from difference of diſtance ſimply conſidered, as far as they had yet experienced, the force thereof was very little if at all impaired. When they ſtood upon originally-electrics, and touched the water or ground with an iron rod, the electrical commotion was always felt in their arms and wriſts: when they ſtood upon the ground, and touched either the water or ground with their iron rods, they felt the ſhock in their elbows, wriſts, and ankles: when they ſtood upon the ground without the rod, the ſhock was always in the elbow and wriſt of that hand, which held the conducting wire, and in both ankles. The obſervers here being ſenſible of the electrical commotion in different parts of their bodies, was owing in the firſt inſtance to the whole of it's paſſing (becauſe the obſerver ſtood upon wax) through their arms, and through the iron rod: in the ſecond, when they ſtood upon the ground, the Electricity paſſed both through their legs, and through the iron: in the third, when they ſtood upon the ground without either wax or rod, the Electricity directed it's way through one arm, and through both legs to complete the circuit.

The gentlemen were deſirous of cloſing the preſent inquiry, by examining not only whether or no the electrical commotions were perceptible at double the diſtance of the laſt experiments in ground perfectly dry, and where no water was near; but alſo, if poſſible, to diſtinguiſh the reſpective velocities of Electricity and ſound. To execute this required the whole ſagacity and addreſs of the gentlemen concerned; for they had met with very great difficulties in the laſt day's operations, where the wire was conducted but little more than a mile; all which could not

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but be greatly augmented by doubling that diſtance; becauſe it was neceſſary, that the houſe, wherein the electrifying machine was placed, ſhould be viſible at leaſt at one of the ſtations; and that the ſpace between that houſe and the ſtations, through which the wire was conducted, ſhould be very little interſected by hedges, roads, or foot-paths; neither ſhould the wire in this ſpace be ſubject to be diſturbed by the horſes or cattle, which were grazing; nor ought it to touch in it's paſſage the trees or any other vegetables, which at this ſeaſon of the year were every-where luxuriant. To find a place within a convenient diſtance of *London* with theſe requiſites was not very eaſy; but at laſt, *Shooters-Hill* was pitched upon, as the moſt convenient.

As only one ſhower of rain had fallen during the preceding 5 weeks, the ground could not but be very dry; and as no water was near, if the electrical commotion was felt by the obſervers at the ſtations, it might be ſafely concluded, that water had no ſhare in conducting it.

Aug. 14. 1747. there met at *Shooters-Hill* for this purpoſe, the Rev. Mr *Birch*, the Rev. Mr Profeſſor *Bradley*, *Peter Daval*, Eſq; Mr *G. Graham*, *R. Graham*, Eſq; Mr *Nourſe*, *George Lewis Scott*, Eſq; Mr *Short*, *Charles Stanhope*, Eſq; and myſelf, who were of the *Royal Society*, and Dr *Bevis*.

It was here determined (as the gentlemen were ſatisfied from many of the former trials, that if, when the coated phial was diſcharged, the obſervers at the ſtations ſtood upon originally-electrics, and touched neither water nor ground with iron rods, or any part of their bodies, the electrical commotion would be ſcarcely perceptable) to make twelve exploſions of the coated phial, with an obſerver placed at the 7 mile ſtone, and another at the 9 mile ſtone, both ſtanding upon wax, and touching the ground with an iron rod. This number of exploſions was thought more neceſſary, as the obſervers at theſe ſtations were not only to examine whether or no the Electricity would be propagated to ſo great a diſtance; but if it were, the obſerver at the 7 mile-ſtone was by a ſecond watch to take notice of the time lapſed between feeling the electrical commotion, and hearing the report of a gun fired near the machine, as cloſe as might be to the inſtant of making the exploſion: and therefore, to examine this matter with the requiſite exactneſs, this number of exploſions ſhould be made.

To execute this, the electrifying machine was placed up one pair of ſtairs in a houſe upon the weſt ſide of *Shooters-Hill*; and a wire from the ſhort iron rod, with which the gun-barrel was touched in making the exploſions was conducted upon dry ſticks as before into a field near the ſeven mile-ſtone. The length of this wire, excluſive of it's turnings round the ſticks, was a mile, a quarter, and 8 poles, or 6732 feet. In great part of this ſpace it was found very difficult to ſupport the wire, on account of our ſcarcely being able to fix the ſticks in the ſtrong gravel there almoſt without any cover of ſoil; nor could the wire in ſome places

be prevented from touching the brambles and buſhes, nor in one field the ripe barley.

Another wire was likewiſe conducted upon ſticks from the coated phial to the nine mile-ſtone. In this ſpace, the ſoil being a ſtrong clay, the wire was very well ſecured, and in it's whole length did not touch the buſhes. The length of this wire was 3868 feet. As much as the place, where the obſervers were ſtationed in a corn-field, was nearer the machine than the 7 mile-ſtone, ſo much were the other obſervers placed beyond the 9 mile-ſtone, that their diſtance from each other might be 2 miles. The 40 feet of wire in theſe 2 meaſures exceeding 2 miles, was what connected the ſhort iron rod before-mentioned, and the coated phial, with their reſpective conducting wires.

The obſervers being placed at their reſpective ſtations, the obſerver at the machine proceeded in making the exploſions of the coated phial; he having before placed an aſſiſtant exactly in his view before the window of the houſe, who, upon the word of command, was to diſcharge a muſket. As ſoon as ever the flaſh was ſeen to come from the mouth of the gun, the obſerver diſcharged the electrified phial. When 8 exploſions had been made, a ſervant was ſent from the gentlemen at the 7 mile-ſtone giving an account of the wire's being broken, and the ſticks thrown down by a man riding through them; that the obſervers there had felt nothing; and deſired, as by this time the wire was replaced, that we ſhould begin again. This was complied with, and 12 other exploſions made without further moleſtation.

Not only the firſt 8, but eleven of the laſt 12 very ſtrongly ſhocked the obſervers at the 9 mile-ſtone: at the twelfth exploſion the obſerver on purpoſe ſtood upon the wax without touching the ground with his iron rod, or any part of his body; and only felt a ſlight tingling in his finger and thumb that held the wire. In another of theſe experiments, as the gentlemen here were ſatisfied in their own perſons of the ſtrength of the electrical commotion, they indulged 2 country-fellows, who were by-ſtanders, with feeling one: theſe 2 with 4 of the gentlemen formed a chain, the firſt of them taking hold of the extremity of the wire with one of his hands. They all ſtood upon the ground, and made no uſe of the iron rod. Upon the exploſion they were all ſo ſtrongly ſhocked in their arms and ankles, that the countrymen could by no means be prevailed upon to try the experiment again. Why, in the firſt eight exploſions, the obſervers here were ſenſible of the electrical commotion, when the obſervers at the other ſtation felt nothing, was explained in the former experiments. The obſervers at this ſtation, from their ſituation under the hill, and from what wind there was being againſt it, never heard the report of the gun.

Though the obſervers near the 7 mile-ſtone from the breaking of their wire, were not ſenſible of the 8 firſt exploſions of the charged phial, they felt the other 12. This demonſtrated to the ſatisfaction of the gentlemen concerned, that the circuit here formed by the Electricity was 4 miles;

miles; *viz.* 2 miles of wire, and 2 miles of ground, the ſpace between the extremities of that wire. A diſtance without trial too great to be credited! How much further the electrical commotion will be perceptible, future obſervations can only determine.

The electrical commotion by the obſervers near the 7 mile-ſtone was but ſlightly felt; not could it be otherwiſe expected, the wire in many parts of it's length touching, as was before-mentioned, the moiſt vegetables; which, in as many places as they were touched, formed ſubordinate circuits. We find, in all other inſtances, that the whole quantity of Electricity, accumulated in the coated phial, is felt equally through the whole circuit, when every part thereof is in a great degree non-electric; ſo here the whole quantity, or nearly ſo *, determined that way, was felt by the obſervers at the 9 mile-ſtone whilſt thoſe at the other ſtation felt ſo much of their quantity only, as did not go through the vegetables; that is, that proportion only in which iron is a greater non-electric than the vegetables.

Tho' the electrical commotions, felt by the obſervers near the 7 mile-ſtone, were not ſtrong; they were equally concluſive in ſhewing the difference between the reſpective velocities of Electricity and ſound.

The ſpace through which ſound is propagated in a given time, has been very differently eſtimated by the authors, who have written concerning this ſubject. *Roberval* gives it at the rate of 560 feet in a ſecond; *Gaſſendus*, at 1473; *Merſenne* at 1474; *Du Hamel*, in the Hiſt. of the *Acad. Sc. Par.* at 1172; the *Acad. del Cimento*, at 1185; *Boyle* at 1200; *Roberts* at 1300; *Walker* at 1338; Sir *I. Newton* at 968; Dr *Derham*, in whoſe meaſure Mr *Flamſtead* and Dr *Halley* acquieſced, at 1142. But by the accounts ſince publiſhed by M. *Caſſini de Thury* in the Memoirs of the *R. Acad.* of *Sciences* at *Paris* for the year 1738. where cannon were fired at various as well as great diſtances, under great variety of weather, wind, and other circumſtances, and where the meaſures of the different places had been ſettled with the utmoſt exactneſs, ſound was propagated at a *medium* at the rate only of 1038 *French* feet in a ſecond. The *French* foot exceeds the *Engliſh* by ſeven lines and a half, or is as 107 to 114: and conſequently 1038 *French* feet are equal to 1106 *Engliſh* feet. The difference therefore of the meaſures of Dr *Derham* and M. *Caſſini* is 34 *French* 36 *Engliſh* feet in a ſecond †. According to this laſt meaſure, the velocity of ſound, when the ‖ wind is ſtill, is ſettled at the rate of a mile, or 5280 *Engliſh* feet in $4'' \frac{77}{100}$.

* The author of this paper, from a great variety of experiments, is of opinion; that in this and the like diſpoſitions of the *apparatus*, the electrical power, accumulated in the matter contained in the coated phial, is directed upon the exploſion thereof towards both obſervers at the ſame inſtant.

† M. *Caſſini du Thury* afterwards meaſured the velocity of ſound at *Aiguemortes* in *Languedoc*, and found the obſervations there from thoſe made about *Paris* vary only half a toiſe in a ſecond. See *Mem. de l'Acad. Royale des Sciences, pour l'année* 1739, *p* 126.

‖ Dr *Derham* found, that when ſound was carried againſt the wind, not only it's diſtance but it's velocity was leſſened; and in M. *Caſſini*'s Memoir, there is an experiment, where ſound being carried againſt the wind, which then blew very ſtrong, was retarded near $\frac{1}{12}$ of the uſual time in it's progreſs.

To return to our purpoſe; the length of the conducting wire from the machine to the obſervers near the 7 mile-ſtone, was (as has been before-mentioned) a mile, a quarter, and 8 poles, or 6732 feet: the length of that to the 9 mile-ſtone, 3868 feet. The firſt of theſe meaſures only was made uſe of in the preſent operations concerning the velocity of Electricity. In 12 diſcharges of the coated phial, which were felt by Mr *G. Graham*, Mr *Short*, and *Ch. Stanhope*, Eſq; the obſervers near the 7 mile-ſtone, and who, by a ſecond watch of Mr *Graham*'s, meaſured the time between feeling the electrical commotion, and hearing the report of the gun, with the utmoſt attention and exactneſs; the time, I ſay, between feeling the electrical commotion, and hearing the report of the gun, was, at a *medium*, $5'' \frac{1}{4}$, or $5'' \frac{250}{1000}$. And as the gun was diſtant from theſe obſervers 6732 feet, it follows, from the experiments which have been made on the velocity of ſound, that the real inſtant of the diſcharge of the gun preceded that of the obſervers hearing it's report, at this time when the ſtrength of the wind was not ſo great as to enter into the computation, $6'' \frac{.087}{.1000}$; or preceded the inſtant when the electrical commotion was felt only $0'' \frac{827}{1000}$. But this inſtant was, from the nature of the experiment, neceſſarily prior to that of the electrical exploſion, which was not made 'till the fire of the gun was actually ſeen; and therefore the time between the making of that exploſion, and it's being actually felt by the obſerver, which muſt have been leſs than $0'' \frac{827}{1000}$, was really ſo ſmall, as not to fall under any certain obſervation, when it is to be diſtinguiſhed from that, which muſt of neceſſity be loſt, between the firing of the gun, and the electrical exploſion itſelf.

In all the experiments, where the circuit was formed to any conſiderable length, though the coated phial was very well charged, the ſnap at the gun-barrel, upon the exploſion, was not near ſo loud as when the circuit is formed in a room; ſo that a by-ſtander, though verſed in theſe operations, from ſeeing the flaſh, and hearing the report, would imagine the ſtroke at the ends of the conducting wire to be very ſlight; the contrary whereof, when the wire has been properly conducted, has always happened.

From a review of theſe experiments, the following obſervations may be deduced.

I. That, in all the preceding operations, when the wires have been properly conducted, the electrical commotions from the charged phial have been very conſiderable only, when the obſervers at the extremities of the wire have touched ſome ſubſtance readily conducting Electricity with ſome part of their bodies.

II. That the electrical commotion is always felt moſt ſenſibly in thoſe parts of the bodies of the obſervers, which are between the conducting wires, and the neareſt and the moſt non-electric ſubſtance; or in other words, ſo much of their bodies, as comes within the electrical circuit.

III. That

III. That, upon theſe conſiderations, we infer, that the electrical power is conducted between theſe obſervers by any non-electric ſubſtances, which happen to be ſituated between them, and contribute to form the electrical circuit.

IV. That the electrical commotion has been perceptible to 2 or more obſervers at conſiderable diſtances from each other, even as far as 2 miles.

V. That when the obſervers have been ſhocked at the end of 2 miles of wire, we infer, that the electrical circuit is 4 miles; *viz.* 2 miles of wire, and the ſpace of 2 miles of the non-electric matter between the obſervers, whether it be water, earth, or both.

VI. That the electrical commotion is equally ſtrong, whether it is conducted by water or dry ground.

VII. That if the wires between the electrifying machine and the obſervers are conducted upon dry ſticks, or other ſubſtances non-electric in a ſlight degree only, the effects of the electrical power are much greater than when the wires in their progreſs touch the ground, moiſt vegetables, or other ſubſtances in a great degree non-electric.

VIII. That by comparing the reſpective velocities of Electricity and ſound, that of Electricity, in any of the diſtances yet experienced, is nearly inſtantaneous.

I ſhall conclude this paper with obſerving, that it was thought convenient to lay a detail of all the operations relating to theſe experiments before the *Society*; in conſequence of which the gentlemen may make themſelves judges, how far the deductions here recited are warrantable from the experiments.

* The gentlemen concerned were deſirous, if poſſible, of aſcertaining the abſolute velocity of Electricity at a certain diſtance; becauſe, although laſt year, in meaſuring the reſpective velocities of Electricity and ſound, the time of its progreſs was found to be very little, yet we were deſirous of knowing, ſmall as that time was, whether it was meaſurable; and I had thought of a method for this purpoſe.

Accordingly, *Auguſt* 5. 1748. there met at *Shooters-Hill* for this purpoſe, the Preſ of the *R. Soc.* the Rev. Mr *Birch*, the Rev. Mr Profeſſor *Bradley*, *James Burrow*, Eſq; Mr *Ellicot*, Mr *G. Graham*, *Rich. Graham*, Eſq; the Rev. Mr *Lawrie*, *Charles Stanhope*, Eſq; and myſelf, who were of the *Royal Society*, Dr *Bevis*, and Mr *Griſchow*, a member of the *R. Acad.* of *Sciences* at *Berlin*.

It was agreed to make the electrical circuit of 2 miles, in the middle of which an obſerver was to take in each hand one of the extremities of a wire, which was a mile in length. Theſe wires were to be ſo diſpoſed,

* Theſe experiments to meaſure the abſolute velocity of Electricity were made whilſt this paper was at the preſs; but as they had ſo near a relation to the experiments made the preceding year, it was thought proper to inſert them here.

that

that this obſerver being placed upon the floor of the room near the electrifying machine, the other obſervers might be able in the ſame view to ſee the exploſion of the charged phial, and the obſerver holding the wires, and might take notice of the time lapſed between the diſcharging the phial, and the convulſive motions of the arms of the obſerver in conſequence thereof; inaſmuch as this time would ſhew the velocity of Electricity, through a ſpace equal to the length of the wire between the coated phial and this obſerver.

The electrifying machine was placed in the ſame houſe as it was laſt year. We then found ourſelves greatly embarraſſed by the wire's being conducted by the ſide of the road, which we were compelled to, on account of the ſpace neceſſary for the meaſuring of ſound: but ſo great a diſtance from the machine was not now wanted, though the circuit through the wire was intended to be at leaſt 2 miles. We had diſcovered by our former experiments, that the only caution now neceſſary was, that the wires conducted upon dry ſticks ſhould not touch the ground, each other, or any non-electric in a conſiderable degree in any part of their length: if they did not touch each other, the returns of the wire, be they ever ſo frequent, imported little, as the wire had been found to conduct Electricity ſo much better than the ſticks. It was therefore thought proper to place theſe ſticks in a field 50 yards diſtant from the machine. The length of this field being 11 chains, or 726 feet, 8 returns of the wire from the top to the bottom of the field made ſomewhat more than a mile, and 16 returns more than 2 miles, the quantity of wire intended for the Electricity to paſs through to make the experiment.

We had found laſt year, that, upon diſcharging the electrified phial, if two obſervers made their bodies part of the circuit, one of which graſped the leaden coating of the phial in one hand, and held in his other one extremity of the conducting wire; and if the other obſerver held the other extremity of the conducting wire in one hand, and took in his other the ſhort iron rod with which the exploſion was made; upon this exploſion, I ſay, they were both ſhocked in the ſame inſtant, which was that of the exploſion of the phial. If therefore an obſerver, making his body part of the circuit, was ſhocked in the inſtant of the exploſion of the charged phial in the middle of the wire, no doubt would remain of the velocity of Electricity being inſtantaneous through the length of that whole wire. But if, on the contrary, the time between making the exploſion, and ſeeing the convulſions in the arms of the obſerver holding the conducting wires, was great enough to be meaſured, we then ſhould be able to aſcertain it's velocity to the diſtance equal to half the quantity of wire employed only, let the manner of the Electricity's diſcharging itſelf be what it would.

It has been a queſtion with ſome, who have conſidered this ſubject, whether the Electricity, in compleating the circuit from the matter contained in the glaſs, paſſed, either by the wire in the mouth to the coating of the glaſs, the contrary way by the coating to the wire in the mouth,

mouth, or otherwiſe directed itſelf both ways at once? That the Electricity muſt paſs off one of theſe 3 ways was certain, as the exploſion would not be complete, unleſs in the inſtant thereof ſome matter very non-electric communicated between the wire in the mouth, and the coating of the glaſs. Unleſs therefore the obſerver was placed in the centre of the conducting wires, it might be objected, that the experiment was not made with the exactneſs neceſſary; becauſe any perſon, who was of opinion that the Electricity directed itſelf from the mouth of the glaſs to the coating, might object, if the wire from the ſhort iron rod to the obſerver was only half the length of that between the obſerver and the coating of the glaſs, that the Electricity in the time found, paſſed only through the ſhort wire, and *vice verſa*. But if, as it was here thought proper, the obſerver was placed in the centre of the conducting wire, let the direction of the Electricity be what it would, no difference could happen in the reſult of the experiments, if made with the neceſſary caution; becauſe, if the effects in the middle and both ends of the wires were inſtantaneous, the concluſion therefrom would be very obvious.

To make the experiment, the ſame phial filled with filings of iron, and coated with ſheet-lead, which was uſed laſt year, was placed in the window of the room near the machine, and was connected to the prime conductor by a piece of wire. To the coating of this phial a wire was faſtened, which, being conducted upon dry ſticks to the before-mentioned field, was carried in like manner to the bottom, and being conducted thus from the bottom of the field to the top, and from the top to the bottom 7 other times, returned again into the room, and was held in one hand of an obſerver near the machine. From the other hand of this obſerver, another wire of the ſame length with the former was conducted in the ſame manner, and returned into the room, and was faſtened to the iron rod with which the exploſion was made. The whole length of theſe wires, allowing 10 yards for their turns round the ſticks, amounted to 2 $\frac{1}{4}$ miles and 6 chains, or 12276 feet.

As the night preceding theſe experiments had been very rainy, care was taken, by ſilk lines properly diſpoſed, that the wires in their paſſage from the window of the houſe might not touch the wood thereof; leſt, from the moiſture of this wood, the electrical circuit might be ſhortened.

When all parts of the *apparatus* were properly diſpoſed, ſeveral exploſions of the charged phial were made; and it was invariably ſeen, that the obſerver holding in each hand one of the extremities of theſe wires, was convulſed in both his arms in the inſtant of making the exploſions.

Inſtead of one, 4 men were then placed, holding each other by the hand near the machine, the firſt of which held in his right hand one extremity of the wire, and the laſt man the other in his left. They were all ſeen convulſed in the inſtant of the exploſion. Every one who felt it, complained of the ſeverity of the ſhock.

It

It was then desired by one of the gentlemen concerned, that an explosion should be made with the observer holding only one of the wires. This was done accordingly; but the observer felt nothing, the phial discharging itself in a different manner to what it did before, on account of the circuit's not being completed.

It was then tried, whether an observer would be shocked upon the discharge of the phial, if the 2 wires at their extremities slightly touched each other, whilst an observer at the same time held one of these about a foot from their ends in each of his hands? Upon trial he felt nothing, though the phial exploded very quick, because the iron wire conducted the Electricity better than the body of the observer.

It was then tried, whether or no, as the ground was wet, if the explosion was made with the observer holding the extremity of each wire standing upon the ground near the window of the house, any difference would arise in the success of the experiment? No difference was found, the observer being shocked in the instant of the explosion as before, in both his arms, and across his breast.

Upon these considerations we were fully satisfied, that through the whole length of this wire, being, as I mentioned before, 12276 feet, the velocity of Electricity was instantaneous.

As it was found last year, we observed again, that although the electrical commotions were very severe to those who held the wires, the report of the explosion at the prime conductor was little, in comparison of that which is heard when the circuit is short. From whence it was conjectured, that the very loud report, in the experiment of *Leyden*, is confined to a very short circuit.

Fig. 18. *A*, The prime conductor. *BB*, the silk lines. *C*, the coated phial. *D*, it's hook communicating with the prime conductor. *EE*, the wire reaching from the coating of the phial to the left hand of the observer, being more than a mile in length. *F*, the place of the observer. ⌒, a supposed line, drawn upon the explosion through his body and arms. *GG*, another wire, of the length of *EE*, which goes from the right hand of the observer to *H*. *H*, the short iron rod to make the explosion.

§ I. *Some further inquiries into the nature and properties of* Electricity; *by the same.* Ibid. p 93.

28. The last paper contained some accounts of what had been done by some gentlemen of the *Society*, in order to examine, not only to what distance the electrical power was perceptible, but also to investigate, as near as might be, the respective velocities of Electricity and sound: Electricity indeed is the subject of the present paper, yet, as it relates to *phænomena* thereof different from those mentioned in the former, I thought proper to separate them.

***Read* Jan. 21. 1747-8.**

§ II. I took notice, in my sequel to the experiments relating to Electricity *, of an observation of the ingenious Professor *Bose* of *Wittemberg*, *viz.* ‘ that if the electrifying machine is placed upon originally-electrics,

* Art. 9.

‘ the

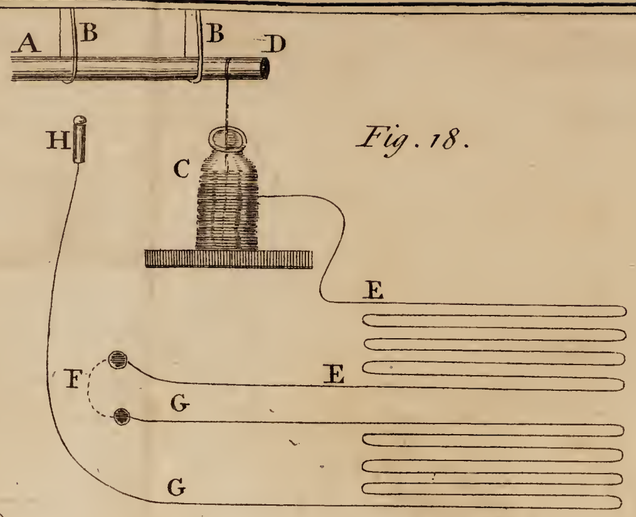


Fig. 18.

Fig. 16.

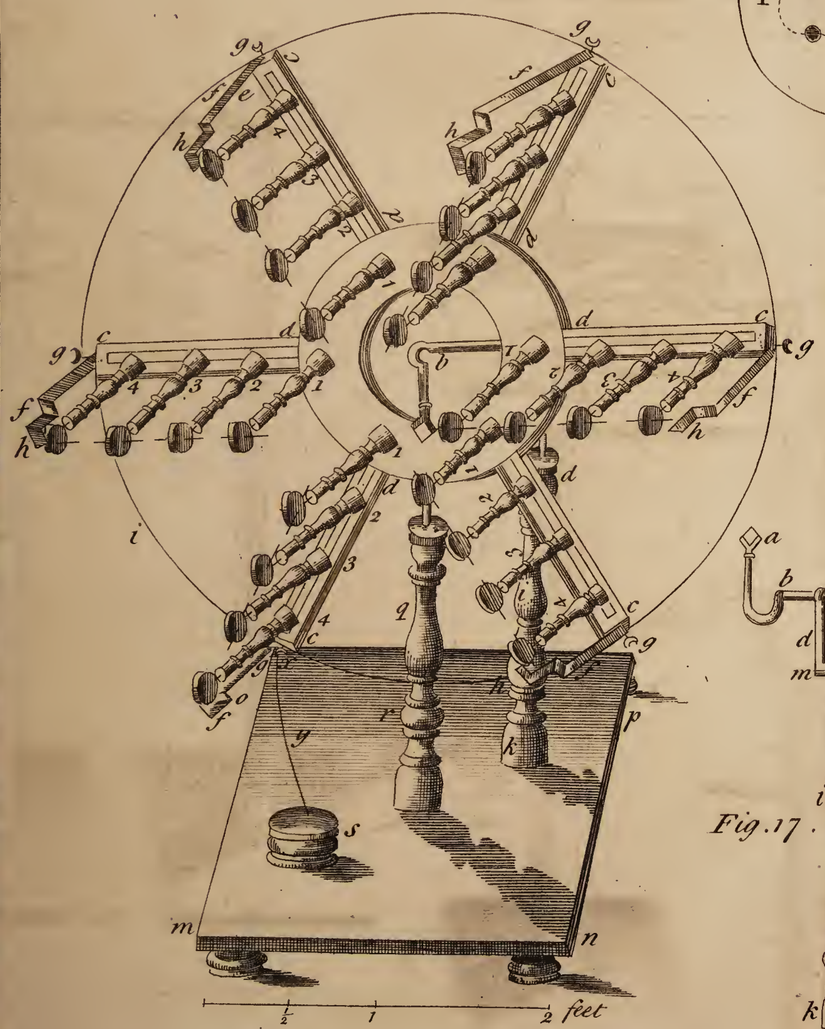
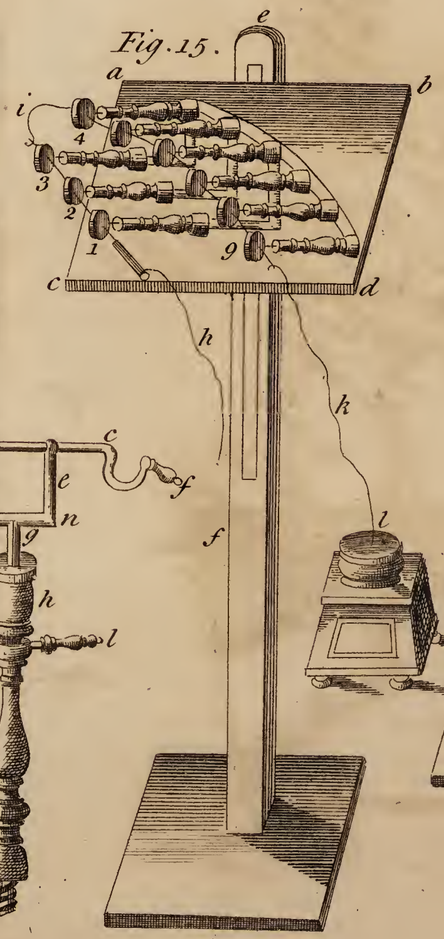


Fig. 15.



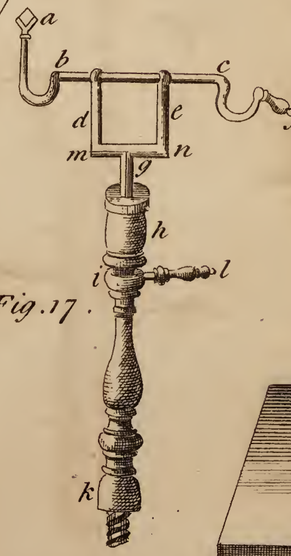


Fig. 17.

Fig. 14.

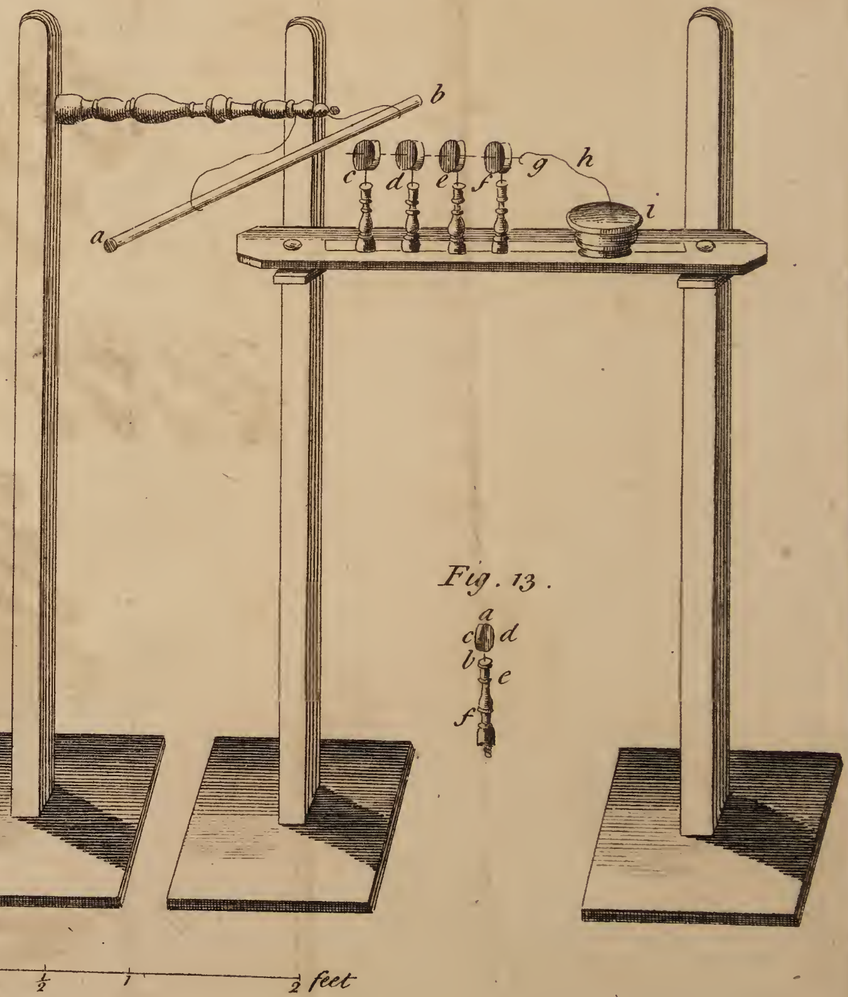


Fig. 13.

'the man who rubs the globe with his hands, even under theſe appa-
'rently favourable circumſtances, gives no ſign of being electriſed when
'touched by an unexcited non-electric. But if another perſon, ſtanding
'upon the floor, does but touch the globe in motion with the end of one
'of his fingers, or any other non-electric, the perſon rubbing is inſtantly
'electriſed, and that very ſtrongly.' This experiment, almoſt a year ſince, Dr *Bevis* carried further, by placing whatever non-electric touched the globe as a conductor, whether it were a man or a gun-barrel, upon originally-electrics. If then, either the man who rubbed the globe, or he who only held his finger near the equator thereof, were touched by any perſon ſtanding upon the floor, a ſnapping from either of them, I ſay, was perceptible upon that touch.

§ III. As in my ſequel I had aſſerted, and by many experiments therein had endeavoured to evince, that, contrary to the received opinion, the Electricity was not derived from glaſs, the air, or other electrics *per ſe*, I was deſired to conſider how far this experiment did not prove the reverſe of that aſſertion; inaſmuch as neither the man who rubbed the globe, or he who touched it with his finger, from their being here both ſupported by originally-electrics, could receive any ſupply from the floor; and yet both of them ſnapped upon the touch of a perſon not ſupported by electrics *per ſe*. Many experiments had proved that the Electricity was not derived from the glaſs; and therefore it was concluded, by Dr *Bevis*, and ſeveral others to whom this gentleman ſhewed the experiment, that the Electricity here was communicated to the perſon rubbing from the air, by means either of the ſuſpended gun-barrel, or of the man who touched the globe.

§ IV. I was by no means ſatisfied with this concluſion, as being directly contrary to numberleſs facts. From a careful conſideration therefore of the experiment itſelf, from comparing it's effects with thoſe of ſeveral others, and, in general, from ſurveying all the properties of Electricity we are hitherto acquainted with, I gave the following as my opinion.

1. That what we call Electricity is the effect of a very ſubtile and elaſtic fluid, diffuſed throughout all bodies in contact with the terraqueous globe (thoſe ſubſtances hitherto termed Electrics *per ſe* probably excepted), and every-where, in it's natural ſtate of the ſame degree of denſity.
2. That this fluid manifeſts itſelf only, when bodies capable of receiving more thereof than their natural quantity are properly diſpoſed for that purpoſe; and that then, by certain known operations, it's effects ſhew themſelves by attracting and repelling light ſubſtances, by a ſnapping noiſe, ſparks of fire, *&c.* directed towards other bodies, having only their natural quantity, or, at leaſt, a quantity leſs than thoſe bodies from which theſe ſnappings, *&c.* proceed.
3. That no ſnapping is obſerved in bringing any two bodies near each other, in which the Electricity is of the ſame denſity, but only in thoſe bodies in which the denſity of this fluid is unequal.

4. That this ſnapping is greater or leſs, in proportion to the different denſities of the Electricity in bodies brought near each other, and by which ſnapping each of them becomes of the ſame ſtandard.
5. That glaſs, and other bodies, which we call Electrics *per ſe*, have the property of taking this fluid from one body, and conveying it to another, and that in a quantity ſufficient to be obvious to all our ſenſes.
6. That, in the experiment in queſtion, the reaſon why no ſnapping is obſerved by a perſon upon the floor touching him who rubs the globe with his hands ſtanding upon wax, without at the ſame time ſome other non-electric ſupported by originally-electrics, or otherwiſe being in contact with the globe, is owing to whatever part of this man's natural quantity of Electricity, taken from himſelf by the globe in motion, being reſtored to him again by the globe in it's revolutions; there not being any other non-electric near enough to communicate the Electricity to; and that therefore, in this ſituation, the Electricity of this man ſuffers no diminution of it's denſity.
7. That the fact is otherwiſe, when every thing elſe being as before, either a gun-barrel ſuſpended in ſilk lines, or a man ſupported by wax, or ſuch like, is placed near the globe in motion; becauſe then, whatever part of the Electricity of the perſon rubbing is taken from him, is communicated either to the other man or to the gun-barrel, theſe, from their ſituation, being the firſt non-electrics, to which the Electricity taken from the perſon rubbing can be communicated.
8. That, under theſe circumſtances, as much Electricity as is taken from the perſon rubbing, is given to the other; by which means the Electricity of the firſt man is more rare than it naturally was, and that of the laſt more denſe.
9. That the Electricity in either of theſe perſons is in a very different ſtate of denſity from what it naturally was, or from that of any perſon ſtanding upon the earth; this laſt being in a middle ſtate between the two other perſons; that is, he has not his Electricity ſo rare as the man rubbing the globe, nor ſo denſe as that of him ſupported by electrics *per ſe*, and touching the equator of the globe.
10. That therefore the ſame effect, a ſnapping, is obſerved, upon bringing any non-electric near either of theſe perſons, from very different cauſes: for it is apprehended, that, by bringing the non-electric near him, whoſe Electricity is more rare, this ſnapping reſtores to him what he had loſt; and that, by bringing it near him, whoſe Electricity is more denſe, it takes off his ſurcharge, by which means their original quantity is reſtored to each.

§ V. This ſolution of this *phænomenon*, without allowing any part of the Electricity of either of theſe two perſons to be furniſhed by the circumambient air, was ſatisfactory, not only to the gentleman who propoſed it, but to many of the *Royal Society*, excellent judges of this matter,

matter, to whom I ſhewed the experiment: and this the more ſo, as it is to be obſerved, that if, under the before-mentioned circumſtances, the perſon rubbing the globe was touched by him who held his finger to the globe, the ſnapping was much greater than if either of them touched a perſon ſtanding upon the floor; as the denſity of the Electricity between theſe two perſons was ſo much more different than that of either of them to him on the floor: whereas did their Electricity proceed from the air, from their being both electriſed they ought not to ſnap at all from their touching each other; or, admitting they did touch each other, they both of them, upon a ſuppoſition that they did receive their Electricity alike from the air, ſhould manifeſt the accumulation thereof, and ſnap upon the touch of a man ſtanding upon the floor, the contrary of which invariably happens.

§ VI. At this time I am the more particular concerning the ſolution of this ſingular appearance, as Mr *Collinſon*, has received a paper concerning Electricity from an ingenious gentleman, Mr *Franklin*, a friend of his in *Penſylvania*. This paper, dated *June* 1. 1747, I very lately peruſed, by favour of our moſt worthy *Preſident*. Among other curious remarks there is a like ſoluſion of this fact; for though this gentleman's experiment was made with a tube inſtead of a globe, the difference is noways material. As this experiment was made, and the ſolution thereof given, upon the other ſide of the *Atlantic Ocean* before this gentleman could poſſibly be acquainted with our having obſerved the ſame fact here, and as he ſeems very converſant in this part of Natural Philoſophy, I take the liberty of laying before you his own words.

'1. A perſon ſtanding on wax, and rubbing a tube, and another perſon 'on wax drawing the fire; they will both of them, provided they do 'not ſtand ſo as to touch one another, appear to be electriſed to a perſon 'ſtanding on the floor; that is, he will perceive a ſpark on approaching each of them with his knuckle.

'2. But if the perſons on wax touch one another during the exciting 'of the tube, neither of them will appear to be electriſed.

'3. If they touch one another after the exciting the tube and drawing the fire as aforeſaid, there will be a ſtronger ſpark between them, 'than was between either of them and the perſon on the floor.

'4. After ſuch a ſtrong ſpark neither of them diſcover any Electricity.

'Theſe appearances we attempt to account for thus:

'We ſuppoſe, as aforeſaid, that electrical fire is a common element, of 'which every one of theſe three perſons has his equal ſhare before 'any operation is begun with the tube. *A*, who ſtands upon wax, 'and rubs the tube, collects the electrical fire from himſelf into the 'glaſs; and his communication with the common ſtock being cut off 'by the wax, his body is not again immediately ſupplied. *B*, who

'ſtands upon wax likewiſe, paſſing his knuckle along near the tube, 'receives the fire which was collected by the glaſs from *A*; and his 'communication with the common ſtock being cut off, he retains the 'additional quantity received. To *C* ſtanding on the floor, both ap-'pear to be electriſed: For he, having only the middle quantity of 'electrical fire, receives a ſpark upon approaching *B*, who has an over 'quantity, but gives one to *A*, who has an under quantity. If *A* and '*B* approach to touch each other, the ſpark is ſtronger; becauſe the 'difference between them is greater. After ſuch touch, there is no 'ſpark between either of them and *C*, becauſe the electrical fire in 'all is reduced to the original equality. If they touch while electri-'ſing, the equality is never deſtroyed, the fire only circulating. Hence 'have ariſen ſome new terms among us. We ſay, *B*. (and bodies 'alike circumſtanced) is electriſed poſitively; *A*, negatively; or, 'rather, *B* is electriſed *plus*, *A*, *minus*. And we daily in our experi-'ments electriſe *plus* or *minus*, as we think proper. To electriſe *plus* 'or *minus*, no more needs be known than this; that the parts of the 'tube or ſphere that are rubbed, do in the inſtant of the friction at-'tract the electrical fire, and therefore take it from the thing rubbing. 'The ſame parts immediately, as the friction upon them ceaſes, are 'diſpoſed to give the fire, they have received, to any body that has 'leſs. Thus you may circulate it, as Mr *Watſon* has ſhewn *; you 'may alſo accumulate or ſubſtract it upon or from any body, as you 'connect that body with the rubber, or with the receiver, the com-'munication with the common ſtock being cut off.'

The ſolution of this gentleman, in relation to this *phænomenon*, ſo exactly correſponds with that which I offered very early laſt ſpring, that I could not help communicating it.

§ VII. In Sect. 51. and 62. of my ſequel, from not having conſidered this experiment in a ſtatical view, and from not then imagining the velocity of Electricity ſo great as we ſince have found it, I concluded, that the ſnapping obſerved, if a perſon ſtanding upon the floor touched the man ſtanding upon wax, who turned the wheel of the electrifying machine placed likewiſe upon wax, to be owing to the inverſion of the uſual courſe of the Electricity; as that ſnapping was only conſtant, when the gun-barrel ſuſpended in ſilk lines was touched by non-electrics. As from divers experiments I had found that Electricity was not furniſhed by dry air, by many more that it could not come down clean ſilk lines; and as, from his ſnapping, the man upon the wax argued the preſence of Electricity, I conceived that this could happen no other way, than that the rubbing of the globe by a cuſhion or the hand of a man, gave it a fitneſs to take off the Electricity, furniſhed by the ſuſpended gun-barrel from the non-electric upon the floor, and lodge it upon the machine, and upon the man who turned the wheel thereof. But the expe-

* *See* Art. 9. § 64.

riment

riment of circulating the electrical fire *, where the bruſh of blue flame from a blunt wire properly diſpoſed, can always be ſeen to paſs diverging into the machine, though not ſo, when brought near the gun-barrel under the moſt favourable circumſtances; as well as the experiment before-mentioned brought to ſhew that the Electricity came from the air, have induced me to change my opinion; and inſtead of the courſe of the Electricity being inverted, the *phænomena* aroſe, as far as I am capable of judging, from the man who turned the wheel of the electrifying machine having leſs than his original quantity of Electricity, and the gun-barrel from having more: to theſe add, that the perſon, who touched theſe while ſtanding upon the floor, had a quantity different from each of theſe, that is, his natural quantity.

§ VIII. I beg leave to correct alſo what I mentioned in my *ſequel*, in relation to my ſuggeſting, that, in the exploſion of the charged phial through the body of a man, or other non-electrics, as much Electricity as was taken from his body, was immediately replaced by the floor of the room upon which he ſtood: I having ſince found, that the charged phial would explode with equal violence, if the hook of the wire, which is uſually run through the cork of the phial, was bent in ſuch a manner as to come near the coating of the phial, without any other non-electric being near, from which ſuch quantity could be ſupplied.

§ IX. I take notice of theſe, inaſmuch as, notwithſtanding the very great progreſs that has been made in our improvements in this part of Natural Philoſophy within theſe few years, poſterity will regard us only as in our noviciate; and therefore it behoves us, as often as we can be juſtified therein by experiment, to correct any concluſions we may have drawn, if others yet more probable preſent themſelves.

§ X. I laid down and conſidered largely in my ſequel, that the ſtroke from the phial, in the experiment of *Leyden*, was not in proportion to the quantity of matter contained in the glaſs, but was increaſed by the quantity of matter in the glaſs, and the number of points of non-electrical contact on the outſide of the glaſs. This fact I have purſued further, and increaſed thereby the electrical exploſion to an aſtoniſhing degree. To this end I procured 3 cylindrical phials blown very thin, about 17 inches in height and 4 in diameter: after theſe were coated within an inch of their necks with ſheet-lead, I put into each 50 pounds of leaden ſhot. I choſe this form for the glaſſes, that the matter therein contained might be expoſed under as large a ſurface, as could conveniently be obtained. Theſe glaſſes were placed near each other in a convenient part of my room, and did communicate with each other by means of a ſmall iron rod lying upon all their mouths, and touching pieces of ſtrong wire ſtuck into the ſhot contained in them: by this management one of theſe could not be electriſed without communicating with the reſt. The leaden coatings of theſe glaſſes were alſo connected together by ſmall wires, all which centered in one tail wire; ſo that, when the matter contained in

* Art. 9. § 65.

theſe

theſe 3 glaſſes was replete with Electricity, which was done by a wire from the gun-barrel faſtened to the iron rod lying upon their mouths, the whole quantity of Electricity here accumulated might be diſcharged at once by touching the gun-barrel with an iron rod faſtened to the tail wire. When the glaſſes are ſufficiently electriſed, if the room is dark, you will ſee bruſhes of blue flame from ſeveral parts of the conducting wire; and theſe indicate the proper time of making the exploſion. Theſe glaſſes, from the thinneſs of their ſides, and from the weight of their leaden ſhot, are very liable to burſt; and if one of them happens to have the leaſt crack in any part of it's ſurface, which is under the lead, none of them can be electriſed; all the Electricity paſſing off by that crack. The electrical exploſion from 2 or 3 of theſe glaſſes is not double or treble to that from one of them; but the exploſion from three is much louder than that from two, that from two much louder than that from one.

§ XI. The experiment juſt mentioned induced me to imagine, that the exploſion from theſe phials was owing to the great quantity of non-electric matter contained in them: and whilſt I was conſidering of ſome certain method of aſſuring myſelf whether the fact were ſo, Dr *Bevis* informed me, that he had found the electrical exploſion to be as great, as when he had accumulated the Electricity in a half pint phial of water, by the following method. He covered a thin plate of glaſs, of about a foot ſquare on both ſides, with leaf-ſilver; this he made to adhere to the glaſs with very thin paſte. A margin of an inch was left on both ſides; otherwiſe, upon electrifying this plate, the Electricity would be prevented from being accumulated upon one of it's ſurfaces, by being propagated from the ſilver on one ſide to that of the other. When the glaſs plate was thus prepared, if it was placed upon a table in ſuch a manner, that when fully electriſed by a wire or ſuch-like from the prime conductor, a perſon touched the under ſurface with a finger of one of his hands, and brought one of the fingers of his other near the upper ſurface thereof, or near the prime conductor, he was ſhocked in both his arms and acroſs his breaſt. The ſame effect happened, if, when this plate was electrified in the before-mentioned manner, a perſon holding it in his hand by the margin, and without touching the ſilver, preſented it, even ſome time after it had been taken from the prime conductor, to another perſon who touched the under ſurface with his finger, and held it there till he touched the upper ſurface with a finger of his other hand.

§ XII. This experiment was ſufficiently convincing, that the greatneſs of the electrical exploſion, in my former trials, was not owing ſolely to the great quantity of non-electric matter contained in the glaſſes; as the exploſion from the glaſs plate ſilvered was occaſioned by about ſix grains of ſilver, upon which the Electricity was accumulated; more eſpecially as this exploſion was equal, if not ſuperior, to that from half a pint of water contained in a thin glaſs as uſual, under the moſt favourable circumſtances.

As

As each of the ſurfaces of the glaſs plate juſt mentioned meaſured 64 § XIII.
ſquare inches, I was deſirous of purſuing this inquiry further; and accordingly procured a cylindrical glaſs jar blown very thin, of 16 inches in height, and 18 inches in circumference. This I cauſed to be covered both within and without with leaf-ſilver, to within an inch of it's top. This glaſs with it's margin made very clean (upon which the ſucceſs of the experiment conſiderably depends) was fully electriſed by the means of a piece of chain, let down to the bottom of the jar, by a wire from the prime conductor; and the exploſion made by it's being placed upon a plate of metal, to which was faſtened a wire connected to an iron rod, and this rod was brought near ſome gilded leather lying upon the prime conductor. This exploſion was equal to that from the 3 glaſſes before-mentioned, containing 150 pounds of leaden ſhot; though here the weight of the ſilver lining the internal ſurface of the glaſs, upon which the Electricity was accumulated, did not exceed 30 grains. So much of the internal ſurface of this jar, as was covered with ſilver, amounted, as the ſurfaces of cylinders are as their length multiplied by their periphery, and allowing 36 ſquare inches for the bottom, to 306 ſquare inches. If this exploſion was made in a dark room, the coruſcations within the jar, at the inſtant of the exploſion, were extremely brilliant.

When this jar is fully electriſed, if, inſtead of making it explode, you only bring the ſhort iron rod, with which the exploſion is uſually made, near a piece of gilded leather lying upon the prime conductor, though not near enough to make the glaſs explode at once, you hear the Electricity, accumulated within the jar, eſcape with a noiſe very like that of a ſmall heated iron bar quenching in water.

The great exploſion from the jar before-mentioned, when ſo little § XIV.
non-electric matter was included therein, has cauſed me to be of opinion, that the effect of what we call the experiment of *Leyden* is greatly increaſed, if not principally owing, not ſo much to the quantity of non-electrical matter contained in the glaſs, as to the number of points of non-electrical contact * within the glaſs and the

* Bodies having the power of readily conducting Electricity ſeems to depend very little upon their ſpecific gravity ſimply conſidered: metals, for inſtance, and water, are in a great degree non-electrics, and conſequently conduct Electricity the beſt of any ſubſtances, that have yet fallen under our notice; whereas the *calces* of metals, though very denſe bodies, and very greatly more ſo than water, prevent in a great degree the quick propagation of the electrical power So that a phial coated within and without with ceruſe, *i. e.* the *calx* of lead, and electriſed, did not, upon the application as uſual of one hand to the external ſurface thereof, and touching the prime conductor with the other, occaſion any ſhock, or make any exploſion more than the ſimple ſtroke from the prime conductor. The ſame obſervation holds good with regard to red lead, litharge, and lunar cauſtic or the *calx* of ſilver, none of which ſnap, when electriſed. For the ſame reaſon, filings of iron, which are ruſty, *i. e.* have their ſurfaces converted into a *calx*, are much leſs proper to be put in glaſſes to make the experiment of *Leyden*, than thoſe that are not; inaſmuch as theſe laſt cauſe a much louder exploſion than the firſt. The making uſe of ruſty filings of iron was the occaſion of my mentioning in my *ſequel*, § 16. that the ſtroke from theſe was leſs than that from water; the contrary of which I afterwards found true, when filings of iron not ruſty were ſubſtituted.

denſity,

denſity * of the matter conſtituting thoſe points, provided this matter be in it's own nature a ready conductor of Electricity. For this reaſon it is preſumed, that ſo much of the lead contained in the ſhot in the before-mentioned experiment, only concurred to make the electrical exploſion, as touched the internal ſurface of the glaſs: as a great part of this ſurface was without contact, occaſioned by ſuch of the ſhot as preſented themſelves thereto, touching, from their ſpherical figure, only in one point, there conſequently remained without contact comparatively great ſpaces between each ſhot. This defect was obviated by the univerſal contact of the ſilver, and thereby was occaſioned the greater exploſion.

§ XV. The following experiment has ſome relation to the preceding. If a phial of warm water, without being coated with ſheet-lead, or other non-electrical matter, is electriſed by connecting it to the prime conductor; and a ring of ſmall wire, in lieu of the uſual coating, is put round this phial, the wire being continued of a ſufficient length to touch the prime conductor; upon diſcharging the phial, you have a ſlight exploſion, and a flaſh of fire ſeems at that inſtant to fill the glaſs. But if this experiment is made in a very dark room, and with great attention, this flaſh in the phial will not then ſeem to proceed from the whole quantity of water contained therein; but, as far as the ſuddenneſs of the exploſion will permit the eye to follow it, will be ſeen to occupy only the internal ſurface of the phial.

§ XVI. I ordered another glaſs jar as large as poſſible to be blown, ſo that the glaſs thereof might be very thin; and after many attempts of the glaſs-makers I procured one, the height of which was 22 inches, the periphery 41. This was covered within and without, leaving a margin of an inch at top, with leaf-braſs. As much of the internal ſurface as was covered amounted to 1129 ſquare inches. But the difficulty I met with in procuring this glaſs, was ſufficiently recompenſed by the great increaſe of the exploſion therefrom, when fully electriſed, and diſcharged in the ſame manner as the glaſs jar before-mentioned. The report was vaſtly louder; all the attendant *phænemona* greatly exceeded any thing of this kind I was before acquainted with. As the quantity of metal within this jar did not exceed 2 drams, this experiment gives further weight to my opinion before-mentioned § 14. in relation to the manner of increaſing the effects of the experiment of *Leyden*; and from what the *phænemona* of that ſurpriſing experiment principally proceed; *viz.* not from the volume of the prime conductor, nor from the quantity of non-electrical matter contained in the glaſs, but from the number of points of non-electrical contact both within and withoutſide of the glaſs, and from the denſity † of the matter conſtituting thoſe points.

* I heretofore took notice, how much the effect of this experiment depended upon the quantity of non-electric contact upon the outſide of the glaſs.

† Though the denſity of the matter conſtituting theſe points proceeds from their number in a mathematical ſenſe, yet in a popular one I take the liberty to diſtinguiſh them.

It

It muſt be obſerved, that, *cæteris paribus*, the electrical exploſion is greater from hot water included in glaſſes than from cold; and from theſe glaſs jars warmed than when they are cold. § XVII.

The exploſions from the large glaſſes juſt mentioned fully electriſed, as well as from ſmall ones under the ſame circumſtances, will not be conſiderable, unleſs the circuit, frequently mentioned in my writings upon this ſubject, be completed; that is, unleſs ſome matter, non-electric in a conſiderable degree, and in contact, with the coatings of the phials, is brought into contact, or nearly ſo, with ſuch non-electrics as communicate with the matter contained in the phials themſelves. When indeed the circuit can be completed, the exploſion from the large glaſſes is prodigious; the whole quantity of Electricity therein accumulated, or nearly ſo, being diſcharged in an inſtant. But the fact is otherwiſe, if the circuit is not completed, and the iron rod in the mouth of one of theſe phials is touched by a non-electric (the hand of a man, for inſtance) not in contact with the tail wire: for then there will be no exploſion, no ſhock; but the perſon, approaching his finger near the iron rod, will ſee a ſucceſſion of ſmall ſparks, more intenſely red than that large one ſeen, when the phials explode at once; and the perſon making the experiment, will feel a very pungent pain, but confined to that finger which touches the iron rod. This ſucceſſion of ſparks continues, until the Electricity accumulated in the phials is nearly exhauſted. So that the exploſion from any given quantity of Electricity, accumulated as before-mentioned, is greater or leſs in proportion to the time expended in making that exploſion: in like manner as a given quantity of grained gunpowder rammed hard in a piſtol, is almoſt inſtantaneouſly fired, and that with a great report; when the ſame quantity of gunpowder rubbed fine, and rammed hard, takes a conſiderable time in burning as a ſquib, and makes no exploſion. § XVIII.

The cauſes why the charged phial will not explode quick, without the Electricity therein deſcribing a circuit through ſubſtances non-electric in a great degree, may be very difficult to be aſſigned. It is ſufficient for us in the preſent inquiry to be aſſured of it's being a certain, an invariable law: and in order to prove, that the Electricity, upon the exploſion, paſſes with it's whole force through the circuit of non-electrics, contrary to what has been ſuggeſted, I made the following experiment. § XIX.

I procured 2 ſmall ſquare iron bars, of about 14 inches long: an inch at each end of theſe I cauſed to be bent at right angles. Theſe iron bars were ſupported in ſuch manner (by ſubſtances whether originally-electric, or not, was no ways material) that each of their ends came within about $\frac{2}{10}$ of an inch of ſome warm ſpirit of wine, or eſſence of lemons, in 4 ſpoons placed upon a table. I then ſuſpended a common coated phial filled with filings of iron to the gun-barrel, the tail wire of which reached to a table at a few feet diſtance, and was placed under a braſs weight which ſupported the handle of the firſt of the ſpoons: over this ſpoon, at the diſtance juſt mentioned, I placed one of the ſquare iron bars, and § XX.

at it's other end was placed another ſpoon: this ſecond ſpoon touched the handle of the third, which was placed under one end of the other ſquare bar, whoſe other end came near to the ſpirit in the fourth ſpoon, the handle of which lay upon a weight; and under this was placed a wire connected to the ſhort iron rod, with which the exploſion was made, when the coated phial was charged. When the phial was well charged, if the ſpirit of wine ſent forth vapours, and the ſquare iron bars were at a proper diſtance from it; upon making the exploſion at the gun-barrel the Electricity ſnapped between the ſpirit and the iron bars, and the ſpirit was ſet on fire at the ſame inſtant in all the ſpoons. It ſometimes happened, that ſome of them only were fired. If the iron bars were too near the ſpirit, it was not fired, though the circuit was compleated; becauſe then no electrical flame ſnapped between the rods and ſpirit; that effect happening only, when the parts of the non-electrics deſcribing the circuit are not in immediate contact; on the other hand, if the ſpace left between the bars and ſpirit was too great, the circuit could not be compleated, and there would be no exploſion.

§ XXI. This experiment will ſeem more ſurpriſing in the following manner. When the *apparatus* is diſpoſed of as before, the tail wire from the coated phial, before it reaches to the table, is faſtened to an iron rod ſtanding in a pail of water: another iron rod is likewiſe placed in the ſame pail of water, and a wire from this laſt reaches under the weight, which ſupports the firſt of the before-mentioned ſpoons. From beneath the weight which ſupports the handle of the fourth ſpoon, a wire reaches to an iron rod ſtanding in a ſecond pail of water, in which is placed alſo another iron rod, to which is faſtened another wire connected with the ſhort iron rod, which is employed to make the exploſion. When, with this diſpoſition of the *apparatus*, the charged phial is cauſed to explode, the ſpirit or eſſence of lemons in ſome or all of the ſpoons is ſet on fire; to accompliſh which, the Electricity muſt neceſſarily paſs through one of the pails of water, and poſſibly through both. But here it muſt be underſtood, that the pails of water ſtand upon a dry wooden floor; for if they ſtand upon one that is wet, or upon the ground, the circuit will be, for reaſons frequently mentioned in the courſe of theſe inquiries, completed between the two pails, where the non-electric matter is continuous, and be prevented from paſſing by the ſpoons where it is not ſo; and this will defeat the ſucceſs of the experiment. The number of ſpoons in the manner before-mentioned, and their diſtance from each other, may be varied as far as is thought neceſſary. The circuit may likewiſe be directed through any number of men, provided that each of them holds in one of his hands a ſpoonful of warm ſpirit, and brings one of the fingers of his other hand at the proper diſtance to the ſpirit held in the hand of the perſon next him: by theſe means the exploſion of the charged phial will ſet on fire the ſpirit in ſeveral of the ſpoons at the ſame time, provided the perſons employed hold their hands ſufficiently ſteady.

This

This experiment exhibits new and unexpected *phænomena*: in all the § XXII.
experiments to kindle inflammable ſubſtances by Electricity hitherto attempted both here and abroad, either the ſpirit or the non-electric, wherewith it was intended to be ſet on fire, were placed upon originally-electrics. But here, on the contrary, although both one and the other are placed upon non-electrics, we ſee the ſame effect produced. Nor is the electrical power leſſened, by exciting ſeveral different quantities of flame; in doing which, it paſſes ſo quick as to prevent the poſſibility, in ſeveral ſpoonfuls of ſpirit, fired by the ſame operation, of determining which of them was on fire firſt: And though we know from it's effects, that the Electricity goes through the whole circuit of non-electrics with it's whole vigour, it's progreſs is ſo quick as not to affect, by attracting or otherwiſe, light ſubſtances diſpoſed very near the non-electrics, through which it muſt neceſſarily paſs.

I would here recommend to thoſe gentlemen of the *Royal Society*, who § XXIII.
laſt ſummer meaſured the reſpective velocities of Electricity and Sound, a proceſs of this ſort to be executed at a proper time; whereby they would be able to a very great nicety to aſcertain the abſolute velocity of Electricity. For it may be contrived, that a man may be placed in the ſame room with the electrifying machine, taking hold of a wire in each of his hands: theſe wires may be ſo managed, that by means of the electrical circuit, the man holding them may be made ſenſible of the electrical commotion, even under the eye of an obſerver at the machine; though before the Electricity can arrive at the perſon holding the wires, it will be obliged to paſs through whatever large ſpace ſhall be thought convenient for the obſervation. The time then ſpent between the exploſion of the charged phial, and the perſon holding the wires feeling the electrical commotion, will give the abſolute velocity of Electricity to great exactneſs *.

As my inquiries upon the ſubject of Electricity have always tended as § XXIV.
much as poſſible to the analyſis thereof, I have often obſerved, that if, when the electrifying machine ſtands upon the floor, the globes thereof are rubbed with their cuſhions, or with hands covered with originally-electrics of a ſufficient thickneſs, and perfectly dry, no Electricity will be perceptible upon the touch of a gun-barrel ſuſpended in ſilk lines, and touching the globe in motion, or upon the touch of any other ſubſtances ſupported by electrics *per ſe*; or, in other words, there will be no accumulation of Electricity. The only originally-electrics fit for this experiment (as all unctuous ſubſtances, as wax, reſin, and ſuch-like, though electrics *per ſe*, by ſticking to the outſide of the glaſs render it unfit to excite Electricity from other bodies) are to be obtained from the animal kingdom: and of theſe only ſuch as do not partake, from their manufacture or otherwiſe, of any non-electric ſubſtances. Thoſe of this ſort,

* This has been ſince put in execution. *See the preceding* Art.

which I have tried, and always with the ſame ſucceſs, when perfectly dry, have been ſilk (woven or not), velvet, hair-cloth, woollen-cloth, and the dry ſkins of rabbets dreſſed in their fur; and the event has been the ſame, whether theſe ſubſtances have been rubbed under a greater or a leſs degree of friction: and ſcarce any Electricity has been perceptible, when thoſe parts of theſe ſubſtances, which immediately are in contact with the globes, have been rubbed over with dry chalk, a non-electric ſubſtance. But the ſucceſs is different, when theſe originally-electric ſubſtances have lain in damp places, or have been held over the ſteam of warm water; becauſe then the water imbibed by theſe ſubſtances, ſerves as a canal of communication to the Electricity between the hands or cuſhions and the globes in the ſame manner, as the air, replete with vapours in damp weather, prevents the accumulation of Electricity in any conſiderable degree, by conducting it as faſt as excited to the neareſt non-electrics. On the contrary, moſt ſubſtances of the vegetable kingdom, whoſe form makes them fit for this treatment, though made as dry as poſſible, furniſh Electricity, though in different quantities. I have tried hemp, linnen-cloth of various kinds, paper both of linnen and hemp, cotton in the wool, fuſtian, cotton-velvet, and many others of this claſs. I have covered at one time the cuſhion, with which I rubbed a globe, with eight lamina of ſheet-lead, and have excited Electricity from that metal: and however improper a deal-board may ſeem for the purpoſe of rubbing a globe, I have more than once accumulated Electricity from that, though it's ſubſtance has the appearance of being much leſs fit than every one of the originally-electrics I mentioned before.

§ XXV. To the doctrine here laid down it may be objected, that leather is an animal ſubſtance, which, though perfectly dry, excites Electricity the ſtrongeſt of all the ſubſtances hitherto diſcovered; that dry leather ought to be conſidered as an originally-electric; and therefore, according to the rule before-mentioned, ſhould not furniſh, from rubbing the globe therewith, any Electricity at all. To this I anſwer, that though the dry ſkins of animals are electrics *per ſe*, dry leather is far from being ſo; and this is owing to the vaſt quantities of reſtringent vegetable ſubſtances imbibed by the ſkins throughout their whole contexture in the operation of tanning in ſome ſpecies of leather, and of ſaline ſubſtances, ſuch as alum, in others; both which ſubſtances are non-electric, and of theſe leather very conſiderably partakes: for by theſe the hides and ſkins of animals (and any muſcle of their bodies is liable to the ſame treatment), which otherwiſe are as putreſcent as any part of their bodies ſoever, are made to laſt through many ages, and be ſubſervient to many valuable purpoſes of life. The ſame concluſion muſt be drawn concerning hats, which, tho' made of the hair of animals, furniſh Electricity, though but in a ſmall degree: and this is occaſioned by the mucilaginous and gummy ſubſtances made uſe of by the Hatmakers, to give their manufacture a ſuitable ſtiffneſs.

From

From what I have advanced § XI. XII. XIII. XIV. XV. XVII. it may poſſibly be conjectured, that the electrical *effluvia* occupy only the ſurfaces of bodies electriſed; as we there found, that a very ſmall quantity of matter, diſtributed under a very large ſurface, would occaſion a greater accumulation of Electricity, than a very much more conſiderable quantity of matter under a leſs. But that the Electricity occupies the whole maſſes of bodies electriſed, and paſſes through their conſtituent parts, is clearly demonſtrated by the following experiments. § XXVI.

When I firſt engaged in theſe inquiries, to aſſure myſelf of this fact, I enveloped an iron rod about 3 Feet in length with a mixture of wax and reſin, leaving free from this mixture only one inch at each end. This iron was warmed, when thus fitted, that the whole of it's ſurface, where it was intended, might be covered. This rod, when electriſed at one of it's ends, ſnapped as ſtrongly at the other, as though it was without the wax and reſin. This could not have happened from the Electricity's paſſing along the ſurface of the iron rod, becauſe there it was prevented by the originally-electrics, and conſequently muſt of neceſſity paſs through it. § XXVII.

A phial of water, in the experiment of *Leyden*, can be electriſed, and may be cauſed to explode, though the wire, touching the water in the phial in making that experiment, be run through a wax ſtopple, exactly fitted to the mouth of the phial. § XXVIII.

I cauſed a glaſs tube, open at each end, and about 2 feet ½ long, to be capped with braſs cemented to the ends of the tube. In the centre of each of thoſe caps was faſtened a ſlender braſs rod; and theſe were diſpoſed ſo in the tube as to come within half an inch of each other. When the tube was properly ſuſpended in ſilk lines with one of it's extremities near a glaſs globe in motion, the braſs work at both ends ſnapped equally ſtrong. As the Electricity could not paſs along the ſurface of this tube warmed and wiped clean, this effect could not have happened, unleſs the Electricity pervaded the ſubſtance of the braſs caps. Upon touching the braſs at the end of the tube moſt remote from the electrifying machine, the ſnaps from one of the braſs rods within the tube to the other were ſeen to correſpond with the ſnaps without. More experiments of this kind might be added, but theſe, I preſume, are ſufficient to ſhew, that the Electricity occupies the whole maſſes of non-electric bodies electriſed. That the Electricity paſſes through originally-electrics to a certain thickneſs I took notice of in a paper I did myſelf the honour to communicate in *Feb.* 1745. § XXIX.

I ſhall forbear at preſent to lay before you a ſeries of experiments *in vacuo*; from the compariſon of which, with the experiments in open air it appears, that our atmoſphere, when dry, is the agent, whereby, with the aſſiſtance of other electrics *per ſe*, we are enabled to accumulate Electricity in and upon non-electrics; that is, to communicate to them a greater quantity of Electricity than they naturally have: from hence alſo we ſhall ſee, that, upon the removal of the air, the Electricity pervades § XXX.

vades the *vacuum* to a conſiderable diſtance, and manifeſts it's effects upon any non-electrics, which terminate that *vacuum*: and by theſe means that originally-electric bodies, even in their moſt perfect ſtate, put on the appearance of non-electrics, by becoming the conductors of Electricity. But theſe matters may poſſibly be the ſubject of a future communication.

Part of a letter from Abbé Nollet, *of the* R. Acad. *of* Sc. *at* Paris, *and F. R. S. to* M. Folkes, *Eſq*; Pref. *concerning* Electricity. *Tranſlated from the* French, *by* T. Stack, *M. D. F. R. S.* N°. 486. p. 187 Feb. *and* Mar. 1748. *Read* Feb. 11. 1747-8.

29. For ſeveral years paſt Electricity has been my chief occupation. Laſt ſummer I read 3 Memoirs at our weekly meetings, which contained many particulars on this ſubject: but as theſe were matters of mere curioſity, and of no real uſe, they almoſt tired out my patience. I now ſend you ſome experiments, which I made during the vacation, which ſeem to promiſe at leaſt the being of ſome ſervice; but of this you will be the beſt judge. I will deſcribe them in the ſame order as I made them, and to which I was not led by mere accident. You know, that when a veſſel full of liquor, which runs out through a pipe, is electrified, the electrified jet or ſtream is thrown farther than uſual, and is diverged into ſeveral divergent rays, much in the ſame manner as the water poured out from a watering pot. Every body at firſt ſight will judge, that the ſtream is accelerated, and that the electrified veſſel will ſoon be empty. I was unwilling to rely on the firſt appearances, and therefore reſolved to aſcertain the fact, by meaſuring the time, and the quantity of the liquor running out. And in order to know if the acceleration, ſuppoſing there was any, was uniform, during the whole time of the running out, I made uſe of veſſels of different capacities, terminating in pipes of different bores, from 3 lines diameter to the ſmalleſt capillaries: and I give you in groſs the reſult of upwards of 100 experiments, as it is not ſo eaſy a taſk to draw a ſafe concluſion, as may at firſt be imagined.

1. The electrified ſtream, though it divides, and carries the liquid farther, is neither accelerated nor retarded ſenſibly, when the pipe, through which it iſſues, is not leſs than a line in diameter.
2. Under this diameter, if the tube is wide enough to let the liquid run in a continued ſtream; the Electricity accelerates it a little, but leſs than a perſon would believe, if he judged by the number of jets that are formed, and by the diſtance to which it ſhoots.
3. If the tube is a capillary one, from which the water ought naturally to flow, but only drop by drop, the electrified jet not only becomes continued and divided into ſeveral, but is alſo conſiderably accelerated; and the ſmaller the capillary tube is, the greater in proportion is this acceleration.
4. And ſo great is the effect of the electrical virtue, that it drives the liquid out of a very ſmall capillary tube, through which it had not before the force to paſs, and enables it to run out in caſes, where there would not otherwiſe have been any diſcharge.

Theſe

Theſe laſt facts have ſerved as a baſis to my inquiries. I conſidered all organized bodies as aſſemblages of capillary tubes, filled with a fluid that tends to run through them, and often to iſſue out of them. In conſequence of this idea, I imagined, that the electrical virtue might poſſibly communicate ſome motion to the ſap of vegetables, and alſo augment the inſenſible perſpiration of animals. I began, by ſome experiments, the reſult of which confirmed my notions. I electrified, for 4 or 5 hours together, fruits, green plants, and ſponges dipped in water, which I had carefully weighed; and I found, that, after this experiment, all theſe bodies were remarkably lighter than others of the ſame kind, weighed with them, both before and after the experiment, and kept in the ſame place and temper. I alſo electrified liquors of all ſorts in open veſſels; and I remarked, that the electrification augmented their evaporation, in ſome more, in others leſs, according to their different natures. Wherefore I took 2 garden-pots, filled with the ſame earth, and ſowed with the ſame ſeeds; I kept them conſtantly in the ſame place, and took the ſame care of them, except that one of the two was electrified for 15 days running, for 2 or 3, and ſometimes 4 hours a day. This pot always ſhewed it's ſeeds raiſed two or three days ſooner than the other, a greater number of ſhoots, and thoſe longer, in a given time: which makes me believe, that the electrical virtue helps to open and diſplay the germs and facilitates the growth of plants. I advance this, however only as a conjecture, which deſerves further confirmation: as the ſeaſon was already too far advanced, to allow me to make as many experiments as I could have wiſhed: but here are yet other facts, of which I have a greater certainty, and which are not leſs intereſting.

I choſe ſeveral pairs of animals of different kinds, cats, pigeons, chaffinches, ſparrows, *&c.* I put them all into ſeparate wooden cages, and then weighed them. I electrified one of each pair for 5 or 6 hours together: then I weighed them again. The cat was commonly 65 or 70 grains lighter than the other; the pigeon from 35 to 38 grains; the chaffinch and ſparrow 6 or 7 grains: and in order to have nothing to charge upon the difference that might ariſe from the temperament of the individual, I again repeated the ſame experiments, by electrifying that animal of each pair, which had not been electrified before; and notwithſtanding ſome ſmall varieties which happened, the electrified animal was conſtantly lighter than the other in proportion.

Electricity therefore increaſes the inſenſible perſpiration of animals: but in what proportion? In the *ratio* of their bulks, or in that of their ſurfaces? Neither of the one or the other, ſtrictly ſpeaking, but in a *ratio* much more approaching to the latter than to the former. So that there is no room to apprehend that a human perſon electrified would loſe near a 50th part of his weight, as it appeared to me that it happened to one ſort of bird; nor the 140th part, as to the pigeon, *&c.* All that I have been hitherto able to learn upon this head, is, that a young man or woman, from 20 to 30, being electrified during 5 hours, loſt ſeveral

ounces

ounces of their weight, more than they were wont to loſe, when they were not electrified. Theſe laſt experiments are difficult to purſue with exactneſs; becauſe the cloathing, which cannot ſtrictly be compared to the hair or feathers of animals, retains a good ſhare of the perſpired matter, and hinders one from forming a good judgment of the whole effect of the electrical virtue.

This forced electric perſpiration is very naturally accounted for, if we conſider, that the electrical matter pervades the interior parts of bodies, and that it viſibly darts from within outward: for it is very plain, that theſe electrical emanations muſt carry with them whatever they find in the ſmall veſſels, thro' which they are ſeen, or at leaſt are known, to iſſue.

This explanation will, in my opinion, occur to every one, who has ſeen the principal *phænomena* of Electricity. But how ſhall we account for all the following effects? All thoſe animals, whoſe perſpiration is increaſed upon their being electrified, all thoſe ſeeds, which ſhoot and grow quicker; all thoſe liquors, which evaporate; all that acceleration of liquids flowing thro' tubes; all thoſe particulars, I ſay, happen in the ſame manner, when, inſtead of electrifying thoſe bodies themſelves, they are only held near electrical bodies of a pretty large bulk. The notion which I have, for theſe 3 years paſt, formed of Electricity, not only affords me an explication of this, as ſimple as the former, but I venture to ſay, it was this ſame notion, that led me to the experiments, and made me even foreſee their ſucceſs.

I am not only ſatisfied of the exiſtence of an *effluent* electric matter, which all the world allows, and which ſhews itſelf 1000 ways; but many convincing reaſons have alſo aſſured me, that there is, round every electrified body, an *affluent* matter, which comes to it not only from the ambient air, but likewiſe from all the other bodies, whether ſolid or fluid, that are round about, and within a certain diſtance of it. If theſe ſurrounding bodies are of a ſimple nature, as a ſtone, a piece of iron, *&c.* nothing iſſues from them but pure electrical matter: but if they are animals, plants, or fruits, or, in a word, any organized bodies, or ſuch, in the pores of which there is any ſubſtance capable of giving way to the impulſes of the electric matter; this matter will, in iſſuing forth with the great rapidity, which it is known to have, carry along with it whatever it finds moveable enough to be diſplaced by it; and by ſo much will the weight of the body be diminiſhed; the ſame effect being here produced by the *affluent* matter, as is produced on electrified bodies by the *effluent*. If you will pleaſe to read over my eſſay, what I advance will be better underſtood. The increaſe or diminution of perſpiration is not a matter of indifference to the animal œconomy: this new method of increaſing it at will may poſſibly prove of uſe; it is neither inconvenient nor dangerous; and neither I myſelf, nor any body elſe of thoſe on whom I made my experiments, ſuffered even the leaſt inconveniency from it. One feels neither motion nor heat differing from that of the natural ſtate. Nor

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did the animals give any ſigns of uneaſineſs, while they were elctrifying: a little wearineſs, and a better appetite, were the only effects we ever perceived.

As to the facility of applying this method, 'tis well known that the electrical virtue is eaſily tranſmitted a good way off by chains, *&c.*; and one may eaſily imagine, that an eaſy chair, or even a bed, ſuſpended or ſupported in a proper manner, will put the moſt infirm perſons in a ſituation to be very commodiouſly electrified. But as there is no neceſſity to electrify them actually, it will become eaſier ſtill; for nothing more will be requiſite, than to place near them a baſket of old iron rendered electrical. The commoneſt degree of ſagacity will ſuffice to put this method in practice, whenever it is found to be uſeful.

I ſhall obſerve further, that, when I electrify an animal, I render his perſpiration more copious; and this effect is univerſal thro' every part of it. When I only place it near an electrified body, it perſpires as much. But is it's whole body equally ſenſible of this effect? I mean, what exhales in conſequence of the Electricity, does it iſſue from every part of his ſurface? I believe it does not; and that for theſe reaſons.

If it be the electrical matter of the ſkin that drives out the matter of perſpiration, by ruſhing towards the electrified body, it is natural to think, that this effect takes place only in the part out of which the electrical matter iſſues: thus the perſpiration, which is electrically forced out, ought to iſſue from thoſe parts only, which are the moſt directly applied toward the electrical body. Let us confirm this by experiments.

To an electrified body I apply a veſſel full of liquor, which iſſues drop by drop thro' ſeveral little tubes placed in different parts of it's circumference: theſe drops become continued ſtreams, and are accelerated, as if the veſſel had been electrified: but this effect is obſervable on that ſide only which faces the electrified body.

I moiſten a thick ſponge with water, and cut it in two: I weigh theſe two halves ſeparately; I join them again, and place the whole near a large electrified body, ſo as to make one half of the ſponge face the body directly, and the other the contrary way. After an electrification of 5 or 6 hours, that half, which faced the electric body, was found to be lighter than the other, *&c.*

Wherefore I think I have good grounds to believe, that a man, who preſents a ſhoulder, or one ſide of his head, to a large electrified body, perſpires more thro' that part than thro' any other. Add to this, that ſince theſe animals, which I cauſed to perſpire in this laſt manner, and which had but one ſide of their bodies expoſed to the Electricity, loſt as much of their weight, as the others which were throughly electrified; it follows, that they perſpired as plentifully thro' the expoſed part, as the others thro' the whole body. Whence we may infer, that, of the two methods, which I propoſe for augmenting inſenſible perſpiration, the latter is the moſt powerful, and moſt proper to remove obſtructions from the pores, or to ſcour them of any noxious humours which they may happen to contain.

An Eſſay *towards diſcovering the* Laws *of* Electricity, *by Mr* John Ellicott, *F. R. S. in a letter to* M. Folkes, *Eſq*; *Pr. R. S. Ibid.* p. 195. *Read* Feb. 25. 1747-8.

30. The Abbé *Nolet* *, takes notice, that he was led to his inquiries, from the acceleration which (he found from a great number of experiments) was given to the motion of fluids thro' capillary tubes, upon their being electrified. As I formerly made ſeveral experiments on this ſubject, I ſhall ſubmit it to your conſideration, whether the following obſervations on thoſe experiments may deſerve the notice of this illuſtrious *Society*. In which I have principally endeavoured to prove, that the acceleration of the motion of fluids thro' capillary tubes or ſyphons, is not barely owing to their being electrified, but that, in all caſes whatſoever, there are ſome other circumſtances neceſſary, in order to produce this effect. And I doubt not but to make this fully appear, by ſhewing, that water, being electrified, may either be made to run in a conſtant ſtream thro' a capillary tube or ſyphon, or only to drop, as if it had not been electrified at all: and likewiſe, that the water may be made to run from the ſame ſyphon in a conſtant ſtream, without being made electrical, but ceaſe to run, and only drop, the moment it becomes electrical. Under the one or other of theſe caſes, I ſhall have an opportunity of taking notice of the ſeveral varieties obſervable in theſe experiments; all of which I ſhall endeavour to account for from the following general principles.

1. That the ſeveral electrical *phænomena* are produced by means of *effluvia*.

2. That the particles compoſing theſe *effluvia* ſtrongly repel each other.

3. That the ſaid particles are ſtrongly attracted by moſt if not all other bodies whatſoever.

That the electrical *phænomena* are produced by means of *effluvia*, is in general acknowledged by all the authors who have written upon Electricity, however they may differ in opinion with regard to the bodies in which they are contained. The properties I have mentioned of theſe *effluvia* may be eaſily deduced from moſt of the treatiſes lately publiſhed on this ſubject. But to leave no room for any objection, I would beg leave to obſerve, that the exiſtence of theſe *effluvia* is proved by all thoſe experiments in which a ſtream of light is ſeen to iſſue from the electrified body; particularly thoſe ſtreams which are ſeen to iſſue in diverging rays from the end of the original conductor, when made of metal, and reduced to a point; from their being felt to ſtrike againſt the hand like a blaſt of wind, when it is brought near the ſtream, and from that offenſive ſmell which generally accompanies theſe experiments, and which is always more perceptible, the more ſtrongly the ſphere is excited.

That the particles compoſing theſe *effluvia* repel each other, appears from thoſe experiments, in which 2 bodies, how different ſoever they may be in kind, repel each other when they are ſufficiently impregnated with theſe *effluvia*. As a feather, by the excited tube; the ſeveral fibres

* See the preceding Article.

of

of the ſame feather, or two cork balls, which will be found ſtrongly to repel each other, ſo long as they retain any conſiderable quantity of theſe *effluvia*. Which property will always decreaſe, as the quantity they contain diminiſhes.

That theſe *effluvia* are ſtrongly attracted by moſt if not all other bodies, is ſo evident from almoſt all the electrical experiments, as to make any particular examples of it needleſs here; eſpecially as I ſhall have occaſion to take notice of the ſtrong attraction between the electrical *effluvia* and water, in accounting for theſe experiments. And the firſt, I would take notice of, I ſhall now proceed to ſtate as follows.

If a veſſel of water is hung to the prime conductor, having a ſyphon in it of ſo ſmall a bore that the water will be diſcharged from it only in drops, on the water's becoming electrical by means of the machine, it will immediately run in a ſtream, and continue to do ſo, till the water is all diſcharged, provided the ſphere is continued in motion. Exp. I.

That water does not run in a conſtant ſtream, but only in drops, from a ſyphon of a ſmall bore, is doubtleſs owing to the ſame cauſe by which it is ſuſtained above the level in capillary tubes. If therefore water is made to run in a ſtream barely by it's being impregnated with the electrical *effluvia*, it ſhould follow, that if one or more capillary tubes be placed in a veſſel of water, that which is ſuſtained in them would either ſink down to a level with the reſt of the water, on it's being made electrical, or at leaſt that it would not continue at the ſame height as before; but if the experiment is made, the water will be found to continue exactly at the ſame height, whether it is electrified or not.

Again, if the bare electrifying the water was the cauſe of it's running in a ſtream, it would continue to run in the ſame manner, ſo long as the water continued electrical, which it will not do: for, on ſtopping the motion of the machine, the ſtream will immediately ceaſe, and the water will only drop from the ſyphon, notwithſtanding it's being ſtrongly impregnated with the electrical *effluvia*. To account then for the water's being made to run in a ſtream in this experiment, I would obſerve, that ſo long as the machine is in motion, there is a conſtant ſucceſſion of the electric *effluvia* excited, and which viſibly run off from the end of the prime conductor in a ſtream, and as they are in like manner carried off from all bodies hung to it, thoſe *effluvia* which run off from the end of the ſyphon, being ſtrongly attracted by the water, carry ſo much of it along with them, as to make it run in a conſtant ſtream.

That the attraction between the water and electric *effluvia* is ſufficient to produce this effect, might be proved by a variety of experiments; but I ſhall only obſerve, that to this attraction it is owing that ſilk lines and glaſs tubes (which, from their imbibing ſo very ſmall a quantity of theſe *effluvia*, are generally made uſe of as ſupports in many of the electrical experiments) on only being wetted become ſtrong conductors: and that if an excited tube is held over a veſſel of water, the water is found to imbibe a very conſiderable quantity of this electric matter; and, on

the approach of a finger, or any other non-electric body, the water will be perceived to riſe towards it; and if the finger is brought ſo near the ſurface as to draw off the *effluvia*, they will carry ſeveral particles of the water along with them towards the finger, in a direction directly contrary to that of gravity; and therefore may well be ſuppoſed, when acting in the ſame direction, to have an influence ſufficient to produce a ſtream, as in the experiment.

And that this current of the electric *effluvia* is the true cauſe why the water runs in a ſtream from the end of the ſyphon, is farther evident, in that whatever tends to increaſe or diminiſh the current of the *effluvia*, produces the ſame effect upon the water. I have already obſerved, that when the *effluvia* are ſtrongly excited, they will be ſeen to paſs off from the end of the prime conductor in luminous rays; and the ſame may be obſerved with reſpect to thoſe which paſs with the water from the end of the ſyphon; but if any non-electric body is brought under the ſyphon, as, by it's attraction, the current of the *effluvia* will be increaſed, ſo theſe luminous rays will likewiſe extend to a greater length. Again, if the motion of the machine is ſtopped, the current of the electric *effluvia* will thereby be ſtopped, and the water will immediately ceaſe to run in a ſtream, notwithſtanding it's being ſtrongly impregnated with the electrical *effluvia*.

And that the water is ſtrongly impregnated will not only appear from the drops being ſooner divided into ſmall particles than they would be if they had not been electrified, but from thoſe particles being ſeparated to a greater diſtance from each other, by the repulſive property of the electric *effluvia*; and if any of the water is received into a dry glaſs veſſel, on the approach of a finger towards it's ſurface, there will be ſeen a ſpark to iſſue from it in the ſame manner as from water electrified by an excited tube; or if any non-electrical body is brought under the ſyphon, by whoſe attraction the *effluvia* may be drawn off, the water will immediately be found to accompany it in a ſtream.

Exp. II. If the veſſel of water with the ſyphon in it is ſuſpended by any non-electric body over another ſtrongly electrified, the water will immediately run from the ſyphon in a ſtream; but if ſupported by a piece of ſilk, or any other electrical body, the water will immediately ceaſe running, and only be diſcharged in drops. Theſe *phænomena* may, from what has been already ſaid under the former experiment, be eaſily accounted for.

That the water is made to run in a ſtream, is plainly owing to the mutual attraction between the electriſed body and the water; which attraction will continue, ſo long as the veſſel which contains the water, by being ſupported by a non-electric, is prevented from retaining any of the electrical *effluvia*; theſe *effluvia* being drawn off by the non-electric body, to which the veſſel is ſuſpended: but, on the contrary, when the veſſel is ſuſpended by an original-electric, the *effluvia*, not being attracted thereby, will be prevented from running off, and the water will ſoon be found

found to have imbibed a quantity of them, ſufficient, by their repelling property, to greatly weaken, or wholly to deſtroy, the former attraction, when the water will ceaſe to run in a ſtream, and only drop, as if it had not been held near any electriſed body. M. *L'Abbé Nolet* has endeavoured to account for the former part of this experiment, by ſuppoſing there is, what he calls, both an affluent and an effluent electric matter; but he takes no notice of the latter part, which is not eaſily ſolved upon his ſuppoſition. But if what I have obſerved on theſe experiments is ſatisfactory, I apprehend I have accounted for the ſeveral *phænomena* on much more ſolid principles, and that thereby any leſs certain hypotheſis is rendered uſeleſs.

I intended to have taken ſome notice of the different acceleration of the fluids thro' tubes of different bores; but as this acceleration will always vary with the current of the electrical *effluvia*, unleſs ſome method could be found out to render this current uniform throughout the whole ſeries of experiments, the proſecution of this inquiry will be rendered extremely difficult, and the reſult will at beſt be very uncertain.

When the foregoing curious letter was read at the meeting of the *Royal Society* on *Thurſday* 25 *Feb.* 1747. I acquainted the gentlemen preſent, that the ſame ingenious author had communicated to me a paper ſeveral months before, in which he had more fully and particularly delivered his thoughts on the ſurprizing *phænomena* of Electricity, and as ſeveral perſons expreſſed their deſire of ſeeing that paper, I requeſted of him either a copy, or an abſtract of the ſame; in compliance with which he, ſome days after, gave me the two following papers, containing the ſubſtance of what he had before ſhewn me; and I immediately put them into the hands of Dr *Mortimer*, one of the Secretaries of the *Society*, who read them at the two meetings of the *Society*, on the ſeveral days noted at the head of thoſe papers.

M. Folkes.

31. The great difference I obſerved in the ſentiments of thoſe ingenious gentlemen who have favoured us with their diſcoveries in Electricity, made me very deſirous of finding out ſome general principles, by means of which I might be able to form a judgment of the ſeveral hypotheſes whereby they have endeavoured to account for the principal *phænomena* obſervable in thoſe experiments. In order to this I took a general ſurvey of all the more remarkable experiments, and out of them made choice of ſuch as I judged were moſt proper for my purpoſe; and from theſe I deduced the general principles hereafter mentioned. The advantage I promiſed myſelf from this method was, that the plainer and more ſimple the experiments were, which I made choice of, the leſs liable I ſhould be to miſtake in any concluſions drawn from them; and that every freſh experiment, I could account for by them, would be an additional proof in their favour; and if my attempt in explaining the following experiments

An Eſſay *towards diſcovering the* Laws *of* Electricity, *addreſſed to the* Royal Society. *Ibid.* p. 203. *Read* March 24. 1747-8.

ments from thoſe principles ſhould prove ſatisfactory, the truth of them would be thereby ſo fully confirmed, that we might ſafely rely on them in forming a judgment of any of the diſcoveries already made; and (how general ſoever they may ſeem to be) I doubt not but they will be found of ſervice in proſecuting our future inquiries on this ſubject.

The experiments from which I deduced theſe principles were theſe which follow.

Exp. I. If a glaſs tube is rubbed by a very dry hand, and a finger is brought near any part of it, a ſpark of fire will ſeem to iſſue from it, and ſtrike againſt the finger; and if the finger is carried at a like diſtance from the end of the tube towards the hand in which it is held, a number of ſparks at a ſmall diſtance from each other will be ſeen coming from it, and a ſnapping noiſe will be heard. The tube is then ſaid to be excited, or to be electrical; and at ſome times, when it is ſtrongly excited, ſparks will iſſue from the tube in ſtreams, not only while it is rubbing, but will continue to dart out from it for a conſiderable time after the rubbing has ceaſed, and a very ſtrong offenſive ſmell will be perceived.

Exp. II. If the tube, when thus excited, is held over ſome pieces of leaf-gold, or any light bodies whatſoever, they will be attracted towards it; and the more ſtrongly the tube is excited, the greater diſtance they will be attracted from; and when they come near the tube (tho' without touching it) they will be repelled from it, and continue to be ſo, unleſs touched by ſome other body, when they will be attracted by the tube as before: but if the tube is but weakly excited, they will be attracted quite to the tube, to which they will ſometimes adhere, without being repelled from it.

Exp. III. If a ball (of cork ſuppoſe for lightneſs) be hung by a ſilk line, and the excited tube is applied to it, it will not only be attracted, but will have an attractive quality communicated to it from the tube; and if any light bodies are brought near the ball, they will be attracted by it.

Exp. IV. As the tube, when ſtrongly excited, will not only attract, but afterwards repel any light bodies brought near it, in like manner the cork-ball will be endued with the ſame property; ſo that a ſmaller ball will firſt be attracted towards it, and then repelledfrom it, the ſame as the leaf-gold in *Exp.* 2. and on touching any other body it will be again attracted; and this may be repeated ſeveral times, provided the ſmaller ball is much leſs than the larger one, tho' the effect will conſtantly grow weaker and weaker, as every time the leſſer ball is attracted, it carries off with it ſome of the electric virtue, and is likewiſe endued with the ſame properties as the larger ball.

Mr *Gray*, Mr *Dufay*, and others, have obſerved, that this electrical quality is not only to be excited in glaſs, but in moſt ſolid bodies capable of friction (metals excepted); tho' in ſome it will be ſcarcely ſenſible, and that it is found to be ſtrongeſt in wax, reſins, gums, and glaſs: and as glaſs is the eaſieſt procured of a proper form, it has generally been uſed in making theſe experiments. It has been further obſerved, that

thoſe bodies in which the electrical quality is capable of being excited, the ſtrongeſt by friction will receive the leaſt quantity of it from any other excited body, and therefore are properly made uſe of to ſupport any body deſigned to receive the electrical virtue. The truth of this will ſufficiently appear from the following experiment.

Hang up two lines, one of ſilk, and the other of thread; that of thread will be attracted by the tube at a much greater diſtance than the ſilk. Again; faſten to each ſtring a feather, or other light body; if the tube is brought to the feather faſtened to the ſilk, it will be firſt attracted, and afterwards repelled; and from the virtue communicated to it from the tube, the ſeveral fibres of the feather will ſtrongly repel each other. But when the tube is brought to the feather faſtened to the thread, the feather will be ſtrongly attracted, and continue to be ſo, without ever being repelled, the virtue paſſing off by the thread it is hung to. If a glaſs ball is hung to the ſilk line, it will be but weakly attracted by the tube; but one of cork or metal much ſtronger. Exp. V.

Let a rod of iron be ſuſtained by ſilk lines, and by means of a glaſs ſphere (which can be more regularly and conſtantly excited than a tube) be made electrical; it will be found to have all the properties of the excited tube mentioned in *Exp.* 1. A ſtream of light will come from the end of it, if it is pointed; it will attract, repel and communicate this virtue to any other non-electric body: on the approach of a non-electric, a ſpark of fire, with a ſnap attending it, will come from it; which ſpark will be greater or leſs, as the bodies approaching it have more or leſs of the electrical quality reſiding in them; and there will likewiſe be the ſame offenſive ſmell as was obſerved of the tube. Exp. VI.

From theſe experiments, which I think contain the principal *phænomena* of Electricity, may juſtly be drawn the following concluſions.

1. That theſe remarkable *phænomena* are produced by means of *effluvia*; which, in exciting the electrical body, are put into motion, and ſeparated from it.
2. That the particles compoſing theſe *effluvia* ſtrongly repel each other.
3. That there is a mutual attraction between theſe particles, and all other bodies whatſoever.

That there are *effluvia* emitted from the tube when rubbed, and which ſurround it as an atmoſphere, is evident, from that offenſive ſmell ariſing from them, from that ſenſation on the hands or face, when the tube is brought near either of them, and from thoſe ſparks of light, on a ſtill nearer approach of the finger to it.

That the particles of theſe *effluvia* repel each other, is proved by the cork-balls (*Exp.* 4). and the fibres of the feather (*Exp.* 5). repelling each other, when impregnated with them; and by the leaf-gold (in *Exp.* 2). being repelled by the tube, and not returning to it again, until, by coming near, or touching, ſome non-electric body, the *effluvia* are drawn off from it.

From

From this property it is, that theſe *effluvia* expand themſelves with ſo great a velocity whenever they are ſeparated from the electric body; and as they are likewiſe capable of being greatly condenſed, may we not from hence juſtly conclude they are elaſtic?

That there is a mutual attraction between theſe *effluvia* and moſt other bodies, appears from their collecting from the tube ſuch quantities thereof, as to endue them with the ſame properties with the tube itſelf, as was proved by the *3d*, *4th*, and *5th* but more particularly by the *6th Experiment*.

Theſe principles being admitted, it will follow, that the greater Difference there is in the quantity of electrical *effluvia* in any two bodies, the ſtronger will be their attraction. For, if the *effluvia* in each are equal, inſtead of attracting, they will repel each other; and in proportion as the quantity of electric matter is drawn from one of the bodies, will the attraction between them increaſe, and conſequently be ſtrongeſt, when any one of them has all the electrical matter drawn from it.

The particles of theſe *effluvia* are ſo exceeding ſmall, as eaſily to pervade the pores of glaſs, as is evident, in that a feather, or any light bodies incloſed in a glaſs ball hermetically ſealed, will be put in motion on the excited tube being brought near the outſide of it; and it has been generally thought that they paſs through the pores of the denſeſt bodies; and there are ſeveral experiments which render this ſuppoſition not improbable; tho' I muſt acknowledge I have not yet met with any one that I think is quite concluſive.

I ſhall now proceed to ſhew, how, from theſe principles, the *phænomena* of ſome of the more remarkable experiments of Electricity may be accounted for.

Exp. VII. Let a rod of iron, pointed at one end, be ſuſpended on ſilk lines, as in *Exp.* the *6th*, and by the ſphere be made electrical. When the rod is ſtrongly electrified, a ſtream of light in diverging rays will be ſeen to iſſue from it's point; and if any non-electric body is held a few inches from the point, the light will become viſible to a greater diſtance, and if the non-electric body is likewiſe pointed, a light will ſeem to iſſue from that in diverging rays in the ſame manner as from the electrified rod. But if the non-electrical body is flat, and held at the ſame diſtance from the rod as the pointed one was, no light will be ſeen to come from it.

The principal *phænomena* to be accounted for in this experiment are; why a light is only ſeen at the point of the rod, and not through the whole length of it? Why this light is viſible to a greater length, when the point is approached by a non-electric? And, why a light is ſeen to iſſue from the non-electric when it is pointed, and not when it is flat.

Upon which I obſerve, that whenever the ſphere is excited, the electrical *effluvia* are thereby put into motion, and made to form an atmoſphere round about it, from whence, by their repulſive property, they endeavour to expand themſelves on all ſides equally; but being ſtrongly attracted by the iron, a great part of them are drawn off along the rod, about whoſe

whoſe ſurface they likewiſe form an atmoſphere, which will be denſer or rarer, in proportion as the attraction of the rod is greater or leſs; and as the repulſive power of theſe *effluvia* will always increaſe in proportion with their denſity, it will follow, that whenever the ſphere is ſo ſtrongly excited, that the *effluvia* ſurrounding it are denſer than thoſe ſurrounding the rod, they will, by their repulſive property, drive the *effluvia* off from the end of it in a ſtream, and that with a very great velocity; as is evident, from their ſtriking againſt the hand like a blaſt of wind when brought near the end of the rod: and as this velocity is partly owing to the attraction of the rod, ſo this attraction continuing quite to the end of it, the velocity of the particles will there be greateſt; and as they approach towards the point, they will be brought nearer together, and therefore become denſer there than in any other part of the rod; and therefore if the light is owing to the denſity and velocity of the *effluvia*, it will be viſible at the point, and no-where elſe.

And that the light is thus produced, will appear, in that whatever increaſes or diminiſhes either the velocity or denſity of the particles will increaſe or diminiſh the light. For, let the motion of the wheel which turns the ſphere be ſtopped, the current of the *effluvia* will likewiſe be ſtopped, and the rays of light will no longer be ſeen to iſſue from the point, and yet the whole rod will continue to be electrical; but, on putting the ſphere again into motion, the *effluvia* will become viſible as before, and will increaſe, as the ſphere is more ſtrongly excited. Again, the light will be viſible to a greater or leſs diſtance, as the point is more or leſs acute; and as this light is always brighteſt next the point and grows fainter, as the rays diverge, this is plainly owing to the different denſity of the rays at equal diſtances; for, when the point is more acute, the rays will diverge leſs, and therefore will be denſer to a greater diſtance than when it is leſs acute.

When a non-electric, whoſe end is flat, is brought within a few inches of the point of the electrified rod, the electric ſtream will be attracted by it, and the rays made to diverge leſs than before; and the effect will be the ſame as if the point was more acute; *viz.* a continuation of the light to a greater diſtance, and which will be farther increaſed by the additional velocity the particles will acquire from the attraction of the non-electric. What will follow on a nearer approach of the non-electric to the rod, will be conſidered under the next experiment.

If the non-electric is pointed and held in the ſame place as the former, a light will appear from it the ſame as from the electrical body: for, as the points of the two rods are the parts which approach neareſt each other, the attraction there will be ſtrongeſt: the rays therefore, which diverged from the electrical rod, will be attracted by, and made to converge towards, the point of the non-electrical rod, and will conſequently be nearly of the ſame denſity at the one as the other; and the velocity being accelerated by the additional attraction, the rays will become luminous at the point of the non-electric, the ſame as at the point

of the electrified rod. If this experiment is made with a tube, inſtead of a ſphere, as it cannot be ſo uniformly excited as the ſphere, the light will iſſue from the rod in flaſhes, as the tube is more or leſs excited.

Several very ingenious gentlemen, and in particular the Abbé *Nollet*, have imagined, that the light ſeen at the point of the non-electric was produced by means of *effluvia* iſſuing from it in diverging rays towards the electrified rod, and which current of *effluvia* is therefore ſuppoſed to be the cauſe of the attractive, as a like current iſſuing at the ſame time from the electrified rod is ſuppoſed to be the cauſe of the repulſive property of Electricity.

This conjecture being directly contrary to the account I have given of this *phænomenon*, I ſhall offer ſome conſiderations in ſupport of what I have advanced, and which I think will make it appear highly improbable, that any ſuch current of *effluvia* iſſues out of the non-electric.

A continuation of the foregoing Eſſay. Ibid. p. 213. *Read* May 19. 1748.

32. In the preceding paper, I endeavoured, from the principles therein laid down, to account for ſome of the moſt remarkable *phænomena* of Electricity; and in particular for that appearance of a light iſſuing from the end of an iron rod, when pointed, and made electrical; why this light was viſible only at the point, and in no other part of the rod: why the light was viſible to a greater length when the point was approached by a non-electric: and why a light will be ſeen as iſſuing from the non-electric when it is pointed, but not when it is flat.

I ſhall now endeavour, from the ſame principles, to account for thoſe *phænomena*, which will be produced on a nearer approach of the non-electric to the electrified rod.

EXP. VIII. If the non-electric body, whether flat or pointed, is brought nearer to the end of the rod, than in the laſt experiment, there will be a ſmall ſtream of light produced, reaching quite from the electric to the non-electric body; and if brought ſtill nearer, there will iſſue a ſpark attended with a ſmall ſnapping noiſe, which will be ſucceeded by others at equal intervals; and if the non-electric is held at ſome diſtance from the ſide of the rod, the point of it will frequently appear luminous, but no part of the electrified rod will be ſo. If it is brought nearer, there will likewiſe be ſparks produced at nearly equal intervals from each other, which will ſometimes appear as iſſuing from the ſide of the electrified rod, at others, as coming from the non-electric.

If a finger is uſed as the non-electric, it will receive a ſmart ſtroke; and if ſpirit of wine, heated ſo as to emit an inflammable vapour, is made uſe of, it will be kindled by the ſpark.

Theſe *phænomena* may, on the afore-mentioned principles, be thus accounted for.

If the non-electric rod is pointed, and brought ſo near, as, by it's attraction, to prevent the rays iſſuing from the point of the electrified rod from diverging, they will be drawn off parallel to each other, and conſequently be equally luminous throughout the whole diſtance between the two rods.

If

If the non-electric be brought ſtill nearer, the attractive force will be ſo much increaſed, as not only to affect the *effluvia*, when they are driven off from the point of the electrified rod, but to be capable of drawing them off from a conſiderable part of the rod beyond the point; and that with a velocity, and in a quantity, ſufficient to occaſion both the ſpark and blow, as well as the noiſe that is heard.

The ſame is the caſe, when the non-electric rod, or a finger, is held againſt the ſide of that which is made electrical: at a greater diſtance a light will appear as iſſuing from the non-electric, the particles attracted from a large ſurface of the rod (and therefore not viſible as coming from it) being made to converge to a point, are thereby rendered luminous, and, if brought nearer, there will iſſue ſparks in the ſame manner as when held to the end: and that this is owing to the increaſe of the attractive force, ſeems plain; for it was obſerved in the laſt experiment, the attraction was capable of changing the direction of the rays at the diſtance of ſeveral inches; whereas a ſnap or ſpark is ſeldom produced, when the non-electric is held more than an inch and half diſtant. If therefore the attraction decreaſes, as the ſquares of the diſtances increaſe, as it probably does, the attractive force will be many times greater in one caſe than in the other, and if where the attractive power was weaker, as in the former experiment, there were ſo many rays of the electric matter collected, as to be ſufficient to produce a light, it cannot be thought extraordinary, when the attraction is ſo greatly increaſed on the nearer approach of the non-electric, that both the denſity and velocity of the particles ſhould be thereby increaſed, ſo as to produce heat ſufficient to fire the vapour ariſing from ſpirit of wine, or any other inflammable vapour.

And that the quantity of the electric particles is greatly increaſed, as well as their velocity, is evident from that large ſurface of the rod, which, by the approach of a finger, is in one ſpark diveſted of them; and which requiring ſome time before it can be again ſufficiently recruited, I apprehend is the reaſon of that interval between the ſparks. And here it muſt be obſerved, that the diſtance the point of the non-electric is held at from the rod, in order to produce the greateſt ſpark, muſt be varied, in proportion as the rod is electrified in a greater or leſs degree; the more ſtrongly the rod is impregnated the greater will be the diſtance; and if then the non-electric is brought nearer, the ſparks will be ſmaller, but ſucceed each other quicker; ſo that when it is brought almoſt to touch the rod, they will appear like a ſmall ſtream. The reaſon of which I take to be, that as the electric atmoſphere ſurrounding the rod is denſer nearer it than farther off, when the non-electric is brought into ſo very denſe a part of the atmoſphere, it will from thence become nearly as electrical as the rod itſelf; and therefore loſe great part of it's attractive force, and conſequently will only be able to draw off thoſe particles from the rod which are neareſt to it.

I would farther take notice, that the ſparks are always produced in the ſpace between the non-electric and the rod, and often appear as iſſuing from the non-electric. This appearance is probably owing to thoſe particles, which, by their elaſticity, are reflected back again from the non-electric towards the rod, and which, by ſtriking againſt thoſe coming from it, produce both the ſparks and noiſe that is heard; and as I have already ſhewn, that the particles often appear in luminous rays at the point of the non-electric, it thence happens, that the ſpark is frequently kindled ſo near to the non-electric, as to appear as iſſuing from it.

I obſerved, in my former paper, that ſeveral ingenious gentlemen, from this appearance of a light at the point of the non-electric, have imagined there was a current of electrical *effluvia* continually iſſuing out of it, and which, ſetting in towards the electrified rod, was the cauſe of the attraction of the Electricity: and this conjecture of theirs will ſeem to be greatly favoured by the following experiment.

If ſome of the fibres of a down-feather be faſtened to the end of a ſmall ſkewer or wire, and made electrical, they will ſtrongly repel each other, and will expand themſelves on all ſides to the greateſt diſtance poſſible from each other; but if a non-electric perſon bring the point of a pair of compaſſes, or any other ſmall-pointed body near them, they will be repelled from it, and driven up together as with a blaſt of wind, and, in the dark, a light will be ſeen as iſſuing from the point; from whence it might be concluded, that the fibres are repelled by *effluvia* iſſuing out of the point of the non-electric.

As the Abbé *Nollet* endeavours to account for the attraction of Electricity on this principle, I ſhall offer ſome conſiderations, which, notwithſtanding theſe appearances, have induced me to be of a different opinion; and they are founded on the following obſervations.

1. That however replete any bodies may be with the electric matter, none of theſe *phænomena* are ever produced, unleſs the *effluvia* are firſt excited in ſome particular body, and put in motion, either by rubbing, or ſome ſuch-like operation.
2. That the *effluvia* are not to be equally excited in all bodies, but much ſtronger in ſome than in others; and that, in particular, they are not capable of being at all excited in metals by friction.
3. The attractive and repulſive property will be ſtronger or weaker in any body, in proportion to the quantity of excited *effluvia* wherewith it is impregnated.
4. That thoſe bodies which are moſt eaſily excited by friction, will receive the leaſt quantity of the electrical *effluvia* from any other excited body; and, on the contrary, metals, or thoſe bodies in which they cannot be excited by friction, will receive the moſt.

From theſe obſervations I think it may be ſhewn, that this appearance of light is ſo far from proving that the *effluvia* come out of the non-electric,

electric, at whoſe point they are viſible; that from thence it cannot be concluded the body has any of the electrical matter reſiding in it, but is rather a proof to the contrary. For I have already ſhewn, that the ſame appearance would be produced from the ſetting in of the *effluvia* into the non-electric; and this might be confirmed, if neceſſary, by a variety of experiments. And as thoſe bodies, at whoſe point this light appears the ſtrongeſt, afford us no ſigns of their having any of the electrical *effluvia* reſiding in them, either by their attracting or repelling other bodies, or by their being capable of being excited in them by friction, as in glaſs, *&c.* nor in ſhort any ſort of evidence whatſoever, but what ariſes from this appearance; may we not expect ſome better proof of their being poſſeſſed of theſe *effluvia*, before we admit of their iſſuing out of them?

Again, it appears very extraordinary, that thoſe bodies, in which the *effluvia* cannot be excited by any other method, ſhould ſend forth ſuch ſtreams of them, only on their being brought within a few inches of the electrified rod, and that theſe ſtreams ſhould increaſe as the rod is more ſtrongly electrified; and yet that few or none of theſe ſtreams ſhould iſſue from thoſe bodies in which the *effluvia* can be excited: and if the firſt-mentioned bodies are themſelves ſtrongly impregnated, the ſtreams will diſappear, and they will be ſo far from parting with any of their *effluvia*, that, on the contrary, they will be ſtrongly repelled by the rod.

I farther apprehend, on this ſuppoſition, it will be extremely difficult, if not impoſſible, to account for the ceaſing of the ſtream from the point of the non-electric on ſtopping the machine; as likewiſe that the rod ſhould ſo ſoon be diveſted of it's *effluvia*, on ſuch a non-electric's being held near it, which it would otherwiſe retain for ſeveral hours, and which I think is a ſtrong proof of the *effluvia*'s paſſing from the rod into the non-electric. And that it certainly does ſo, may be confirmed by the perſon who holds the non-electric ſtepping upon a cake of wax, when he will ſoon become electrical, from the *effluvia* he will receive (thro' the point of the non-electric) from the rod; but ſo long as he continues to be ſo, there will not be ſeen any light to iſſue from the point; which I apprehend cannot be accounted for on any other principle, but that of the ſetting in of the *effluvia* at the point of the non-electric. And as I have already ſhewn, that all the *phænomena* are naturally to be accounted for on this principle, without being liable to any of the above-mentioned objections, I muſt remain of the opinion (till I can ſee theſe objections anſwered) that this appearance of light is no proof that the *effluvia* iſſue out of the non-electric, but of the direct contrary.

The above-mentioned objections might be brought, with equal force, againſt the fibres of the feather being repelled by *effluvia* iſſuing out of the point of the non-electric that is held near it, and in particular, that this effect would ceaſe to be produced, either when the machine was ſtopped, or the perſon who held the point became electrical. And to theſe I would

would add, that if this was really the caſe, the fibres would continue to be repelled, notwithſtanding any alteration in the ſhape of the non-electric; whereas, on the contrary, if the joint of a pair of compaſſes was held towards them, inſtead of the point, they would be ſtrongly attracted to it: and the ſame will always happen, whenever an obtuſe body is brought near them inſtead of a pointed one.

The true cauſe of this remarkable *phænomenon* I apprehend to be the different denſity of the *effluvia* at the extremities of the two bodies; for I have already ſhewn, the *effluvia* will be much denſer at the extremity of a pointed body than at an obtuſe one: and as the force by which the particles endeavour to expand themſelves, increaſes in proportion to their denſity, it follows, that the particles will be reflected back with greater violence from the pointed body than the other; and this force exceeding the attractive power of that particular part of the feather, to which it is directed, the fibres will be repelled by it; whereas the force, with which the particles endeavour to expand themſelves from the obtuſe body, being leſs than the attractive power, it follows, that the fibres of the feather will continue to be attracted by it.

Exp. IX. Take two plates of metal, very clean and dry, whoſe ſurfaces are nearly equal; hang one of them horizontally to the electrified rod, and bring under it upon the other any thin light body, as leaf-ſilver, *&c.* when the upper plate is made electrical, the ſilver will be attracted by it; and if the under plate is held at a proper diſtance, will be perfectly ſuſpended at right angles to the plates, without touching either of them; but if they are either brought nearer together, or carried farther aſunder, the leaf-ſilver will ceaſe to be ſuſpended, and will jump up and down between them. The ſame effect will be produced, if you reverſe the experiment, by electrifying the bottom plate, and ſuſpending the other over it.

If the upper plate is electrified when the leaf-ſilver is brought near, it will be attracted upwards by it, and thereby become electrical; and ſo long as it continues to be electrical, it will likewiſe be attracted downwards by the non-electrical plate. Whenever therefore this laſt attraction added to the gravity of the ſilver, which acts in the ſame direction, is equal to the contrary attraction upwards, the leaf-ſilver will, by means of theſe two oppoſite forces, be kept ſuſpended between the plates, and will continue to be ſo, as long as the equality of theſe forces is preſerved.

I have already ſhewn, that the attraction between any two bodies will always be in proportion to the different quantity of electric *effluvia* they are poſſeſſed of; the greater that difference is, the greater will be the attraction. In order therefore to obtain this equal attraction at firſt, the leaf-ſilver muſt be imbued with a greater or leſſer quantity, in proportion as the plate is more ſtrongly or weakly electrified; but always with a much leſs quantity than the plate; and likewiſe the lower plate will require to be placed at different diſtances, in proportion to the quantity of electric matter the upper plate is poſſeſſed of. As therefore the ſuſpenſion of

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the ſilver depends upon the exact proportion of attraction (ariſing from the different quantities of electric matter) in the two plates and leaf-ſilver, it follows, that whatever alters the quantity contained in any one of them would prevent the ſuſpenſion.

It is well known, that, by the attraction between any two bodies, the electric *effluvia* are continually drawn off from that which has the greateſt quantity of them, till the other being ſufficiently impregnated, the attraction ceaſes. In order therefore to preſerve theſe proportions, it is neceſſary, that, as faſt as the non-electric plate draws off any of the *effluvia* from the leaf-ſilver, it ſhould part with it again; and ſo, by continuing to be a non-electric, an equal degree of attraction be preſerved; and again, that the leaf-ſilver ſhould receive a freſh ſupply from the electrical plate, equal to what it conſtantly parts with; and the electrical plate muſt likewiſe receive an equal ſupply from the globe; and that there is ſuch a conſtant current of the electrical *effluvia*, is evident, from thoſe ſmall ſtreams of light, viſible at the two corners of the ſilver next the plates. If therefore the globe ſhould be ſtopped, or the under plate by any means become electrical, theſe proportions would be thereby deſtroyed, and the leaf-ſilver would ceaſe to be ſuſpended.

That the leaf-ſilver is always nearer to the non-electrical than to the electrified plate, is owing to it's receiving it's ſupply of *effluvia* from the atmoſphere ſurrounding the electrified plate: for as the plate is more ſtrongly electrified than the ſilver, it's atmoſphere of *effluvia* will be denſer to a greater diſtance than that ſurrounding the leaf-ſilver, and therefore can ſupply an equal quantity at a greater diſtance than what the lower plate can receive from the ſilver, whoſe atmoſphere is rarer; and therefore, as the ſilver will always be ſuſpended in that part where the two currents are equal, without which I have already ſhewn the proportion would be deſtroyed, it will conſequently be always nearer to the non-electrical than to the electrified plate. If the experiment is reverſed, by electrifying the under plate, and making the upper one the non-electric, the only difference will be, that the gravity of the ſilver muſt then be added to the attraction of the electrified plate, and will therefore cauſe the ſilver either to be nearer the non-electrical one, or the plates to be moved a little farther aſunder, or perhaps both.

33. Electricity has a power of dividing ſubtilly. It carries off with it the parts of thoſe bodies which it diſſolves, and tranſfers them to thoſe places where the electrical ſparks appear. If odorous ſubſtances are ever ſo cloſely confined in glaſs veſſels, it ſo divides them, that their exhalations penetrate the glaſs as eaſily as magnetical powers, and flow like a river thro' the atmoſphere of cylinders and chains, to which the Electricity is communicated. The electrical matter, which comes out of the other extremity of the cylinder, gives an aromatic odour to the hand that touches it. But the odour communicated does not ſtop in that part of the body on which the electrical river has flowed, but with a continued aſpiration pervades the whole human body. Not only the ſkin and garments are ſcented, but even the air breathed by the lungs, the

A new diſcovery of the uſefulneſs of Electricity *in Medicine, by* John Hen. Winkler, *Prof. at* Leipſick, *F. R. S.* N°. 486. p. 262. Feb. *and* Mar. 1748. *Dated* Mar. 12. 1748. *Read* Mar. 31. 1748.

the ſpittle, and the ſweat of the perſon affected, ſmell of the aromaticks, which are agitated by Electricity in the cloſed veſſel.

This unexpected virtue is made probable by ſeveral obſervations and experiments which were made with care and attention. In 1747 I filled a glaſs veſſel with water, and diſſolved nitre therein. This veſſel ſtood unmoved for ſeveral weeks. The water therefore became very clear, after the heavier parts of the nitre had ſubſided. At the latter end of the year I put a wire into this clear water, and joined it to a metalline tube ſuſpended on ſilken threads. I put under this tube at different times ſometimes metals, ſometimes metalline veſſels full of water, in which were glaſs ſpheres filled with metalline particles. When theſe were prepared I excited the Electricity. The electrical fire touched the bodies placed underneath. I repeated the agitation of Electricity for ſeveral days. And now, beyond my expectation, I found a great quantity of nitrous parts of various textures in the metals and veſſels, which had been touched by the electrical fire under the metalline tube. More veſſels were placed in the room where I made the experiments, and were not touched by the electrical matter from the metalline tube. In theſe there was no trace of nitre. Hence it is eaſy to conjecture, that the parts of nitre are taken out of water by Electricity, and carried into places which are touched by the electrical fire.

About the beginning of the preſent year 1748, I received a letter from *Venice*, which greatly confirms this conjecture. The author, *Joannes Daniel Gaifel*, related an affair, which ſurprized all the learned in *Venice*, *Bologna*, and other cities of *Italy*. It was accompanied with a printed epiſtle in *Italian* *, written by an eminent perſon at *Venice*, Sig. *Jo. Franc. Pivati*. In this epiſtle, the ſubject of which is Medical Electricity, he relates a ſtory of wonderful effects to Sig. *Fr. Maria Zanotti*, Secretary of the Academy of *Bologna* : and the art, by which theſe things were performed, was the invention of *Pivati*. A manifeſt example of the virtue of Electricity was ſhewn in the balſam of *Peru* ; which was ſo concealed in a glaſs cylinder, that before the application of Electricity, there could not be the leaſt ſmell of it by any means diſcovered. A man, who having a pain in his ſide, had applied hyſſop to it by the advice of a Phyſician, approached to the cylinder. The man was electrified by it, went home, fell aſleep, ſweated, and diſperſed the power of the balſam. His cloaths, bed, chamber, all ſmelt of it. When he had refreſhed himſelf by this ſleep, he combed his head ; and found the balſam to have penetrated his hair, ſo that the very comb was perfumed. The next day S. *Pivati* electrified a man in health after the ſame manner, who knew nothing of what had been done before. On his going into company about $\frac{1}{2}$ an hour afterwards, he found a gradual warmth diffuſing itſelf thro' his whole body. He grew lively and more chearful

* *Lettere ſopra L'Electricitá principalmente per quanto ſpetta alla Medicina. In* Venezia *appreſſo* Simone Occhi, *con Licenza de' ſuperiori* 1747.

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than uſual. His companions were ſurprized at an odour, and could not imagine whence it proceeded : but he himſelf perceived, that the perfume aroſe from his own body, at which he was much ſurprized ; not having the leaſt ſuſpicion that it was owing to the operation, that had been performed upon him by S. *Pivati*.

Being ſtruck with a relation ſo extraordinary, I was deſirous to try the power of Electricity on certain ſubſtances, and found the event to confirm what had been related. I put ſome beaten ſulphur into a glaſs ſphere, ſo well covered and ſtopped, that on turning it over the fire, there was not the leaſt ſmell of ſulphur perceived. When the ſphere was cooled, I electrified it. Immediately ſulphureous vapours iſſued from it, and on continuing the Electricity, filled the air, ſo as to be ſmelt at the diſtance of more than 10 feet. I called a friend well verſed in Electricity, Prof. *Haubold*, and ſeveral others, as witneſſes and judges of this fact : but they were preſently driven away by the ſtench of the ſulphur. I ſtaid a little longer my ſelf in this ſulphureous atmoſphere, and was ſo impregnated thereby, that my body, cloaths, and breath, retained the odour even the next day. On repeating the experiment in the preſence of one who was converſant in the effects of ſulphur, the ſigns of an inflamed blood were viſible in the mouth on the third day. After this I tried the effect of a more-agreeable ſmell, and filled the ſphere with cinnamon. When I had treated it as before, the ſmell of cinnamon was ſoon perceived by the company, and the whole room was in a ſhort time ſo perfumed by it, that it immediately ſaluted the noſes of all that came in ; and the odour remained on the next day. I tried the balſam of *Peru* with like ſucceſs. My above-mentioned friend, whoſe teſtimony I did not care to be without, after he had received the power of the balſam, ſmelt ſo ſtrong of it, that going abroad to ſupper, he was often aſked by the company, what perfume he had about him. The next day, when I drank tea, I found an unuſually ſweet taſte, owing to the fumes of the balſam, that ſtill remained in my mouth. In a few days, when the ſphere had loſt all the ſcent of the balſam, we let a chain out of the chamber window, and extended it thro' the open air into another room detached from the former. Here we ſuſpended the chain on ſilken lines, and gave it into the hand of a man who ſtood on an extended ſilken line, and knew nothing of our purpoſe. When the Electricity had been excited for ſome time, the man was aſked whether he ſmelt any thing ; and on ſnuffing up his noſe he ſaid he did. Being aſked again what ſmell it was, he ſaid he did not know. When the electrical commotions had been continued a quarter of an hour, that room ſmelt ſo ſtrong of it, that the man, who knew nothing of our balſam, ſaid his noſe was filled with a ſweet ſmell, like that of ſome ſort of balſam. After ſleeping in a houſe at a conſiderable diſtance from the room where the experiment was tried, he roſe very chearful in the morning, and found a more pleaſant taſte than ordinary in his tea.

When I conſider theſe things, I cannot think it improbable, that Electricity may be of ſervice in the cure of ſome diſeaſes. There are 2 great benefits to be expected from medicine; for either noxious particles being mixt with the blood or other jucies, are to be ſeparated and expelled; or ſuch as are beneficial to health are to be introduced. In both theſe caſes Electricity may be of ſervice. For as ſoon as it touches a human body, it immediately pervades it in ſuch a manner, that no place is left free from it; nor is there any thing in the body, that can be rendered volatile, that is not diſſolved, diſſipated, and carried off by it. We cannot doubt therefore, that blood, with which Electricity is communicated, is divided into more minute parts; that ſeveral of them are ſeparated from the maſs of blood, and in a ſhort time are diſperſed in the air. The tenacity of the blood does not blunt the electrical power; nor does the firmneſs of the veins hinder the avulſion; nor the fat repreſs it. The coherence of glaſs, tho' it is much firmer than the contexture of the veins, fleſh, and ſkin, cannot however hinder ſpirits and aromaticks from being diſſolved into particles, that fly off thro' the pores of the glaſs. We ſeem therefore to have reaſon enough to think, that Electricity may cauſe certain ſubſtances to fly off from the blood, and other parts of the body.

That the blood and humours of the body are greatly agitated, reſolved, and attenuated by Electricity is manifeſt: for I knew a woman, whoſe *menſes* flow immediately on her being electrified. Dr *Thebeſius* wrote to me a few days ago from *Hirſchberg* in *Sileſia*, that his being electrified was conſtantly attended by a bleeding at the noſe.

But Electricity has not only a power of ſeparating and expelling, but is alſo very efficacious in filling the blood with powers, which are contained in plants, and minerals; which is manifeſt from what I have already ſhewn with regard to ſulphur, cinnamon, and balſam of *Peru*. The electrical power of nouriſhing the blood differs from the uſual method of healing in this, that it ſupplies the blood with aliment without the help of the ſtomach, and that it enriches the vital juice with thoſe exhalations, which paſs thro' the glaſs, and excel in ſubtilty and purity. Medicines received by the mouth muſt be carried into the ſtomach, before they can be mixed with the blood, and wander thro' many and long paths, and therein be changed. But the ſpirits raiſed by benign Electricity, flow into the blood without theſe windings. Sometimes a part of the body is diſordered by it, becauſe the paſſages, thro' which the blood or other liquor ought to flow, are ſo obſtructed, that the remedies applied have no power at all of opening them, or at leaſt require a long time for it. But the part that is touched by electrical *effluvia* is ſtrongly opened and penetrated by them.

By the conjunction therefore of medicine and the electrical art, I am of opinion, that new and happy cures of diſeaſes may be performed, remarkable examples of which have been publiſhed by the learned S. *Pivati*, who made uſe of the advice of a learned and experienced Phyſician.

ſician. He reſtored the obſtructed courſe of the blood in a woman, by treating the uſual medicines in ſuch a manner, that their powers reached the body of the patient by means of Electricity, from the glaſs cylinders in which they were incloſed. S. *Pivati*'s aſſiſtance was implored by a young gentleman, who was ſo miſerably affected by an abundance of collected and corrupted humour in his foot, that it eluded all the attempts of the Phyſicians. S. *Pivati* filled a glaſs cylinder with proper materials, and having electrified it, applied to the part affected, which he cauſed to emit electrical ſparks, and continued the operation for ſome minutes. When the patient went to bed, he had a good night, and a mitigation of his pain. When he awaked in the morning, he found a ſmall red tubercle on his foot, which only itched, as if a cold humour had flowed thro' the inner part of his foot. He ſweated every night for 8 days together, and at the end of this time was perfectly well. After this S. *Donadoni*, Biſhop of *Sebenico*, came to S. *Pivati*, attended by his Phyſician and ſome friends. His Lordſhip was at that time 75 years old, and had been afflicted with pains in his hands and feet for ſeveral years. The gout had ſo affected his fingers, that he was not able to move them, and his legs, ſo that he could not bend his knees. He was ſo miſerable, that when night came, his ſervants were obliged to bring him in a chair to the bedſide, and lift him gently into it. The poor old Biſhop intreated S. *Pivati* to try the effects of Electricity on his body. He proceeded after the following manner; he filled a glaſs cylinder with diſcutient medicines, and managed it ſo that the electrical virtue might enter into the patient. He preſently felt ſome unuſual commotions in his fingers. The action of Electricity was continued for 2 minutes. His Lordſhip in leſs time than could be imagined, opened and ſhut both his hands, gave a hearty ſqueeze with his hand to one of his attendants, got up, walked, ſmote his hands together, helped himſelf to a chair and ſate down, wondering at his own ſtrength, and hardly knowing whether it was not a dream. He walked out of the chamber down ſtairs, without any aſſiſtance, and with all the alacrity of a young man. Soon after, S. *Pivati* relieved a Lady of 60 years in like manner from the gout, with which ſhe had been 6 months tormented. Her fingers were much ſwollen, and continually trembling, and one of her arms was convulſed. But after receiving the powers of Electricity for 2 minutes, the trembling of her fingers ceaſed: and the next day the ſwelling was ſo far abated, that ſhe could draw on her gloves, and make uſe of her fingers.

Theſe things are ſo manifeſt, that there ſeems to be no room to doubt of the aſſiſtance that may be given to Medicine by Electricity. In which opinion I am greatly confirmed by the concurrence of the judicious and ſkilful Phyſician S. *Morgagni*, Profeſſor of Anatomy at *Padua*, who highly approved of what S. *Pivati* had done, and encouraged him to proceed in his attemps to improve Medicine, in a manner ſo beneficial to human kind *.

* *See Art.* 38, *and* 39.

A letter from Mr Henry Baker *F. R. S. to the* Preſident, *concerning ſeveral* Medical Experiments *of* Electricity. Ibid. p. 270. *Read* March 13. 1748.

34. Though perhaps as many curious and well-contrived experiments have been made in *England* as in all the other parts of *Europe*, to diſcover the general laws and properties of Electricity; we have not hitherto attended to the effects that may be thereby produced in the bodies of living animals, any further than to aſſure ourſelves they may be killed thereby; a ſuppoſition that diſeaſes may be cured by means of this power, having met with ſo little countenance amongſt us, that very few trials have been made, to aſcertain what, in diſtempered caſes, it can or cannot perform. *Foreigners*, on the contrary, ſeem fond of believing that the ſubtile electric fluid (be it fire, æther, or whatever elſe) which can pervade all bodies, and (being accumulated) even kill an animal, in certain circumſtances, and by certain methods of application, may, poſſibly, in other circumſtances, and applied in different degrees, and by different methods, ſo operate on the fluids or ſolids, and perhaps on both, that very beneficial and ſalutary effects may reſult therefrom.

With this view the Abbé *Nollet* made ſeveral experiments on living birds, kittens, and human bodies; and if we may give credit to the accounts thereof communicated to us, he found, in every trial, that *perſpiration* was ſo conſiderably promoted thereby, as to cauſe a very ſenſible difference between the weight of ſuch animals as had been electrified, and others of the ſame kind that were treated exactly alike in every reſpect beſides: whence he naturally concludes, that, in caſes where it is neceſſary to quicken the circulation of the fluids, and throw off a greater quantity of the perſpirable matter, Electricity muſt be greatly uſeful.

The Philoſophers in *Italy* and *Germany* have applied their induſtry to diſcover by experiment, how far Electricity may, ſimply and in itſelf, be of ſervice in ſeveral diſeaſes, and likewiſe how far it may conduce towards conveying the more ſubtile and active *effluvia* of uſeful Medicines, either into the whole body, or into ſome diſtempered part. Mr *Watſon* read, laſt *Thurſday*, before the *Royal Society*, an abſtract of the preceding paper.

My ingenious friend Dr *Joſeph Bruni*, one of the principal Phyſicians at *Turin*, and F. R. S. has likewiſe ſent to me an account, lately received by him, of experiments made at *Rome*, and at *Bologna*; which I now lay before you, in order to ſhew what attempts to the ſame purpoſe have been made in different countries, and by different people. The Doctor informs me, that at *Turin* they have repeated, with great ſucceſs, the electrical experiments made in *England*, whereof I had ſent him printed accounts; that people all over *Italy* are buſily at work making electrical experiments; and that, at *Bologna*, the electrical power has been applied to the cure of diſeaſes. He then gives me a tranſcript of an account ſent him from thence in *French*, which, tranſlated, is as follows.

A man, who had been for a whole twelvemonth deaf of one ear, with a continual noiſe in it like the running of water, attended with moſt violent

violent pain whenever he lay with that ear uppermoſt, coming to Dr *Verati* for advice, the Doctor electrified him, bringing out abundance of fiery ſparks around the diſtempered ear; which, in about five minutes that the electrification was continued, became as red as if a bliſtering plaiſter had been applied to it. But the redneſs diſappeared in a few minutes after, the patient paſſed the night with leſs pain and noiſe, and was perfectly cured of his diſorder.

A footman belonging to the ſaid Doctor, being taken ſuddenly ill of a violent pain in the head, which continued many hours, he was thereupon electrified, the Doctor cauſing the ſparks of fire to iſſue from the temple wherein the pain was felt. The part appeared red, the pain abated; in 3 hours it was entirely gone, and has never returned ſince.

A woman that nurſed one of the Doctor's children, having had a moſt grievous diſorder in her eyes for ſome months, with a continual running of water from one of them, and a conſtant pain over the eye-lid, came to the Doctor for advice; who immediately electrified her, bringing out the fiery ſparks about the eye and eye-lid, whereby the eye appeared very much blood-ſhot; but that went off in 7 or 8 minutes. The woman felt leſs pain the following night, and opened her eye in the morning more eaſily, and without being obliged to wipe it, as ſhe did before: the watry humour and pain were much diminiſhed; and the Doctor hoped, that, by repeating the operation twice more, he ſhould be able to cure her quite.

Dr *Bruni* gives me next his information from *Rome*; which is, that a gentleman there covered the internal ſurface of a cylinder of glaſs (which ſome uſe inſtead of a globe) with a purgative Medicine; and that a man, electrified therewith, found on the ſpot the ſame effects as if he had ſwallowed the Medicine. He then recommends to us in *England* to try how far the electric power may be of ſervice in diſtempers.

Theſe caſes, Sir, and particularly the laſt, as it may to ſome appear extravagant and whimſical, I ſhould have been cautious of bringing before the *Royal Society*, had you not judged it proper they ſhould be added to thoſe ſimilar accounts from other places which were read to us laſt meeting. I think neither myſelf nor Dr *Bruni* anſwerable for the truth of theſe facts, as we relate no more than what we have received. In truth, all the *phænomena* in Electricity are ſo wonderful, that it is ſcarcely prudent to deny the poſſibility of any accounts concerning it, till we have made experiments carefully ourſelves. We are *very ſure* it is poſſible to render a living body replete with electrical *effluvia*, or to tranſmit and ſend ſuch *effluvia* through a living body, in a ſtream, as long as we think proper: we are *not ſure* that it is impoſſible for theſe *effluvia* to convey with them into that living body the moſt ſubtile and active *effluvia* of other ſubſtances; and if they can do ſo, the effects ſuggeſted are not wholly improbable; for ſeveral experiments have proved, that a very minute quantity of Medicine, transfuſed directly into the blood, and circulating fluids, will have the ſame effect as a large doſe

thereof

thereof taken into the ſtomach. Therefore even this laſt caſe, romantic as it may ſeem, ſhould not be abſolutely condemned without a fair trial; ſince we all, I believe, remember the time, when thoſe *phænomena* in Electricity, which are now the moſt common and familiar to us, would have been thought deſerving as little credit, as the caſe under conſideration may ſeem to do, had accounts of them been ſent us from *Rome*, *Venice*, or *Bologna*, and had we never experienced them ourſelves.

A letter from Mr Robert Roche, *to the* Preſ. *of a* Fuſtian Frock *being ſet on fire by* Electricity. N°. 487. p. 323. April, *&c.* 1748. *Dated London*, May 17, 1749. *Read* May 19. 1748.

35. I have a ſon about 16 years old, that has been for 6 or 7 years paſt troubled with ſudden fits that intirely take away his ſenſes. I got him all the helps I could, but to no purpoſe; at laſt I ſent him to St *Bartholomew*'s Hoſpital, as an out-patient; and there he was turned out as incurable. So finding his caſe deſperate, I conſidered the power of Electricity, and made a large machine for electrifying; and afterwards ſhocking him commonly twice a day, he has received ſome benefit: and laſt *Sunday*, being *May* 15, he being on the pedeſtal, and very high electrified, and having on a coarſe fuſtian working frock, the condenſing phial being on the conductor, and I, touching him to procure ſnaps as uſual, touched his right ſhoulder blade; and, to my great ſurprize, the furzy flax of the frock caught fire, with a great blaze, and burnt the whole breadth and length of the ſhoulder, the flame riſing 6 inches above the collar, and I believe would have ſet the frock on fire, had I not put it out with my hands. There was no fire in the room that day: this was about noon; neither was there any thing that could have any inflammable vapour there.

My ſurprize was the greater, becauſe all I read on that ſubject ſays nothing will burn but what ſends forth ſuch vapours.

At 9 the ſame evening I made him put on the ſame frock, and touched the left arm, where the flax had not been burnt before; and it had the ſame effect as above.

Extract of a letter from the Rev. Dr Steph. Hales, *F. R. S. to the Rev. Mr* Weſtly Hall, *concerniug ſome* Electrical *Experiments.* N°. 488. p. 409. June 1748. *Dated Teddington*, Feb. 23, 1746-7, *Read* June 30. 1748.

36. I ſaw laſt week in *London* ſome electrical experiments; in which new field of reſearches there are daily new diſcoveries made: the active electric fluid ſeems to be a great agent, in conjuction with the air, in the production of fire.

A warm thick piece of iron being ſuſpended by two ſilk lines, had a warm very thick piece of braſs laid on it, on which was placed a common hen's egg: when electrified, the flaſhes from the iron were of a bright ſilver light colour; from the braſs (eſpecially near it) the flaſhes were green; and from the egg of a yellowiſh flame colour; which ſeems to argue, that ſome particles of thoſe different bodies were carried off in the flaſhes, whence theſe different colours were exhibited.

It is ſuſpected that great degrees of electrifying have occaſioned ſome women to miſcarry; and no wonder that ſuch ſudden ſhocks ſhould do it. I wrote to Mr *King* the experimenter to electrify a frog, while the circulation of it's blood was viewed with a microſcope, to ſee if it accelerated it's motion, which he has not yet done.

He

He obſerves, that a piece of linnen that has never been waſhed, will ſoon give a good degree of Electricity to a large warm glaſs tube; *viz.* on account of the mealy paſte, which weavers dreſs the linnen with; and therefore any piece of linnen thus dreſſed will do.

37. I laid before the *Royal Society* the beginning of laſt winter an account of what had been done by ſome gentlemen, in order to aſcertain the reſpective velocities of Electricity and Sound; from which it appeared, that through a ſpace meaſuring 6732 feet, the Electricity was perceptible in a quantity of time leſs than $\frac{837}{1000}$ of a ſecond. But the gentlemen concerned were deſirous, if poſſible, of aſcertaining the abſolute velocity of Electricity at a certain diſtance; and a method had been thought of, by which this might be determined with great exactneſs.

An account of the Experiments made by ſome Gentlemen of the R. Soc. *in order to meaſure the abſolute velocity of* Electricity; *by Mr* W. Watſon, *F. R. S.* N°. 489. p. 491. Oct. &c. 1748. *Read* Oct. 27. 1748.

Accordingly, *Aug.* 5. 1748. there met at *Shooter's-Hill* for this purpoſe, the *Preſ.* of the *R. Soc.* Rev. Mr *Birch*, Rev. Dr *Bradley*, Aſtron. Royal, *James Burrow*, Eſq; Mr *Ellicot*, Mr *G. Graham*, *R. Graham*, Eſq; Rev. Mr. *Lawrie*, *Cha. Stanhope*, Eſq; and myſelf, who were of the *R. Soc.* Dr *Bevis*, and Mr *Griſchow*, jun. a Member of the *R. Acad.* of *Sc.* at *Berlin*.

It was agreed to make the electrical circuit of 2 miles; in the middle of which an obſerver was to take in each hand one of the extremities of a wire, which was a mile in length. Theſe wires were to be ſo diſpoſed, that this obſerver being placed upon the floor of the room near the electrical machine, the other obſervers might be able in the ſame view to ſee the exploſion of the charged phial, and the obſerver holding the wires; and might take notice of the time lapſed between the diſcharging the phial and the convulſive motions of the arms of the obſerver in conſequence thereof; inaſmuch as this time would ſhew the velocity of Electricity, through a ſpace equal to the length of the wire between the coated phial and this obſerver.

The electrifying machine was placed in the ſame houſe as it was laſt year. We then found ourſelves greatly embaraſſed by the wire's being conducted by the ſide of the road, which we were compelled to, on account of the ſpace neceſſary for the meaſuring of Sound: but ſo great a diſtance from the machine was not now wanted, though the circuit through the wire was intended to be at leaſt 2 miles. We had diſcovered, by our former experiments, that the only caution now neceſſary was, that the wires conducted upon dry ſticks ſhould not touch the ground, each other, or any non-electric, in a conſiderable degree, in any part of their length: if they did not touch each other, the returns of the wire, be they ever ſo frequent, imported little, as the wire had been found to conduct Electricity ſo much better than the ſticks. It was therefore thought proper to place theſe ſticks in a field 50 yards diſtant from the machine. The length of this field being eleven chains, or 726 feet, eight returns of the wire from the top to the bottom of the field, made ſomewhat more than a mile, and 16 returns more than 2 miles,

miles, the quantity of wire intended for the Electricity to paſs through to make the experiment.

We had found laſt year, that, upon diſcharging the electrified phials, if 2 obſervers made their bodies part of the circuit, one of which graſped the leaden coating of the phial in one hand, and held in his other one extremity of the conducting wire; and if the other obſerver held the other extremity of the conducting wire in one hand, and took in his other the ſhort iron rod with which the exploſion was made; upon this exploſion, I ſay, they were both ſhocked in the ſame inſtant, which was that of the exploſion of the phial. If therefore an obſerver, making his body part of the circuit, was ſhocked in the inſtant of the exploſion of the charged phial in the middle of the wire, no doubt would remain of the velocity of Electricity being inſtantaneous through the length of that whole wire. But if, on the contrary, the time between making the exploſion, and ſeeing the convulſions in the arms of the obſerver holding the conducting wires, was great enough to be meaſured, we then ſhould be able to aſcertain it's velocity to the diſtance equal to $\frac{1}{2}$ the quantity of wire employed only, let the manner of the Electricity's diſcharging itſelf be what it would.

It has been a queſtion with ſome, who have conſidered this ſubject, whether the Electricity, in compleating the circuit from the matter contained in the glaſs, paſſed either by the wire in the mouth to the coating of the glaſs, the contrary way by the coating to the wire in the mouth, or otherwiſe directed itſelf both ways at once? that the Electricity muſt paſs off one of theſe three ways, was certain, as the exploſion would not be complete, unleſs in the inſtant thereof ſome matter very non-electric communicated between the wire in the mouth, and the coating of the glaſs. Unleſs therefore the obſerver was placed in the centre of the conducting wires, it might be objected, that the experiment was not made with the exactneſs neceſſary; becauſe any perſon, who was of opinion that the Electricity directed itſelf from the mouth of the glaſs to the coating, might object, if the wire from the ſhort iron rod to the obſerver was only $\frac{1}{2}$ the length of that between the obſerver and the coating of the glaſs, that the Electricity, in the time found, paſſed only through the ſhort wire, and *vice verſa*. But if, as it was here thought proper, the obſerver was placed in the centre of the conducting wire, let the direction of the Electricity be what it would, no difference could happen in the reſult of the experiments, if made with the neceſſary caution; becauſe, if the effects in the middle and both ends of the wires were inſtantaneous, the concluſion therefrom would be very obvious. To make the experiment, the ſame phial filled with filings of iron, and coated with ſheet-lead, which was uſed laſt year, was placed in the window of the room near the machine, and was connected to the prime conductor by a piece of wire. To the coating of this phial a wire was faſtened; which, being conducted upon dry ſticks to the before-mentioned field, was carried in like manner to the bottom; and being conducted thus from the bottom of the field to the top,

top, and from the top to the bottom 7 other times, returned again into the room and was held in one hand of an obſerver near the machine. From the other hand of this obſerver, another wire, of the ſame length with the former, was conducted in the ſame manner, and returned into the room, and was faſtened to the iron rod with which the exploſion was made. The whole length of the wires, allowing 10 yards for their turns round the ſticks, amounted to 2 miles $\frac{1}{4}$ and 6 chains, or 12276 feet.

As the night preceding theſe experiments had been very rainy, care was taken, by ſilk lines properly diſpoſed, that the wires in their paſſage from the window of the houſe might not touch the wood thereof; leſt, from the moiſture of this wood, the electrical circuit might be ſhortened.

When all parts of the apparatus were properly diſpoſed, ſeveral exploſions of the charged phial were made; and it was invariably ſeen, that the obſerver holding in each hand one of the extremities of theſe wires was convulſed in both his arms in the inſtant of making the exploſions.

Inſtead of one, 4 men were then placed holding each other by the hand near the machine, the firſt of which held in his right hand one extremity of the wire, and the laſt man the other in his left. They were all ſeen convulſed in the inſtant of the expioſion. Every one who felt it, complained of the ſeverity of the ſhock.

It was then deſired, by one of the gentlemen concerned, that an exploſion ſhould be made with the obſerver holding only one of the wires. This was done accordingly; but the obſerver felt nothing, the phial diſcharging itſelf in a different manner to what it did before, on account of the circuit's not being compleated.

It was then tried, whether an obſerver would be ſhocked upon the diſcharge of the phial, if the 2 wires at their extremities ſlightly touched each other, whilſt an obſerver at the ſame time held one of theſe about a foot from their ends in each of his hands. Upon trial he felt nothing, though the phial exploded very quick, becauſe the iron wire conducted the Electricity better than the body of the obſerver.

It was then tried, whether or no, as the ground was wet, if the exploſion was made with the obſerver holding the extremity of each wire ſtanding upon the ground near the window of the houſe, any difference would ariſe in the ſucceſs of the experiment. No difference was found, the obſerver being ſhocked in the inſtant of the exploſion, as before, in both his arms, and acroſs his breaſt.

Upon theſe conſiderations we were fully ſatisfied, that through the whole length of this wire, being, as I mentioned before, 12276 feet, the velocity of Electricity was inſtantaneous.

As it was found laſt year, we obſerved again, that although the electrical commotions were very ſevere to thoſe who held the wires,

the report of the exploſion at the prime conductor was little, in compariſon of that which is heard when the circuit is ſhort. From whence it was conjectured, that the very loud report, in the experiment of *Leyden* is confined to a very ſhort circuit.

A letter from Mr William Watſon, *F. R. S. to the* R. Soc. *declaring that he as well as many others, have not been able to make* Odours *paſs thro' glaſs by means of* Electricity; *and giving a particular account of Prof.* Boſe *at* Wittemberg's *experiment of* Beatification, *or cauſing a* Glory *to appear round a man's head by* Electricity. N°. 494. p. 348. Jan. *&c.* 1750. *Read* March 1. 1749-50.

38. The inquiry into the nature and properties of Electricity has been, within theſe few years, the purſuit of many excellent and ingenious perſons; and moſt of it's extraordinary phænomena, which have been made to appear in one place, have, with proper attention to the requiſite circumſtances, appeared in others: but there have happened 2 very remarkable exceptions to this rule. The firſt is, that the odours of odoriferous ſubſtances do not only pervade, from friction, the glaſſes which contain them, but that theſe odours were carried along with the current of Electricity into ſuch non-electric bodies as were deſtined to receive them, and manifeſted themſelves in thoſe bodies by communicating to them their ſmell, and other properties. Theſe, and other things yet more extraordinary, were ſaid to have been performed by Mr *Pivati* at *Venice*, and to have been repeated by Mr *Winckler* at *Leipſick*; but though no care or expence has been ſpared, either by Abbé *Nollet* at *Paris*, Mr *Jallabert* at *Geneva*, Mr *Boſe* at *Wittemberg*, *Pere Garo* at *Turin*, and by myſelf at *London*, to bring about the ſame effects, they have hitherto been unſucceſsful. For which reaſon the truth of theſe relations has been greatly queſtioned by many; as Mr *Buccamare*, in a * treatiſe ſince publiſhed, ſays, that Mr *Pivati* confeſſed to thoſe, who addreſſed themſelves to him to ſee the experiments, that more eſpecially made with balſam of *Peru*, that it never ſucceeded but once, and that he could never repeat it. I likewiſe received yeſterday a letter from Abbé *Nollet*, who is juſt returned to *Paris* from *Turin* and *Italy*. He ſays, that his firſt care was to inquire into the truth of thoſe wonders in Electricity, of which we have heard ſo much for almoſt 3 years, and which have not ſucceeded either with himſelf or me: and he imagines the *R. Soc.* would be glad to know what they really were: for which reaſon he has juſt now ſent a Memoir to the D. of *Richmond*, in which will be ſeen the moſt circumſtantial account he has been able to procure of them at *Turin*, at *Venice*, and at *Bologna*. For his own part, he thinks that there has been a great deal of prejudice, credulity, and exaggeration; to which may be added, very little care and caution in making theſe experiments. He is now ſorry he has loſt ſo much time in attempting to make them; and thinks Mr *Winckler* has been too haſty in aſſerting, that he had repeated theſe *Italian* experiments: but why ſhould he call them *Italian*, when the nation he ſays will not allow the appellation, and except 3 perſons, he finds there no defender of what has been ſaid to be done; and adds, that there is not a Philoſopher of repute there, who believes them any more than himſelf?

This experiment then ſeems not to arrive at what we have been told; but, for further information, we muſt wait till the reception of Abbé *Nollet*'s Memoir.

* *Tentamen de vi Electric.* &c. p. 183.

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The other is, an experiment called by Prof. *Boſe* at *Wittemberg*, the *Apotheoſis* or Beatification. The making this experiment, in the manner mentioned by this gentleman in his writings, has been attained to by none. He ſays, if in electriſing you employ large globes, and place a man upon a large cake of pitch, by little and little a lambent flame ariſes from the pitch, and ſpreads itſelf around his feet; from hence by degrees it is propagated to his knees, his body, and at laſt to his head: that then by continuing the electriſation the man's head is ſurrounded by a glory, ſuch a one in ſome meaſure, as is repreſented by Painters in their ornamenting the heads of Saints: that in this ſtate if the electriſed man is touched by one that is not, the pain felt by both is very ſevere, reaches from the finger to the ſhoulder, and remains a long time. Prof. *Boſe*, in another part of his * writings, ſays, that the beatification indeed does not always ſucceed with him; that ſometimes, when other circumſtances have been very favourable, a man will be beatified by one ſphere in 2 minutes; at other times, 2 or 3 globes will not do it under 6 or 8 minutes; and even at ſome times after 20 minutes, when 5 or 6 globes were made uſe of, no light has been viſible: that under the ſame circumſtances, when one perſon was capable of being beatified, another was not. This is a ſhort account of Prof. *Boſe*'s beatification, given in his writings, in which, nevertheleſs, nothing of what he ſays eſſential to the operation is omitted.

This experiment, which was not only a deſirable thing to be ſeen, but as it ſeemed to communicate to non-electric bodies a greater quantity of Electricity than any other did, that of *Leyden* excepted, I was very deſirous of repeating: But though I omitted no trouble, and varied not the leaſt circumſtance, that could any ways conduce thereto, I was diſappointed. I tried the combined force of many globes, of different machines, in the beſt weather, and with different perſons, but no radiation in the manner before-mentioned. When I underwent this operation myſelf, ſupported by ſolid electrics *per ſe* of more than 3 feet high, and as much diſtant from the ſides of the room as poſſible to prevent the eſcaping of the electric matter, I found in myſelf, as ſeveral others did, a tingling upon the ſkin in my head, and in many parts of my body ſuch a ſenſation as would be felt from a vaſt number of inſects crawling upon our bodies at the ſame time; but I conſtantly obſerved this ſenſation to be greateſt in thoſe parts of my body which were neareſt any non-electric; but ſtill no light upon the head, though to make the eye more ready to obſerve it, this experiment was made in the dark for ſome continuance. The ſenſation of the ſnaps in this ſtate were very acute. If the hand of a by-ſtander was brought near the back of the hand of the perſon electriſed, the hairs thereupon ſent forth a great number of luminous points; and if a bunch of fine lace wire was placed upon his head, you ſaw a great deal more of the ſame appearance; but this was

* De Electricit. Comment. novus, pag. xvi.

always moſt brilliant in thoſe parts neareſt the non-electric, and ſtill more, when the non-electric was brought to a proper diſtance. But this was vaſtly ſhort of that mentioned by Mr *Boſe*, not only in it's luſtre, but as it never was general, hardly ever ſhewing itſelf in 2 parts of the body at the ſame time. This want of ſucceſs after many trials, as I by no means doubted Mr *Boſe*'s veracity, induced me to conclude, that either ſome very eſſential part of the apparatus had been ſuppreſſed by the author, or that the air of *Germany*, being upon the continent, was more dry, and more fit, than that of our iſland. It was difficult indeed to allow this laſt, as the experiment had failed here, after the long continuance of a very dry ſeaſon. This want of ſucceſs occaſioned many perſons here, well verſed in theſe matters, to conclude, that the experiments in Electricity had been carried further in *Germany* than in *England*.

However, ſome time after, I found that this experiment, in the manner before-mentioned, had been made no-where upon the Continent, *Wittemberg* excepted; and Mr *Jallabert* at *Geneva*, in his excellent * treatiſe upon Electricity, ſays, that he had likewiſe attempted it; but inſtead of beatification, he ſaw from the hair of the head of the perſon electriſed, eſpecially from the back part thereof, a great number of luminous points. Theſe, he ſays, were likewiſe obſervable upon his cloaths, which were made of a mixture of thread and cotton, more eſpecially upon their borders. When the perſon electriſed changed his ſituation upon the pitch, upon which he ſtood, the place he left appeared luminous. What this gentleman mentions beſides is very near alike to what I myſelf experienced, and what I have juſt now related. He ſays likewiſe, that he believes Mr *Boſe* had been the only perſon, who had made the beatification ſucceed.

A perſon here however, that we ſhould not even ſeem to be outdone by our neighbours, exhibited to the public the famous experiment of beatification, found out, as he ſays, by a *German* Profeſſor. Whether he knew how this experiment was ſaid to be done, or whether it was with him as with many of the diſcoverers of the Longitude, and of the quadrature of the circle, I do not determine; but thus it is, that his experiment has been exhibited as Mr *Boſe*'s for 2 or 3 years.

I am unwilling to be thought to detract from the merit of this expement, which I think a very beautiful one; but I take upon me to ſay, that it differs as eſſentially from every part of that, ſaid to have been made by Prof. *Boſe*, as any two electrical experiments ſoever.

In a letter, I wrote the beginning of laſt year, to my correſpondent Mr *Boſe*, among other things, I acquainted him of my not being able to make the beatifying experiment ſucceed; and that, as far as I had yet heard, nobody any-where had been able to do it, ſo that the power of ſeeing this extraordinary *phænomenon* was yet with himſelf alone. I

* *Experiences ſur l'Electricité*. p. 50.

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desired of him further, that if any material part of the process had been omitted in his writings, he would communicate it; for that some people here were not quite satisfied of it's having ever been made. To this he was so obliging as to send an answer nearly in the following words, "As to my beatification, I am highly obliged to you for writing to me "so freely and candidly about it; and I will discover to you my whole "artifice without any retention, though I concealed the same from all "my friends and correspondents: but, Sir, it is true, that I have em-"bellished a little my beatification by my stile and expressions; but it "is also true, that the basis of the *phenomenon* is constant. I found in "our armoury at *Leipzig*, a whole suit of armour, which was decked "with many bullions of steel; some pointed like a nail; others in form "like a wedge; others pyramidal. In the dark you well know, that "not all, but very many, of the said bullions will sparkle and glister "with tails like comets: and it is clear, that when the Electricity is "very vigorous, the helmet upon the head of the person electrised will "dart forth rays like those round the head of a canonised Saint; and this "is my beatification. You are the first, Sir, with whom I trust my my-"stery, which if you communicate to the *Royal Society*, I hope you will "take care of it's being inserted in the *Philosophical Transactions*, that "the beatification did not succeed until I communicated my method. "Many people have imagined this experiment of mine to be extrava-"gant and false. If the armour is not ornamented with steel bullions, "I believe it will not succeed. If the armour is well enriched with bul-"lions, and well polished, the comets appear twice, once in the air, "and once by reflexion from the armour. A stomacher, or a doublet, "set with nails or needles, will exhibit a small degree of beatification."

Thus far Mr *Bose*, to whom I am very much obliged, for the discovery of his process; I cannot but be sorry for his having, as he says, embellished his relation by his stile and expressions. The language of Philosophers should not be tainted with the licence of the Poets; their aim in the communicating their discoveries to the world, should be simple truth without desiring to exaggerate; as we constantly see enough to raise our admiration every step we take in investigating the operations of nature.

The electrising a man in polished armour, with several globes, must exhibit a very beautiful *phænomenon*, by the Electricity running off from several of the points; but I cannot but say, it must fall greatly short of the general radiation promised and expected from the preceding accounts.

Extract of a letter from the Abbé Nollet, *F. R. S. &c. to* Charles *Duke of* Richmond, *F. R. S. accompanying an examination of certain*

39. If your Grace shall have done me the honour to have perused the treatise I sent, intituled, *Recherches sur les Causes particulieres des Phenomenes electriques*, you will have seen my doubts touching the reality of certain facts published in *Italy*, and which have not succeeded any-where else. I will not dissemble, that the desire of knowing how far these things were true, has been one of the principal motives of my journey; and if you have been desirous to have learned the fruits of my inquiries in this respect, you need only look over the Memoir sent herewith,

which

Phænomena *in* Electricity, *publiſhed in* Italy, *by the ſame, and tranſlated from the* French, *by Mr* Watſon, *F.R S.* Ibid p. 368. *Dated Paris*, March 5. 1750. *Read* March 29 1750.

which I beg your Grace afterwards to preſent to the *R. Soc.* I well know how much that learned body intereſts itſelf in relation to the ſubject of this Memoir; and, as one of it's Members, I think it my duty to communicate the reſult of my labours. As I correſpond with Mr *Watſon*, who is well verſed in theſe matters, it may not be diſagreeable to him to put theſe papers in a condition to be laid before the *R. Soc.* I have made the whole tour of *Italy*, which has enabled me to make many obſervations relating to Natural Philoſophy. I have made ſome experiments at the *Grotto del Cani*, near *Naples*, which take off a good deal, in my opinion, of the marvellous of that famous *phænomenon*. I propoſe to myſelf the honour of tranſmitting them upon ſome future occaſion, as my letter is already too long. The eructations from *Veſuvius* were very great when I was there, and were the prelude to 3 earthquakes, which happened juſt after my departure, and which I was fortunate enough not to be witneſs of. The *Lagunes* of *Venice*, and the waters of the *Mediterranean* Sea, appear luminous every-where in ſummer, in dark nights: I have diſcovered, that this light proceeds from a very ſmall inſect, which multiplies prodigiouſly. I have heard all my life, that the water of the ocean appears ſometimes luminous: It may poſſibly proceed from the ſame cauſe, and I ſhould be very glad of a particular inquiry into this fact.

An Examination of certain Phænomena *in* Electricity, *publiſhed in* Italy.

Electricity, after having excited every-where the emulation of the ingenious, after having filled us with wonder by an infinite number of *phænomena* more ſingular and more admirable one than another, ſeems, within theſe few years, to have ſhewn itſelf equally ſurpriſing, but more uſeful, in *Italy*, than it had done in *England*, *France*, *Germany*, &c. where, for theſe 20 or 25 years, ſo great a progreſs had been made. We have heard of nothing leſs than the cure, or the almoſt ſudden relief, of diſtempers of every kind, and of purging all ſorts of perſons in a manner of all others the moſt proper to avoid the repugnance and diſguſt we naturally have to medical potions. Even that diſeaſe which we are moſt deſirous of concealing, was not by theſe means without it's remedy; the mercury being volatilized, and carried, by the electric matter, into the body of the patient, tinged his ſkin of a leaden colour, and procured him a certain cure by a copious ſalivation.

The manner in which this was done was not leſs to be wondered at than the thing itſelf; perſons afflicted with inveterate gouts, rheumatiſms, fluxions, tumours, *&c.* were relieved therefrom by being electrized for a few hours, and often a leſs time was ſufficient. Sometimes the rubbing a glaſs tube only, or at other times a glaſs tube lined with ſome medicine appropriated to the diſeaſe of the patient, was employed. Theſe medicines, to exert their operation upon the patient, paſſed thro' the glaſs; and this they were very certain of, as they ſaw them ſenſibly diminiſh in their quantity, although the glaſs containing them was ſtopped as cloſe as though ſealed hermetically. To promote ſtools, it is only neceſſary that a perſon ſhould be electrized for 6 or 8 minutes, holding

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in his hand a piece of ſcammony or gamboge; the effects were as certain, as though theſe drugs were taken internally. Beſides, if a perſon was deſirous of being perfumed from head to foot, nothing more was neceſſary than being electrized with a glaſs veſſel lined with balſam of *Peru*, *Benjamin*, or ſome ſuch drug; and from this electrization the odours were perceptible for 2 or 3 days, even ſo much as to incommode thoſe to whom theſe ſmells were diſagreeable.

Effects no leſs wonderful than theſe were publiſhed every day by writings printed and printed again *, or by particular Letters and Memoirs in manuſcript addreſſed to the ingenious all over *Europe*. They were alſo confirmed by reſpectable witneſſes, and by ſuch as were capable of impoſing them upon perſons the moſt guarded againſt the exaggerations, which never fail accompanying the relations of intereſting novelties.

The importance of the facts themſelves, and the appearance of authenticity which attended them, demanded that they ſhould be conſidered; and indeed they rouſed every-where the attention of thoſe Philoſophers, who had for any time turned their thoughts to theſe enquiries. Every one of them was deſirous of repeating what Mr *Pivati* ſaid had been done at *Venice*, Mr *Verati* at *Bologna*, and Mr *Bianchi* at *Turin*; and to begin them, as the experiment ſeemed more ſimple, they attempted at firſt the tranſmiſſion of odoriferous ſubſtances through the pores of the glaſs, the firſt foundation of *Intonacatores* †, ſo called by Mr *Pivati*; and which we ſhall, in the progreſs of this paper, call medicated glaſſes; and they endeavoured to purge perſons of all ages, and of both ſexes, by making them hold in their hand, while they were electrized, Scammony, Gamboge, Aloes, and ſuch-like. But it was very extraordinary, that of all the perſons who were engaged in theſe experiments, no one could ſucceed; and, from a ſort of ſhame, each of them expected, that ſome one would complain of his want of ſucceſs: but this was retarded, as yet, by the haſte with which Mr *Winckler*

* The firſt of theſe works is a letter of Mr *Pivati*, an Advocate at *Venice*. It was firſt printed at *Lucca* in 1747. and ſome time after reprinted at *Venice*, always with the ſame title, *Della Elettricita, Lettra del chiariſſimo Signor Franciſco Pivati*, &c. This letter was tranſlated into *French*, and printed at *Paris*. In 1748, there appeared another treatiſe, printed at *Bologna*, intituled, *Obſervationi fiſico-mediche intorno alla Electricita, da Gio. Giuſeppe Verati publ. Profeſſ. nella Univerſita, e nella Academia delle Scienze del inſtituto academico Benedettino*. In the ſame year, 1748, there was printed at *Verona* a little treatiſe concerning medical Electricity, intituled, *Lettra del Signiore Canonico Brigoli ſopra la Machina Elettrica*. Afterwards, in 1749, there was publiſhed at *Venice* a new treatiſe, conſiderably larger than the firſt, in which we find not only the author's own experiments, but alſo thoſe of Mr *Bianchi* of *Turin*, and of ſome other perſons who had taken pains with this view. This laſt work is intituled, *Refleſſioni fiſice ſopra la Medicina Elettrica*. It is principally in this volume that the facts of which we are now treating are mentioned.

† Mr *Pivati* has given this name to the manner in which he prepares hollow cylinders of glaſs in filling them, or lining them, with ſome drug, the virtue of which, he pretends, will tranſude with the electric matter.

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ſent to the R. Soc. and to ſome ingenious men in *France*, the reſult of his own experiments, which well agreed with thoſe of *Italy*, and upon the credit of which he had made them.

For my own part, I will ſpeak without any reſtraint: when I found my attempts were fruitleſs, I without any difficulty communicated it to all the Philoſophers with whom I correſponded: I deſired them to let me know if they had been more ſucceſsful than myſelf, and to acquaint me how they had proceeded, that I might conform myſelf thereby. I was much more willing to confeſs my inability, and to learn from others the method which muſt of neceſſity be obſerved, than to be deprived longer from ſeeing thoſe *phænomena* which ought to reſult therefrom. Inſtead of inſtructions, which might conduct me to the ſucceſs I wanted, I received nothing but ſuch confeſſions as mine: from theſe I ſaw, that all methods had been tried; and that nothing remained to be done, but either to believe every thing upon the faith of others, or to doubt, without hopes of being better informed. The firſt of theſe two caſes was directly oppoſite to the law I had determined to abide by, when I firſt engaged in the ſtudy of Experimental Philoſophy; and the other was putting a great violence upon myſelf. But from this moment I formed my project of travelling; and, among the different motives which made me undertake the journey to *Italy*, I muſt confeſs one of the moſt preſſing was, the deſire of ſeeing ſucceed, in the hands of thoſe who had ſaid they had, thoſe *phænomena* in Electricity, towards the verification of which I had made ſo many fruitleſs efforts. I formed to myſelf a great pleaſure in ſeeing balſam of *Peru*, *Benjamin*, *Camphire*, *Cinnamon*, *&c.* pervade an electrized glaſs, which I had taken care to ſtop myſelf; to ſee people purged by the palm of their hands; to ſee an old gouty Man, as the Biſhop of *Sebenico* *, clap his hands together, ſtrike the ground with his feet, and walk freely, after an Electrization of 2 minutes: but what ſtill more piqued my curioſity was, to learn, if poſſible, why the *Italian* Electricity ſhould enjoy theſe prerogatives, to the excluſion of that of every other country. If this ſingularity was as real as it appeared to be, it was a new wonder more difficult to be explained than any other; and of which I propoſed to ſtudy attentively the circumſtances, to endeavour to find out the cauſe.

If I have had the trouble of paſſing the *Alps*, to ſearch out the truth, it is neither to conceal it, nor yet leſs to diſfigure it with falſhood; and I will relate, with a liberty truly philoſophical, all that I have heard, and all that I have ſeen: but if, in doing this, I ſhall find myſelf obliged to contradict ſome of the facts publiſhed by ſome perſons known in the republic of letters, I proteſt that it is without prejudice, on my part, to the advantageous idea I may have of their candour or abilities; and I ſincerely wiſh, that the reader may conſider them in the ſame manner. If he is judicious, he will willingly concur with me; becauſe, in an enquiry ſo

* See Mr *Pivati*'s Letter, printed at *Lucca*, p. 37.

obſcure

obſcure as this of which we are now treating, an ingenious man, with a very juſt intention, may take what is falſe for that which is true.

I arrived at *Turin* about the beginning of *May* 1749. and one of my firſt cares was, to viſit Mr *Bianchi*, a celebrated Anatomiſt, and the firſt author of purging by Electricity. I related to him all that he had written to me upon this ſubject; and I begged of him, that all the experiments, which had neither ſucceeded with me, nor a great many others, might be repeated between us, and under his direction. His complaiſance eaſily granted what I deſired: we ſet about it; and Pere *Garo*, a *Minim*, and Profeſſor of Philoſophy in the Univerſity, cauſed to be carried to the place where we determined to make our experiments his electrifying machine; which is exactly like that which I have deſcribed in my Eſſay, *Page* 19. *Fig.* 2.

May, 21. about 4 in the afternoon, the weather cool, but uncertain, Mr *Bianchi* having procured a lump of ſcammony, and another of gamboge, each of which was about the ſize of an hen's egg; I took the former in my right-hand, and having applied my left near the ſurface of the glaſs globe, and ſtanding upon a cake of reſin, I was electrized 15 minutes without interruption. This day the Electricity was indifferently ſtrong. *The Experiments of the firſt day.*

After me, a young man, aged 22, and of a pale complexion, was electrized; whom, a few days before, I had taken into my ſervice.

They then electrized a young woman of about 16 or 17, of a weakly conſtitution; but who, at that time, was tolerably well.

After that M. *Beccari*, Prof. Philoſ. in the Univerſity, aged about 35, of a dry habit, was electrized.

They then electrized a ſervant belonging to the houſe where they made the experiments, aged about 24, who did not appear to be indiſpoſed.

They alſo made the ſame experiment upon another ſervant, a ſtrong man of 40, or thereabouts; and each of theſe perſons was electrized the ſame time as I had been; that is, fifteen minutes ſucceſſively.

I did not perceive in myſelf any effect, which I could attribute to the Electricity; no extraordinary motion or pain in my bowels; and it was the ſame with M. *Beccari*, with the ſervant aged 40, and with the young woman.

But the young man of 22, being interrogated after the others, ſaid, that he had had in the night 2 ſtools, and ſome complaints of the colic. The ſervant of the houſe, who was aſked the ſame queſtions, declared, that he had had a very large ſtool, as though he had taken a purge.

Theſe two laſt depoſitions were, as the others, taken upon the ſpot; and I began to conſider them as important, when I learned, from the confeſſion of the laſt, that he had taken, for ſome days, a decoction of wild ſuccory, for an indiſpoſition which he had not ſpoke of till then. The young man who ſaid he had had 2 ſtools, rendered his teſtimony more

than ſuſpicious, by certain ſingularities * which he was deſirous of adding ſome hours after; and ſince that time he has conducted himſelf in ſuch a manner, as to prevent my having any confidence upon what he ſaid.

What I have juſt now mentioned to have found in theſe two ſervants, one of which kept me ignorant ſome time of his having taken broth with ſuccory; and the other having teſtified ſuch a love for the marvellous, that one ought in prudence to ſuſpect every thing he ſaid; this, I ſay, made me very delicate in the choice of the perſons who I was deſirous ſhould be admitted to our experiments. I declared that I was not willing to receive thereto either children, ſervants, or people of the lower claſs; but only that reaſonable people ſhould be admitted, and of an age ſufficient to leave nothing to be feared of the truth of what they might depoſe.

The ſecond Experiment. The day after we had made our firſt ſet of experiments I was again electrized 15 minutes ſucceſſively, as I had been the day before, holding in my hand a large piece of ſcammony; and after me there went ſucceſſively through the ſame trial Dr *Scherra* a Phyſician, Mr *Verne* Demonſtrator of Anatomy, the Marquis of *Sirié*, the Abbé *Porta* a Profeſſor in the Univerſity, the Preceptor to the children of the Marquis *D'Ormea*, and the Preceptor to the young Meſſieurs *D'Oſa*. This day the Electricity was indifferently ſtrong.

Of all theſe perſons who were electrized, not one felt any pains in his belly, no one had any evacuation which could be attributted to the electrical power; but to ſay ſcrupulouſly all that came to my knowledge, after ſeveral queſtions, the Preceptor to young Meſſieurs *D'Ormea* declared, that he had parted with more wind than he had uſually done, and he believed alſo with more urine. Thus of 7 perſons there was but one who ſuſpected the operation of Electricity to have had any ſenſible effect upon him, and this ſuſpicion, as we ſee, was a very ſlight one.

The third Experiment. *May*, 23 the Electricity being more ſtrong than the preceding days, we choſe a piece of new † ſcammony, very ſtrong in it's flavour, and which weighed ℥iv. the Marquis *D'Ormea*, Dr *Allion*, a Phyſician, the 2 above-mentioned Preceptors, Pere *Garo*, Count *Ferrero*, and myſelf, held, one after the other, this piece of ſcammony, and each was electrized 15 minutes, as had been done in the former experiments.

Two days paſſed, and abſolutely none of theſe perſons perceived any thing, that could be attributed to the Electricity.

* This young man made himſelf very happy in relating to every body, that he had been electrized; and that he had been purged thereby, as though he had taken phyſic: and added, that, an hour after his electrization, having had the curioſity of viſiting his wife, to ſee what would be the conſequence, he had communicated this Electricity to her, and that ſhe had been purged as well as himſelf.

† M. *Bianchi* ſuſpected, that the drugs we had made uſe of in our firſt experiments had loſt their moſt ſubtile parts, only capable, as he ſaid, of being introduced with the electric matter.

The

The ſame day we endeavoured to repeat an experiment, which M. *Bianchi* had writ me word of ſome months before, and which had not ſucceeded with me at *Paris*. This experiment was the tranſmiſſion of odours along a chain, or an iron bar electrized. One of us prepared and applied a little piece of linen, covered with balſam of *Peru*, upon the iron bar, which received the Electricity from the globe: we faſtened to this rod the end of an iron chain, which was electrized by communication; and we expected, that the odour of the balſam would be tranſmitted to the other end of the chain, to which was hung a ball of metal. But this was expected in vain; nobody could perceive the ſlighteſt ſign of this tranſmiſſion. *The fourth Experiment.*

M. *Bianchi*, ſeeing, as I did, that the reſult of all theſe experiments did not agree with thoſe, which he had believed to have taken place before, told me, that this difference might ariſe from our having employed an Electricity too ſtrong: becauſe that which he had experienced with ſucceſs had always appeared more weak. I ſubmitted to this reaſon, having no other to give him more plauſible; and to bring the whole operation, as near as might be, to it's firſt circumſtances, we met together, to the number of 14, at M. *Bianchi*'s, where we were expected; and we were electrized, one after the other, by him, as long a time as he judged proper, ſometimes with ſcammony, and ſometimes with gamboge, which he himſelf had choſen.

The machine uſed this day was the ſame, with which M. *Bianchi* had always made his own experiments. It conſiſted of an hollow glaſs cylinder, 3 inches in diameter, and ſomething more than $\frac{1}{2}$ a foot in length *, mounted between two ſupporters upon a board, which was faſtened to a table with ſcrews. This cylindrical veſſel was turned round, without any other intermediate apparatus, by an handle, which was at leaſt 4 inches in it's radius; ſo that the hand, by which this machine was turned, revolved with greater velocity than the ſurface of the glaſs cylinder, which was thereby put in motion.

This machine had this convenience, that one perſon only might turn the handle with one hand, and rub the ſurface of the glaſs veſſel with his other: but there is no difficulty in comprehending, that the Electricity could not but be always very weak with ſuch a cylinder, and from ſuch friction; ſo that, in the experiments of this day, we were ſcarce able to perceive any ſnaps, in touching the iron chain, by which the Electricity was communicated, or from the perſom electrized; but this was preciſely what was deſired.

Theſe experiments were made on *Thurſday*, *May* 29, between 4 and 6 in the afternoon, in a very hot and ſerene day: on *Sunday* evening, all the perſons who had been electrized, being interrogated, anſwered without heſitation, and in a manner abſolute in all reſpects, that they had perceived nothing which could be attributed to theſe experiments:

* The author here means the *French* foot.

theſe perſons were the Marquis *de Siria*, Count *Ferrero*, the Marquis *D'Ormea*, M. *de Tignola*, an officer of artillery, Pere *Beccari*, Pere *Garo*, Dr *Allion*, M. *Verne*, Dr *Scherra*, the Abbé *Porta*, the 2 Preceptors, the young woman, whom I mentioned before, and myſelf.

The night following, that is to ſay, that between *Sunday* and *Monday*, I was troubled with an indigeſtion, and felt pains of the colic; but I attributed them much leſs to the being electrized the preceding *Thurſday*, than to ſome roots I had eaten the day before at dinner, and to a very large glaſs of iced lemonade, which I had drank ſome time after, and contrary to my uſual cuſtom. Nevertheleſs, as ſome perſons were deſirous of ſaying, that the electrizing had purged me, and that I had not the candour to ſpeak of it, I thought it my duty to add here for my juſtification, that during my whole life I have had a weak ſtomach; that I could never take ice, nor liquors very cold, without a good deal of circumſpection, and always at the hazard of being incommoded therefrom; and that theſe roots, which are called *Ravanelle* in *Piedmont*, notwithſtanding my attention to eat ſparingly of them, had oftentimes diſturbed my digeſtion, during my ſtay there, and at times when I had no concern in electrical experiments. Beſides the not being incommoded for 3 days, and more, was ſufficient to prevent my attributing what happened to me to the electrical power.

The extreme circumſpection, with which I was deſirous of chooſing the perſons for all our experiments; the difficulty of procuring and moving ſuch ſick people, who were in a condition and diſpoſition to leave nothing to be feared on their parts from their prejudice, and their heated imagination; that of reconciling my time with that, which a Phyſician of great practice could grant me; theſe obſtacles, I ſay, prevented my attempting with M. *Bianchi* ſuch cures, as he believed to have been brought about by means of the electric virtue, either by it's own action, or by joining thereto Medicines appropriated to the condition of the ſick, and contained in glaſs veſſels electrized by friction. But I teſtified a great deſire of ſeeing thoſe perſons who had been cured, or conſiderably relieved, by this method before this time. I aſked, for this purpoſe, the gentlemen of the Profeſſion, who had been witneſſes of the experiments, and who were yet in a condition of ſeeing every day ſome of the perſons, cited in a manuſcript which I had of M. *Bianchi*'s; and of whom the exact hiſtory is mentioned in the 9th Chapter of a Treatiſe of M. *Pivati* *: I went myſelf to the Shoemaker, in whoſe ſhop the young man of 21 years of age worked, mentioned in the 110th page of the above treatiſe, and in page 419 of my *Recherches* †. The obligation of ſaying the truth, to which Philoſophers ought to ſacrifice every human

* *Reffleſſioni fiſiché ſopra la Medicina elettrica*, p. 149.

† They have tranſlated *Hatter*, inſtead of *Shoemaker*, in taking the *Italian* word *Calcetario* for that of *Calzolaio*, which was ſufficiently legible in the manuſcript.

regard,

regard, will not permit me to diſſemble that my inquiries made with all poſſible diligence, and without any other intereſt than that of knowing the truth, have made me ſee ſufficiently clear, that theſe facts have been greatly exaggerated. I am willing to believe, that it is the fault of the ſick, who, being prejudiced perhaps by too great hope, and poſſeſſed by a kind of enthuſiaſm, have ſaid themſelves, and made others believe, more than really was the caſe. One might have examples enough to cite of ſuch illuſions; but be that as it will, I cannot help believing, that a great part of the electrical cures of *Turin* have been no other than temporary ſhadows, which have been taken with a little too much precipitation, or complaiſance, for realities.

I carried with me to *Venice* the ſame curioſity, and the ſame deſire of being inſtructed, upon the ſubject of the tranſmiſſion of odours from medicated tubes, and of the cures, or of being relieved from diſorders almoſt ſuddenly, by the electrical power. One of my firſt cares was, to find out ſome friends or acquaintance of M. *Pivati* to acquaint him of my arrival, and to obtain of him the favour of being admitted into his laboratory; and that he would have the complaiſance to ſatisfy my great deſire of ſeeing him cauſe odours to pervade the ſides of a well ſtopped glaſs, or by electrifying to diminiſh ſenſibly any ſubſtance therein contained. M. *Angelo Quirini*, a *Venetian* gentleman, a great friend to the Sciences, and one always ready to aſſiſt thoſe who apply themſelves thereto, did me this ſervice among many others, for which I am indebted to his friendſhip and politeneſs. He accordingly acquainted M. *Pivati*; and on *Auguſt* 1, 1749. we waited upon him, and found there a large company, among which were ſeveral perſons of diſtinction: among others were M. *Antony Moſſinigo*, heretofore Embaſſador in *France*, Abbé *Horter*, &c. At the ſight of this great aſſembly I believed (and I had ſome reaſons for believing it) that my curioſity had been * ſuſpected of diſbelief, and of an obſtinacy to doubt; this company therefore was called together to be an evidence of my conviction. I would have been willing to have bought at this price the pleaſure of ſeeing a *phænomenon*, for the verifying of which I had taken ſo much fruitleſs trouble. The manner of making it ſucceed had been without doubt ſome novelty to me, as curious itſelf as the effect which ſhould have reſulted therefrom. But how great were my ſurprize, and my regret, when M. *Pivati* declared frankly to me, in the preſence of this whole company, that he would not attempt to ſhew me the tranſmiſſion of odours; that that *phænomenon* had not ſucceeded but once or twice, as he had ſaid in his firſt letter printed at *Lucca*, although ſince that he had made many attempts to repeat that experiment, with the ſame as well as with other

* I had been acquainted, that my arrival at *Venice* had been notified by letters from *Turin*, which had deſcribed me as a man ſo prejudiced againſt facts, that the ſtrongeſt could not make me believe. In this they did me great injuſtice; unleſs they took for incredulity on my part the precaution I took, againſt illuſion, and falſe appearances.

glaſſes;

glaſſes; that this cylinder had been ſince broken; and that he had not ſo much as kept the fragments of it!

But at leaſt, I told him, I might ſee him uſe one of his medicated tubes, and weigh it before and after electrifying, to ſee, with him, the included matter diminiſh ſenſibly. This fact, he told me, had ſucceeded with him a great many times; but that now there was too much company; that it was too hot, and, in conſequence, that the Electricity would be too weak for it. He might perhaps be in the right: but why did he call together ſo numerous a company?

I then aſked him concerning the cures related in his works, and eſpecially concerning that of the Biſhop of *Sebenico* *. He avowed to me (and in part I knew it already), that the Prelate was not cured; and that, ſince the electrification, he had been as he was before.

I took my leave of M. *Pivati*, and acquainted him, that I propoſed to continue about a week in *Venice*; and I very earneſtly begged of him to collect together his beſt veſſels, to renew the ſubſtances therein, and to let me know, that, if they ſucceded, I might wait upon him, that I might be able to publiſh them as an eye-witneſs; and I ſpoke to him with a good deal of ſincerity. M. *Pivati* promiſed me he would; but, as I heard nothing from him afterwards, I preſume that he had nothing to ſhew me.

Dr *Sommis*, of the faculty of Phyſic at *Turin*, being at *Venice* a little while after me, had alſo the curioſity of viſiting M. *Pivati* in *Auguſt* laſt, and to ſee, under his management, the effects attributed to the medicated tubes. The following is the letter † he wrote me upon this ſubject, *Nov.* 15. 1749.

" Here is, kind Sir, in a few words, the account of what I obſerved " in *Venice*, at Signor *Pivati*'s, during the month of *Auguſt* laſt. The " 25th day, after dinner, he electrified me, making uſe of a tube of " the length of about 5 inches, and a little more than 2 in diameter, " cauſing me to hold in my hand an ounce of ſcammony. There were " preſent at this experiment his Excellency the Abbate *Barbarigo*, the " Fathers *Bertinelli* and *Magrini*, Jeſuits, Dr *Grampini*, and ſeveral " other perſons. I found not any change in myſelf either that evening " or the following day. The 29th of the ſame month I returned again " to Signor *Pivati*, where I found a gentleman of the Houſe of *Soranzo*, " 2 *Spaniſh* officers, 2 other *Venetian* gentlemen, a Phyſician, and ſome " others; and he cauſed a tube to be lined [or plaſtered within] for the " experiment, which was repreſented by him in a dangerous light; but " which was not ſuch however as to hinder my telling him, that I deſired " that the experiment might be made upon myſelf. He began then to " electrize me at 35 minutes after 5 in the afternoon, and made an end, " becauſe the line of the wheel tangled, at 57 minutes after 5. Having

* See page 374, *ſuprà*.

† This letter is tranſlated from the *Italian*.

" then

" then new-fitted the line, he began again at 5 minutes after 6, and " continued till 14 minutes after, making again this time the ſparks to " iſſue continually from my forehead. This tube was nearly of the ſame " length and ſize as the former. The experiment being over, I then " prayed him to tell with what materials the tube had been lined; and " ſo much the rather, as he had let fall in diſcourſe with the *Spaniſh* " gentlemen, that they might have ſeen me ſleep; and he anſwered me, " that the tube was lined with ℥ij ℥vi of flowers of *Benjamin*, and two " drams of *Opium*. Having heard him mention the *Opium*, I prayed " him to take the trouble of making another experiment, his Excellency " S. Abbate *Pietro Barbarigo*, and myſelf alſo, having with us ℥jſs of " *Opium*; and he complied with my deſire. He therefore electrified " his Excellency, making him hold the *Opium*, that is to ſay, the " quantity of ℥ſs of it, in his hand, and the ſparks iſſuing from his " hand for half an hour together, beginning at 18 minutes, and finiſh- " ing at 48 minutes, after ſix. In this ſecond experiment he made uſe " of the ſame tube which he had uſed the *Monday* before, the 25th of " the ſame month: but neither his Excellency nor I ſlept more than " ordinary. Theſe are the experiments which I made at *Venice* with " Signor *Pivati*. In my return home, paſſing thro' *Placentia*, I here " ſpoke with Dr *Cornelius*, who aſſured me, in preſence of Dr *Riviera*, " that he alſo had tried a great many times to purge others by electri- " fying them; but that it had never ſucceeded with him but once, which " was upon a maid-ſervant, to whom he had given ſome rhubarb to hold " in her hand. Notwithſtanding which, finding it never to have pro- " duced the ſame effect in any other perſon, it rather ſeemed to him, " that ſome other cauſe might have occaſioned what happened to his " maid."

We ſee then from this letter, and from the account I have before given of my viſit to M. *Pivati*, that I have not been able to verify at *Venice* any of thoſe facts, in which my curioſity was intereſted. I might add alſo (and I ought, without doubt, ſince I have engaged to mention exactly every thing I have been able to find out from my enquiries upon this ſubject), that of all the perſons of the country, who have been with M. *Pivati*, to be certified of the truth of his experiment from ocular demonſtration, and whom I was able to interrogate, I found but one who atteſted them, as having ſeen them: this was a Phyſician, a friend of M. *Pivati*, whom I found at his houſe, and who had, as he ſaid, almoſt always aſſiſted him in his experiments.

From *Venice* I went to *Bologna*, where I became acquainted with Dr *Verati*, a Member of the Academy *De l'Inſtitut*. From the frequent converſations I had with him, I was convinced that he was a learned, wiſe, and candid man, as I had heard before. I laid before him, without ſcruple, the doubts I had, touching the tranſmiſſion of odours, the effects of lined tubes, purging by electrizing, as well as the almoſt ſudden cures.

Dr

Dr *Verati* anſwered me, firſt, "That he had made many experi-
"ments, from the reſult of which it ſeemed to him, that the odour of
"balſam of *Peru* pervaded from within to the outſide of a glaſs cy-
"linder which he ſhewed me." This tube however, at this time, would not convince us of it's having been done, although we rubbed it with our hands very ſtrongly. But upon my repreſenting to him, that as the glaſs was cloſed only with wooden ſtoppers, which could be taken off at pleaſure, to put in or take out the odoriferous ſubſtances, it might happen, that the odours, agitated by the heat might have paſſed through the pores of the wood; he anſwered me, "That this was poſſible; and
"although appearances had inclined him to believe the tranſmiſſion of
"theſe odours through the pores of the glaſs, he had nevertheleſs ſuſ-
"pended his judgment upon this effect, as well as upon that from lined
"tubes, until new proofs, made with more precaution, ſhould have en-
"tirely diſſipated his doubts. Secondly, with regard to the purging by
"Electricity, he had in his houſe a man and maid-ſervant, who had
"been purged in this manner: that at leaſt theſe two perſons had felt
"the ſame effects as though they had taken Phyſic; after having been
"electrized in M. *Bianchi*'s manner: that this effect having no other
"apparent cauſe than the preceding electrization, *the great number of*
"*facts of this kind, which had manifeſted themſelves at* Turin, had de-
"termined him to believe, that what happened to his two ſervants was
"the natural conſequence of this electrization: that, with regard to the
"reſt, he propoſed to try the experiment again upon a ſufficient num-
"ber of perſons of another ſort; and if this method of purging was not
"conſtant, according to the idea he had had thereof he would correct,
"with great freedom, what he had publiſhed thereupon in his works,
"printed in 1748."

Thirdly, Dr *Verati* aſſured me, "That the ten cures, related in his
"work juſt mentioned, were exactly made in the manner they are de-
"ſcribed:" and they are related with a good deal of prudence, and with a ſimplicity which characterizes the truth. The fifth of them was told and certified to me by the perſon himſelf, one day when I viſited Father *Trombelli*, Abbot of the houſe in which he lives. Theſe cures are not ſuch as give me difficulty to believe them: we ſee, at leaſt, that they are made with ſpeed: we ſee that the diſorder, if I may be allowed the expreſſion, defends itſelf againſt the remedy, and does not give place but by little and little; and that nature makes no ſudden tranſition from one ſtate to the other abſolutely different, by the means of an Electricity ſcarce ſenſible. Theſe cures, I ſay, give me no trouble to believe them; becauſe it appears to me natural enough, and I have ſaid it a great while ago *, that a fluid, active as the electric matter, and which paſſes into our bodies with ſo much eaſe, may produce therein, in time, alterations either ſalutary or pernicious.

* In a diſcourſe read to the *Royal Academy of Sciences* juſt after *Eaſter*, 1746.

I learned

I learned nothing in the other cities of *Italy*, which did not ſtrengthen my doubts in relation to thoſe electrical *phænomena*, which I had a deſire to verify in the courſe of my travels. Pere *La Torre*, Profeſſor of Philoſophy at *Naples*; M. *De la Garde*, Director of the Coinage at *Florence*, one who has been much engaged in theſe inquiries; M. *Guadagni*, Profeſſor of experimental Philoſophy at *Piſa*; the Marquis *Maffei*, at *Verona*; Dr *Cornelio*, at *Placentia*; Pere *Garo*, at *Turin*; all theſe, I ſay, with very excellent and well-contrived machines, and with a great deſire of ſucceeding, have attempted many times to tranſmit the odours, as well as the powers of drugs cloſed (carefully) in tubes or ſpheres of glaſs, by electrizing them: all theſe have attempted to purge a number of perſons; and, according to the accounts they gave me, have never gained their point; or the little ſucceſs they had, appeared too equivocal to draw therefrom conſequences conformable to thoſe M. *Pivati* had believed to have ſeen in his experiments.

I am now then, as it were, certain of what I began to believe laſt year, when I printed my treatiſe, intituled, *Recherches ſur les Cauſes particulieres des Phænomenes Electriques*: I am, I ſay, as it were certain, that M. *Pivati* has been deceived by ſome circumſtance to which he had not given ſufficient attention; and what makes me believe it more than ever is, that he aſſured me himſelf, that this transfuſion of odours, and of drugs, through electrized glaſs veſſels, had never manifeſted itſelf to him but once or twice directly; I mean by a ſenſible diminution of bulk, and by ſuch emanations as the ſmell was capable of perceiving.

Since I have underſtood *Italian*, I have been ſurpriſed not only to find this confeſſion in a letter printed at *Lucca* *, but alſo to ſee, that it had not had all the effect it ought to have had upon the minds of thoſe, who have been in a ſituation to be inſtructed: for my own part, had I known it earlier, I might have ſaved myſelf a great part of the trouble I have taken in verifying the fact; and I am aſtoniſhed, that they ſhould be deſirous of building upon ſuch very ſlender foundations.

It is however upon this pretended tranſmiſſion, and with a glaſs tube, *which was cracked from one end to the other*, as M. *Pivati* tells you himſelf †; it is, I ſay, upon this fact, than which, in my opinion, nothing can be leſs certain, that they have eſtabliſhed the uſe and effects of *lined tubes*, of which they are willing to abate nothing. But how can we reconcile theſe two things, the almoſt never failing operation of *lined tubes*, upon ſo many diſtempers which are ſaid to have been cured, or conſiderably relieved, on one part, and on the other the tranſmiſſion ſo very ſeldom to be perceived of the odours of the drugs incloſed in thoſe glaſſes,

* Page 28. *Un tale dileguamento ſuccedutomi in un cylindro, non mi è poi veramente ſucceduto in altri, di quali mi ſon ſervito per varie guarigioni*

† *Si conſumo la materia interna a ſegno, che ſi riduſſe non oſtante l'eſſere quaſi ermeticamente ſerato alla ſottiliezza di un delicato foglio di carta, e come un capo morto, che ne tenea più odore ni ſapore, e fino il vetro medeſimo quaſi conſunto ſi apri da ſe ſteſſo in più fiſſure per longo.*

with which you electrize? If it is truly the *Peruvian* balſam, the benjamin, camphire, *&c.* which, being animated by the electric matter, have brought about ſo many cures, as M. *Pivati* has given us in his writings, why do not theſe ſtrongly-ſcented ſubſtances ſend forth their *effluvia* copiouſly, and always, in thoſe places where the experiments are made? And why do they not communicate themſelves by their odour to all perſons, who are penetrated thereby by means of electrification? Will they ſay, that the Electricity, ſpecifically operating upon their medical virtue, ſeparates it from their odoriferous quality? Miſerable ſubterfuge! Which does not merit to be oppoſed ſeriouſly; and the more ſo, as it is by the tranſfuſion of their odours, that they pretend to be aſſured of the efficacy of their *lined tubes*.

I am diſpoſed to believe, that the Electricity may have cured or relieved diſtempered perſons; but I do not find the proofs of M. *Pivati* ſufficiently ſtrong, or ſufficiently certain, to make me conceive, that the *lined glaſſes* have contributed to theſe good effects. I think, and M. *Verati* himſelf appeared to me pretty much of the ſame opinion, that if any one has been ſo happy as to cure diſtempers by electrifying with glaſſes containing drugs, all that can be ſaid in favour of theſe ſubſtances is, that they have not hindered the operation of Electricity.

M. *Pivati* appears by his converſation an honeſt and diſintereſted perſon, and one capable of inducing me to be of his opinion: but among the facts which he collects in his writings to fortify his proofs, I find ſome that do not do much honour to his delicacy in chooſing; and which may make him ſuſpected of too great credulity. Would one believe with him, for example, that the electric virtue was capable of ſetting a watch a going, which was ſtopped; and, by it's means, of regulating it's motion, when ſo diſordered as to be impracticable to be done by the hands of the workmen *? Would one believe with him upon the faith of a letter void of authority, and without having tried it, "that an "ounce of mercury had been entirely evaporated through the pores of a "glaſs veſſel, with which a man was electrized, which had made his "ſkin of a leaden colour, and which had been followed by a copious † "ſalivation?" This fact, which was ſaid to have been done at *Naples*, intereſting as it is, had there made ſo little noiſe, that I was not able to find any traces of it during my ſtay in that city, after the printing and publiſhing of the book, in which it is cited.

If any one ſhould think fit to ſay, that it is from humour, or from ſome perſonal intereſt, that I am ſo obſtinate in diſbelieving the facts publiſhed in *Italy*, which are the ſubject of this Memoir, I flatter myſelf, that ſo unjuſt an imputation will make no impreſſion upon reaſonable people, by whom I have the honnour of being known, either perſonally,

* *Reflеſſioni fiſiche ſopra la medicina ellettrica*, p. 103. *La ſubita efficacia (della ellettricita) in dar giuſto movimento alle moſtre, di orologio, o ferme, o reſtie, o ritardanti ſanza rimedio.*

† Ibid. p. 153.

or

or by my writings. Have not I received and publiſhed in *France* all the wonders in Electricity, which have manifeſted themſelves in *England*, in *Germany*, and in *Holland*, as ſoon as I have been able to be aſſured thereof by repetition of them? Have not I ſpoken and written concerning the cure of the paralytic of *Geneva*, as a man truly perſuaded of the truth of the fact, ſince it had been ſo juſtly authenticated *? By what caprice then am I made more difficult of believing what paſſed in *Italy* than in other countries, if the *phænomena*, which are pretended to have been ſeen there, could have been repeated; or if the teſtimonies, which they offered me, were not conſiderably weakened, or entirely aboliſhed, when, being in the places themſelves, I was in a condition of knowing their juſt value?

Had I only conſulted my perſonal intereſt, to whom would it have been more convenient than to me, to have adopted theſe novelties? If they were real, they would have been ſo many evident proofs of a principle †, by which I have endeavoured to account for the electrical *phænomena*: a principle, which as yet has ſufficicently well ſerved me, and which, having offered itſelf to Mr *Watſon* as well as to myſelf, has enabled him likewiſe to give ſome inferences exceedingly probable concerning them: would not odours, would not medical ſubſtances, carried through the pores of glaſs, prove, without doubt, that the *effluent* electric matter ſerved them for a vehicle? If purging ſubſtances were forced to paſs into the hand, and into the body, of an electrized perſon, could one doubt of their being introduced there by the *effluent* matter, which came to the electrized body? If the Electricity reſtores health to a ſick perſon, in delivering him from ſome vitious humour, might not I ſay with great probability, that this effect is brought about by the effluence of the electric matter? Eſpecially as I have demonſtrated by experiments, made with great care, that this ſame matter, in going from the body electrized, accelerates, and conſiderably augments, the inſenſible tranſpiration of animals, and, in general, all organized bodies, replete with fluids.

I have then ſet apart my own intereſt to follow the truth; and if prejudice has tended to lead me aſtray, it would be in inclining me to receive rather than call in doubt the facts, which are the ſubject of this paper. It is only becauſe I cannot conſider them as true, that I refuſe to believe them; and this even with regret, as they favour my ſyſtem: this indeed is of no great importance; but what makes me more deſire their reality, is, the great good which would reſult to ſociety. Could any good ſubject, poſſeſſed of the art of healing by Electricity, as M. *Pivati* pretends to be, ſpend his whole time better than in devoting it to the relief of a great number of human creatures, afflicted with great variety of maladies? I am induced to believe, that the greatneſs of this

* See my Eſſay *ſur l'Electricité des Corps*, printed at *Paris*, 1746, and my *Recherches ſur les Cauſes particulieres des Phænomenes electriques*, 1749.

† *Eſſai ſur l'Electricité des Corps*, p. 148 et ſuiv.

idea has impoſed upon thoſe, who have publiſhed, without doubt, with a little too much precipitation, this new Medicine: the great deſire of being uſeful has made them hope; and the goodneſs of their hearts making them diſpenſe too eaſily with the ſeverity of a neceſſary examination, it may be imagined, that they have conſidered as real ſucceſs, what was in truth only a phantom.

It remains to ſay, that in theſe reſearches I have coveted truth, only for her own ſake; and have no intereſt in convincing thoſe who may think proper ſtill obſtinately to believe, what has been publiſhed concerning lined tubes, electrical purgations, inſtantaneous cures, *&c.* I do not pretend to make any of my opinion, but thoſe, who, having read without prejudice what I have here related, may find themſelves touched by my reaſons: but if after this there can be any one, upon whom the love of the marvellous can make a victorious impreſſion, I ſhall not think ill of them, if they embrace opinions oppoſite to mine; *Qui vult decipi, decipiatur.*

An Obſervation on the Barometer, *by* Sam. Chriſtian Hollman, Philoſ. Prof. Pub. Ord. Gotting. N°. 475. p. 239. Jan. *&c.* 1745. *Dated* Oct. 15. 1744. *Read* Jan. 10. 1744-5.

II. 1. The common Barometer, which I uſe, is ſunk ſo deep at the time of my writing this, that there is only one line remaining of the common ſcale. The S. wind blows with great violence. But at 12 at night the quick-ſilver fell even below the laſt line of it's ſcale.

The Agreement of Barometers *with the changes of the* weather; by the ſame. N°. 492. p. 101. April, *&c.* 1749. *Read* April 23. 1749.

2. It has hitherto ſeemed to be a matter of great difficulty, to explain the true cauſe of the alteration of the height of the quickſilver in the Barometer, and the manner of it's agreement with the ſubſequent changes of the weather. That this is not unattended with ſome difficulty is *evident* from the hypotheſes hitherto framed by the greateſt men. That invented and publiſhed by *Leibnitz*, which has given riſe to controverſies between *Schellhamer* and *Ramazzini*, has been preferred before all others. But it has been ſufficiently proved, by Dr *Deſaguliers*, that this hypotheſis of *Leibnitz* is contrary to the laws of Hydroſtaticks: whence it is ſurpriſing, that ſo many have ſince endeavoured to defend it; eſpecially as it is evident that it by no means agrees with the *phænomena* of Nature. Now if the cauſe of deſcent and aſcent of the quickſilver in Barometers was that which *Leibnitz* has alledged, and if the quick-ſilver could not deſcend much ſooner than the drops of rain began to deſcend thro' the incumbent atmoſphere: and again if it could not aſcend before they ceaſed to deſcend; both which are contrary to experience, as is well known by all who have attended the changes of the Barometer, even for a month or two. For the quickſilver will often fall for 3 or 4 days together before the leſt drop falls from the ambient air; and riſe again, tho' it continues to rain for ſeveral days. Nay the quick-ſilver often falls, without the ſucceſſion of any rain. Nor do the drops of rain, that fall at the end of it's deſcent, preſs the ſuccumbent air in like manner as a weight, in the experiment of *Leibnitz*, deſcending thro' water

water preſſes the bottom of the veſſel in which it deſcended; nor can they reſtore to the air it's loſt equilibrium, as the weight does to the ſuſpended veſſel. Thus all things in the Leibnitian experiment are in nature difform and diſſonant, ſo that I am in doubt whether I ſhould moſt admire the genius of *Leibnitz*, a man in other reſpects of great merit, or rather the blind aſſent of his followers. To this we may add what is called a *fallacy* of *Leibnitz*, when he ſuggeſted that the cauſe of the equilibrium being altered, was that ſolids deſcending in a fluid do not gravitate during their deſcent into the ambient fluid. For the experiment ſucceeds beſt of all, when the diameter of the body deſcending thro' water, as of a leaden ball for inſtance, is very little leſs than that of the tube, and therefore almoſt fills the cavity of the tube, which is full of water. For becauſe the ball cannot deſcend, without expelling a volume of water ſimilar to itſelf, from it's place, it muſt impreſs upon it a motion receding from the bottom of the veſſel, and therefore in the whole time of that deſcent, the volume of water, which anſwers to the magnitude of the falling ball, muſt thereby receive a *niſus* and *impetus* by the natural force of bodies contrary to the centripetal force; and the tube, being wholly ſuſpended on the leg of the ballance, muſt *loſe* it's equilibrium, during the deſcent of the ball, till the ball reaches the bottom of the veſſel, and ſo a part of the water muſt ceaſe to be driven upwards. Thus the famous experiment of *Leibnitz* muſt be faulty and erroneous, and is moſt prepoſterouſly applied to the explanation of the mutations of the Barometer. I paſs over the *vitious circle*, which *Leibnitz* has been guilty of thro' his whole reaſoning. For he is to aſſign the *cauſe*, why the air is lighter before rain, and therefore makes a leſs preſſure on the quickſilver ſtanding in the veſſel of the Barometer, and yet he ſuppoſes it to be rendred lighter: for as long as the gravity and elaſtic force of the air, by which the exhalations might hitherto have been ſuſtained, are ſtill the ſame, they will never be reduced into drops, or deſcend thro' it. But it would be tireſome to add more in a thing ſo evident. I ſhall however ſay a few words with regard to the thing itſelf.

In the firſt place; it ſeems a great difficulty to moſt, what is the cauſe, why when the air is turbid and made more heavy by various exhalations, the quickſilver falls in the Barometers, and riſes again when it is clear, and is therefore rendered lighter? When every thing ought rather to happen quite contrary. But when the queſtion is thus formed, as we ſee it is done by moſt, is not ſomething tacitly aſſumed and ſuppoſed, which has never yet been proved, nor can eaſily be proved, namely, that the air is rendered heavier, when it is turbid and replete with various exhalations; and on the contrary lighter when it is clear? Nay is it not at the ſame time tacitly ſuppoſed, that thoſe vapours and exhalations *come into* our air, and diminiſh it's elaſtic force, at the very time that we ſee them: and on the contrary, that they *are gone*, and that the air is freed from them, and purified, as ſoon as they are withdrawn from our ſight? Therefore if we deny both theſe ſuppoſitions, the greateſt part of the difficulties

difficulties hitherto ſtarted immediately vaniſhes. But that they may, and ought to be denied may be proved by very ſubſtantial reaſons. For, who that is verſed in theſe matters, will affirm, that the motes in our circumambient air are not preſent before they are diſcovered to our ſight by the ſun beams ſhining into a dark room, or that they retire as ſoon as an open and free light is admitted? But not to mention a great many experiments that have been made with acid and alkaline ſalt, which are ſufficiently known, let us make uſe of an example, which is more to our preſent purpoſe. Who does not know, that in the glaſs bell put over the wet orb of the pnuematic engine, as ſoon as the pump begins to work, ſome light clouds begin to ariſe, which, as ſoon as the air is admitted again into the ſame bell, immediately diſappear? Now it would not be eaſy to find any one ſo unſkilful in theſe things, as to think, that the watery exhalations, from which theſe miſts ariſe, enter the glaſs bell at the time of their becoming conſpicuous to us, by the diminution of the elaſtic power of the air; or that they no longer exiſt there, after they are withdrawn from our ſight, and are again hidden in the pores of the air, which before ſuſtained them, after it is become again more heavy and elaſtic. We know therefore, that thoſe exhalations are preſent, before they approach nearer to each other, and become viſible to us; and we know that they are not annihilated, or no longer exiſting in the air, when being more diſſipated they eſcape our ſight, on account of their very great ſubtilty. But they begin to approach nearer to each other, when the elaſtic form of the air is ſo far diminiſhed, that they cannot eaſily be ſuſtained thereby as they were before; and recede again, and ceaſe to affect our ſight, when the air has it's former gravity and elaſtic force reſtored. Therefore *both* theſe things happen *at the ſame time*, that the elaſtic force of the air is diminiſhed, and at the ſame time the exhalations, which hung together in it, are again gradually looſe by it, and begin to appear to our ſight: and again, that the air recovers it's elaſticity, and at the ſame time, that the vapours hanging in it are diſſipated and diſappear; but *the one* cannot therefore be ſaid to be the *cauſe* of the other: nor can the air therefore be ſaid to be at one time more, at another leſs heavy, on account of the ſame exhalations.

Now if we ſuppoſe the ſame thing to happen in the air that ſurrounds our earth; the greateſt part of the difficulties is removed. For let us ſuppoſe it's elaſtic force to be diminiſhed, by any means whatſoever, of which we ſhall ſay more hereafter; the exhalations hanging in it muſt neceſſarily ſubſide and become viſible: and when it is by any means reſtored, the vapours and clouds therein muſt again be diſſipated and vaniſh. But at that very time when thoſe ſubſiding vapours come into our view; the mercury in the Barometer begins to fall, nay often ſome time before for the ſame reaſon; but yet thoſe vapours do not in like manner contribute any thing to it's deſcent, when the vapours and clouds begin to diſappear in our circumambient air, or even a little before the mercury begins to aſcend; and yet that ſerenity of the air is no more the cauſe of the

the aſcent of the mercury in the Barometer, than the aſcent of the mercury can be ſaid to be the cauſe of that ſerenity. It therefore again deſerves to be called a *fallacy* of the *cauſe*, when one of thoſe *phænomena*, which happen about the ſame time, is referred to the other, and ſo one has hitherto been taken for the cauſe of the other by moſt perſons.

But to make all this apparent to the eye, take a glaſs cylinder about 3 or 4 inches in diameter, open at both ends, and long enough to receive a portable Barometer. Let this cylinder be put for a little while before the experiment on the orb of an air pump, covered with water and wet leather, that ſome watery vapours may in the mean time enter into the air therein contained. Afterwards let a Barometer be introduced, and let the cylinder be exactly cloſed at the top, that the air may be exhauſted. Now if all things are rightly prepared, and the included air begins to be pumped out; both a ſort of *miſt* will begin to *riſe* immediately in the cylinder, and the mercury of the Barometer at the ſame time to ſubſide: both indeed, as is apparent enough, from one and the ſame cauſe, but neither of them from the other. But if the ſame air, which was juſt now taken away, is immediately admitted again into the glaſs cylinder; the natural *ſerenity* of the included air will return directly, and the Mercury of the Barometer will aſcend at the ſame time or a little ſooner: and yet it is plain enough that one of them does not depend on the other. Nay at the ſame time it is manifeſt enough, that it is not rendered *heavier* in one caſe, nor lighter in the other. Thus art in ſome meaſure imitates nature: tho' becauſe of the wonderful complications of natural cauſes, it can never be ſufficiently exact.

But tho' the chief difficulty is now removed, yet the whole affair is not brought to a concluſion. For it ſtill remains to be enquired, by what cauſes the elaſtic power of the air can be ſo diminiſhed or increaſed, as to produce theſe alterations. But yet the anſwer does not ſeem very difficult. For the cauſes which ſhew how eaſily the air is expanded and rarified by any approaching heat, and particularly how great is the force of the *ſun-beams* falling perpendicularly, will ſhew, or perhaps, this alone will ſeem ſufficient, how the *equilibrium* of the air is taken away, if there were no others, which are however various. We will paſs over the *diurnal revolution* of the earth, and our air with it, about it's axis; and alſo the *annual motion* of them about the ſun; we will not mention the many *burning mountains* on the ſurface of our earth, nor the many thunders and lightnings in the air; nor the many *earthquakes* and *ſubterraneous fires*, that ſo terribly ſhake the ſurface of the earth and ſea, tho' each of them may have a wonderful effect in increaſing or diminiſhing the elaſtic force of the air: and ſhall at preſent only conſider one thing, which ſeems more worthy to be mentioned than the reſt.

It is allowed by all, that the elaſtic force of the air which immediately touches the ſurface of our earth, depends chiefly on the weight of the incumbent air. It is alſo no leſs known, that the lower air is more or leſs elaſtic according to the greater or leſs height of the incumbent column

of

of air, by the different height of the barometrical mercury, on mountains of greater or leſs height, and in lower places of the earth. Nor is it leſs generally allowed, that the moon is the primary cauſe of the flux and reflux of the ſea. But by what means ſoever this wonderful *phænomenon* is effected by the body of the moon on our earth, of which we need not here particularly treat; this certainly is paſt all doubt, that the moon cannot act on the ſeas of our globe, without acting at the ſame time on the *air which lies* between the moon and our earth. Now if the mobility of fluids is in proportion to their denſity, and their denſity in proportion to their ſpecific gravities; the air neareſt the earth, which is about 860 times lighter than freſh water, will be about 900 times more moveable than ſea water: and therefore the very ſame cauſe, which gives ſo conſtant and regular a motion to our ſeas, acting by the ſame force, may much more eaſily increaſe or diminiſh the height of our air. By a ſort of *flux* therefore ariſing in a determinate region of the air, the columns of air cannot fail of becoming *higher* in the ſame place, and therefore, *caeteris paribus*, the elaſtic force of the lower air muſt be increaſed: but upon a *reflux*, the height of the columns of air muſt neceſſarily be *diminiſhed*, and ſo again, *caeteris paribus*, the elaſticity of the lower air muſt decreaſe. And perhaps this difference of heights is the greater, as the ſpecific gravity of the air, which is moſt rare in the utmoſt limits of our atmoſphere, is overcome by the ſpecific gravity of our ſea waters, and of the lower air. But that this flux and reflux of the air does not obſerve it's alternate motions ſo regularly and conſtantly as the reciprocal tide of the ſeas; beſide it's very great fluidity, thro' which it may be agitated by very ſlight cauſes, the cauſes above enumerated without doubt effect, by which it is manifeſt enough, that there are wonderful agitations and perturbations of the air, almoſt continually, in different parts of our terraqueous globe. From all theſe cauſes therefore taken together, the changes of the heights of columns of air in different times and places, and alſo the changes in the lower air depending on theſe, are to be ſought in the barometrical *phænomena*.

There remains one thing to be juſt ſpoken of. We find many are ſollicitous to *foretel the weather* from the riſing or falling of the mercury in the Barometer, and endeavour to form certain rules for that purpoſe: ſo that this ſeems to be the only thing, which perſons otherwiſe not very ſkilful in nature may expect from this wonderful machine. And I could wiſh, that we knew any thing certain about this alone; becauſe it would be of great uſe in human affairs. But theſe things, which we would have ſo conjoined, do not ſeem to be neceſſarily connected. For our baroſcopes cannot properly, and of themſelves, ſhew any thing except the increaſe or decreaſe of the elaſtic force of the air; but the weather depends on various exhalations, exiſting at the ſame time, or together in the air, or not exiſting or at leaſt not preſent in the ſame quantity. It may therefore be, that to diminiſh the elaſtic force of the air, and ſo to make the mercury fall in the Barometer, there muſt

be a ſufficient quantity of thicker exhalations in the air, and it muſt thereby become turbid, and the vapours begin to ſubſide, and ſo coaleſce into greater or ſmaller drops of rain, and other *phænomena* follow. But it may as eaſily be, that, tho' the elaſticity of the air is diminiſhed, yet becauſe of a defect of a ſufficient quantity of exhalations therein, no ſenſible change of the weather may happen. The ſame may happen on the contrary, by too great a quantity of exhalations in the air; tho' the riſing of the mercury may moſt evidently ſhew, that the elaſtic force of the air is increaſed. Becauſe therefore theſe things *coincide* as it were by ſome *accident*; no certain prediction can be made of a change of weather, either by the riſing or falling of the Barometer. But we may ſafely enough make a *negative* concluſion from it. For a conſtant conſent of obſervations ſhews, that, if foul weather has followed a deſcent of the mercury, fair will not ſucceed, till the mercury begins to riſe again: or if the aſcent of the mercury has been joined with fair weather, there will not be clouds or rain, till the mercury has begun to fall. And this is often of as great uſe in common life, as if we could always *poſitively* predict, what weather ſhould exactly follow the aſcent or deſcent of the mercury. The fall however of the mercury does *more frequently* predict foul than fair weather; and the riſe more often fair than foul, as is confirmed by manifold experience: and ſo it may be of great uſe, to be able to judge *ſafely* in ſuch occurrences. I have alſo obſerved, that in theſe regions, when the wind blows from any point between the N. and W. rain and foul weather begin, or are continued more often than fair, notwithſtanding the riſing of the mercury in the Barometers.

III. 1. I ſend you herewith an extract from my regiſter of the weather, ſhewing the ſtate of my Barometer and Thermometers, for ſome days of laſt week: in which you will obſerve a ſudden change of the temperature of the air, particularly on *Thurſday* morning the 3d inſtant, and by the ſame, you may ſee the little uſe a Thermometer is of, when kept within-doors, to determine the ſtate of the air abroad, as to heat or cold.

A letter from the Rev. H. Miles, *D. D. to Mr* Henry Baker, *F. R. S. concerning the difference of the degrees of* Cold *marked by a* Thermometer *kept within doors, or without in the open air.* N°. 484. p. 613 *Oct &c.* 1747. *Read* Dec. 10. 1747.

I have two Thermometers filled with mercury, and of the ſame conſtruction, made by the late Mr *Siſſon*, in the *Strand*. The one is placed without my chamber-window, in a north-eaſt ſituation, under covert, contrived to admit a free paſſage of the air, but to keep off ſun and rain; the other hangs within the window, about three feet from the former, where the ſun never falls on it: The room is conſtantly occupied, as a bed-chamber, but has had no fire in it this ſeaſon.

It appears by the adjoining table, that on *Tueſday* the 1ſt inſtant, at 8 in the morning, the Thermometer without ſtood at at 17° above 0. or freezing point; that within at 14. At 9 at night, that without was at 0. and that within at 12 above 0. So that in the ſpace of 13^h the former had fallen 17°, the latter but 2. For the other particulars, relating to the Barometer, wind, and weather, I refer to the table.

As the Barometer had been for a good while paſt ſubject to ſudden conſiderable variations, I ſuſpected the ſevere cold on *Wedneſday* night and *Thurſday* morning would not continue long: accordingly, upon my obſerving the Thermometer without at 4 in the morning, I found it at $\frac{0}{9}$ 9° below the freezing point, that within at $\frac{5}{0}$ 5° above the freezing point. But at 8^h the ſame morning, I found the Thermometer without at $\frac{3\frac{1}{2}}{0}$ $3°\frac{1}{2}$ above freezing, and that within at 4° above; ſo that in 4 hours time, that without had riſen $13°\frac{1}{2}$, and that within had fallen 1°. This naturally led me to examine what ſigns there might be of a thaw begun, but could find none, in the ſnow (which was 5 inches deep) or in the poſt, on the windows, but within an hour it was viſible enough, and before 10 the houſes dropt. I would obſerve to you, that the wind at 8 in the morning had varied very little, if any, from what it was the night before, *viz.* from the E. but ſoon after it bore to S. E. and S.

May not this ſudden change of the temper of the air be attributed to a ſubterranean heat? And may not the ſhifting of the wind be cauſed, in a great meaſure, by the ſame?

Days	Morning Barom.	Morning Ther.	Evening Barom.	Evening Ther.	December 1747.
1	28 6 3	$\frac{17}{0}$ $\frac{14}{0}$	29 3 0	0 12 0	At 8 Morn. Wind high at S. W. much Rain preceding Night. Showery afterward in the Morning, and Wind exceeding high. Sleet at $1\frac{1}{2}$ *p. m.* calmer and clearer ſoon after, Wind N. W. and N. began to freeze in the Evening, clear at $9\frac{1}{2}$ *p. m.* when the Evening Account was ſet down.
2	29 5 5	$\frac{0}{5}$ $\frac{9}{0}$	29 4 0	$\frac{0}{2}$ $\frac{5}{0}$	At 8 Morn. cloudy thick Air, hard Froſt; at 4 ſame Morn. very clear, and Glaſſes were at 29 5 0 and $\frac{0}{3}$ and $\frac{4}{0}$ Wind Eaſt, cloudy all Day, at $4\frac{1}{2}$ *p m.* Snow fell, and was deep before 8. Evening Account taken at $9\frac{1}{2}$ *p. m.*
3	29 5 6	$\frac{3\frac{1}{2}}{0}$ $\frac{4}{0}$	29 4 9	$\frac{13\frac{1}{2}}{0}$ $\frac{9}{0}$	At 8 Morn. cloudy, Wind at near E.; blows briſk at 4 the ſame Morn. Glaſſes were 29 6 1 $\frac{0}{9}$ and $\frac{5}{0}$ Rain before 11. Evening Account at $9\frac{1}{2}$ *p. m.*

Explication.

1 Day Barom. Morn. 28 Inches $\frac{6}{10}$ $\frac{3}{100}$.

Ditto Therm. Morn. $\frac{17}{0}$ is 17 Degrees above freezing Point the upper Number is for the Therm. without Doors, the lower for that in my Room, and ſo for the reſt.

2. Chemiſtry

2. Chemiſtry being the moſt extenſive branch of Experimental Philoſophy, hath furniſhed mankind with the greateſt number of curious and uſeful diſcoveries; for not only the art of ſeparating metals from their ores, of which metals are formed ſuch variety of uſeful Inſtruments, but likewiſe Cookery, which is ſo much concerned about the food of mankind during health, and alſo Pharmacy, which furniſhes medicines for the reſtoring health when loſt, the art of dyeing, and many other uſeful manufactures, all owe their improvements to this ſcience; many of which have been light on unexpectedly by the operator, while he had ſomething elſe in view: but in many caſes the Chemiſts complain, that having once accidentally light on a curious experiment, upon endeavouring to repeat it, they have never been able to make their proceſs ſucceed exactly, as it did the firſt time, notwithſtanding that they made uſe of the ſame materials, in the ſame quantity, and conducted the proceſs thro' exactly the ſame operations. Where then muſt the cauſe of the miſcarriage lie? Surely in the degree of heat made uſe of in the two experiments: for, in many common operations, how uſual is it for a preparation to be ſpoiled either by too little, or, moſt commonly, by too much fire, too long or too ſhort a time applied. In order therefore to prevent theſe many miſcarriages, I would adviſe the Chemiſt, in his operations, to obſerve his clock with as much exactneſs as the Aſtronomer doth in his obſervations; and in order to know to a certainty the very degrees of heat he ever made uſe of in any proceſs, that ſo he may be able to repeat and continue the ſame again in any repetitions of the ſame experiment, let him have his laboratory furniſhed with various ſorts of Thermometers, proportioned to the degree of heat he intends to make uſe of. He will find theſe inſtruments as uſeful to him in his proceſſes, as they have proved to the curious Gardener in his ſtoves, who by them is taught to keep his plants in the ſame degrees of heat, as are natural to them in their reſpective climates; which hath been ſet forth in tables, after a very ingenious manner, by Mr *Sheldrake* of *Norwich*. And beſides the enabling him to perform his operations with more exactneſs, theſe inſtruments would ſave him a deal of fuel; for as liquors, while boiling, are not capable of receiving a greater degree of heat, all fuel which is uſed more than to keep them in that ſtate is uſeleſs; and the like happens in many other caſes.

A diſcourſe concerning the uſefulneſs of Thermometers *in Chemical Experiments; and concerning the principles on which the* Thermometers *now in uſe have been conſtructed; together with the deſcription and uſes of a* Metalline Thermometer, *newly invented by* C. Mortimer, *M. D* Sec. R. S. *&c.* Ibid. p. 672. *Read* May 8, 1735. *here printed with ſome alterations.*

Theſe inſtruments would alſo be of great ſervice to Maltſters, Brewers, Diſtillers, and Vinegar-makers; for by Thermometers placed in different parts of the heap of wetted malt, the proper heat for it's ſprouting might be determined, and then regulated: the ſame for the heat of the kiln when the malt is ſpread on it. By Thermometers the Brewer may aſcertain the heat of the water when he pours it upon the malt, the heat of the wort when he ſets it to work, and the heat while working: and in the like manner the Diſtiller and Vinegar-maker, in a word, every artificer, who employs heat in his buſineſs, may by theſe inſtruments be certain of every degree neceſſary in each part of his work.

Many experiments ſhew, that all known bodies, whether fluid or ſolid, increaſe their bulk or rarefy by an addition of heat; and, on the contrary, contract or become more denſe by the diminution of heat, which is the preſence of cold: and theſe alterations are always more or leſs ſenſible in proportion to the natural rarity or denſity of the bodies.

The air we live in, as it is the moſt rare and light fluid, ſo are it's alterations the moſt ſenſible; and indeed I know of no experiments which determine how far it is capable of being expanded by heat, or condenſed by cold; only we find that it will make it's way thro' any fluid in which it lay dormant, when it's elaſtic property is rouzed by the approach of ſuch an heat as will make the fluid boil. On the other hand, when compreſſed by a fluid ſo contracted by cold, as to freeze, or become ſolid, it's elaſticity will only bear a certain degree of compreſſion, till the force wherewith it endeavours to reſtore itſelf, exceeds the force by which the parts of the ſolid, that confines it, adhere to each other, and ſo burſts it's priſon; as we often ſee during hard froſts in ice, glaſs, and other hard bodies, whoſe parts cannot ſtretch.

Next to air is *alcohol*, or the higheſt rectified ſpirit of wine: this, water, and all other liquids, are capable of receiving no greater degree of heat than what makes them boil, as was firſt demonſtrated by M. *Amontons*, a Member of the *R. Acad. des Sci.* at *Paris*; but that ingenious inventor of the quickſilver Thermometer Mr *Fahrenheit* hath diſcovered, that when the Barometer marks a greater preſſure of the atmoſphere, the ſame liquor will receive 8 or 9° more of heat than when the Barometer is at the loweſt. From hence the great *Boerhaave* gives the hint, that, from nice experiments being made of the different degrees of heat marked by a Thermometer in boiling water compared with the different heights of the Barometer, and tables formed upon them, a Thermometer applied to boiling water might, at ſea, where the motion of the ſhip hinders obſervations with the Barometer, ſerve to determine the difference of the gravity of the atmoſphere *.

Theſe, and all other liquids, by a certain determinate degree of cold peculiar to each ſort, loſe their fluidity, and freeze, or become ſolid, but not in the ſame order as by heat they boil; for by cold, oil or water is ſooner frozen than ſpirit of wine, tho' ſpirit of wine will boil ſooner than oil or water. All ſolid bodies likewiſe, as minerals, metals, and even ſtones, will become fluid, or melt, at a certain degree of heat peculiar to each ſpecies; and, when thoroughly melted, it is probable they are capable of receiving no higher degree of heat; and, on the abſence of that heat to a certain degree, they all return to their natural ſolid ſtate. Hence we may reaſonably conclude, that ſolidity is the natural ſtate of all bodies; and that ſome are only accidentally fluid, becauſe their conſtitution is ſuch as to melt by thoſe degrees of heat which our atmoſphere is moſt commonly ſubject to. All ſolid bodies are obſerved to contract themſelves into ſmaller dimenſions by cold, and gradually to expand themſelves at the approach of heat, till at laſt, being by

heat

* See his *Chemiſtry*, Tom. I. p. 171.

heat forced to the greateſt degree of expanſion, the particles of which they are compoſed loſing their coheſion, they become fluid; but no experiments have yet been made, which determine whether ſolids, expoſed to cold beyond certain degrees, will ceaſe to contract any more.

Dr *Muſſchenbroek*, Prof. Aſtron. at *Utrecht*, hath lately invented a very ingenious inſtrument, which he calls a *Pyrometer*, and which Dr *Deſaguliers* hath made ſome improvements to *; a full deſcription of which he hath given in his *Courſe of Experimental Philoſophy*. †

By this inſtrument the elongation of rods of ſeveral ſorts of metals by the approach of a certain number of flames of a ſpirit-lamp, and likewiſe their as ſudden contraction, on the extinguiſhing one or more of thoſe flames, is rendered ſenſible to the eye: which ſufficiently evidences the matter of fact, and puts it beyond all doubt.

From the above-mentioned property of bodies contracting and expanding in cold and heat, have all Thermometers been conſtructed, that have ever been made uſe of in order to obſerve and compare the different degrees of heat, either in our atmoſphere, or in other bodies. The moſt ſimple and moſt ſenſible of any is that aëreal Thermometer deſcribed by Mr *Boyle*, in his New Experiments and Obſervations touching cold, *Lond.* 1683. 4*to. p.* 39. It conſiſts of a glaſs bubble, with a very ſlender ſtem not bigger than a raven's quill. The bubble is left full of air, and a few drops of water being conveyed into the ſtem in an erect poſition, will there remain ſuſpended to a certain height; but, by the leaſt addition of heat, the air in the bubble expanding will puſh the water up higher; or, by the approach of cold, the air contracting, the water will fall lower in the ſtem. This inſtrument may be of uſe in ſmall degrees of heat, and in cold, till the water begins to freeze, when it becomes uſeleſs.

The next in order of ſenſibility is that firſt invented by *Cornelius Drebbelius* of *Alcmar*, and improved by *Boerhaave* ‖. It conſiſts of an hollow glaſs *lens* joined to a ſtem of a larger ſize than in the preceding, and a baſon into which the end of the ſtem is inverted. The air in the *lens* muſt be ſo much rarefied, that the ſtem being inverted into a tinged liquor in the baſon, the liquor will riſe up ſome way in the ſtem; then, by the application of heat to the *lens*, the liquor in the ſtem will be puſhed down, and by cold the liquor will riſe up. This inſtrument will give notice of the ſmalleſt changes in the air; but it cannot be immerſed into any liquid for chemical experiments, unleſs the ſtem were made much longer, and bent downward in form of a ſyphon: but even then it would be very unhandy, and, like the preceding, it would never ſerve for any degree below what would freeze the liquor made uſe of, nor for any above what would force out the confined air through the liquor in the baſon. Beſides, both theſe inſtruments, being ſubject to the preſſure of the atmo-

* This inſtrument hath ſince been greatly improved by Mr *John Ellicot, F. R. S.*

† *Vol.* I. *p.* 421. *&c.*

‖ See his *Chemiſtry*, Tom. I. p. 152, 153.

ſphere,

ſphere, are not proper, without comparing the Barometer at the ſame time, to determine the degrees of heat at a great diſtance of time between each experiment.

The moſt uſual ſort of Thermometers is that deſcribed in the account of the experiments by the *Academy* of *Cemento*; which being the common ones, made of ſpirit of wine tinged, it is needleſs to deſcribe. The bounds of the degrees of heat which theſe will meaſure, and which is commonly called the range of the inſtrument, are from the degree which freezes ſpirit of wine, up to that which makes it boil. The ſpirit Thermometers, commonly made here in *London*, are ſo graduated, that when the ſpirit is rarefied to the degree that the moſt ſultry ſunſhine commonly known in our climate of $51\frac{1}{2}^{\circ}$ N. Lat. can raiſe it, there is placed the mark 0. or degree of no cold. Some few are marked 10 or 20 above this, if they are deſigned to be uſed in hotter climates; but all are graduated downwards from this: ſo that the 45°. is the point of temperate, and 65°. is the point of freezing, and 100°. is placed juſt above the ball. But the moſt accurate ſpirit Thermometers are thoſe lately made by M. *Reaumur*, he hath taken a great deal of pains, and uſed great exactneſs, in fixing the certain points of freezing of water, of temperate air, and boiling water. He determines the freezing point, by leaving his Thermometer a conſiderable time in water, into which is put a good deal of ice, at a time when the water would not freeze of itſelf; and this he marks 0. or the degree of no heat; and his ſcale is marked with numbers running downwards from 0. meaſuring the degrees of cold, and upwards meaſuring the degrees of heat: at $10\frac{1}{5}$ upwards he marks the point of temperate, which he determines by placing his inſtrument in a ſubterranean cavern, which is neither affected by froſt nor ſunſhine, but is obſerved to keep an equable temperature all the year round; ſuch as deep cellars and wine vaults commonly do. In boiling water he finds that his Thermometer riſes to his 80th diviſion, * or 80°, which are formed by dividing the ſpirit when condenſed to the freezing point, into 1000 equal parts; ſo that, with the heat which makes water boil, the ſpirit is expanded only $\frac{80}{1000}$, more than with the cold which freezes water.

Theſe ſpirit Thermometers are of uſe in experiments where ſomewhat greater cold than the freezing of water is required; but they can never be of uſe in any degrees of heat beyond the boiling of the ſpirit itſelf; becauſe it then becomes volatile, or riſes up in ſteam, and not only expands no more, but likewiſe the quantity is diminiſhed by the particles which fly up from the ſurface of the liquor, and are ſuſpended in the top of the tube.

* But, with ſubmiſſion to ſo great a man, I cannot apprehend that his Thermometers, when the ſpirits are raiſed up to 80 do mark any greater degree of heat than their own ſpecific boiling heat, which, if they are *alcohol*, or the moſt rectified ſpirits, anſwer to 174. of *Fahrenheit*'s ſcale; if of the ſtrength of common brandy to 190.

Many

Many have filled their Thermometers with various ſorts of oils * : theſe indeed will meaſure many degrees above the boiling of water, till they boil themſelves, and then they have the ſame defect as the ſpirit ones juſt mentioned, which is the liquor loſing of it's bulk by evaporation; and they congeal much ſooner than water, and ſo are uſeleſs in meaſuring any degrees of cold.

The moſt uſeful inſtruments, as they comprehend the largeſt range, are the mercurial Thermometers, which were brought into uſe by that ingenious artificer *Fahrenheit*. But, to do juſtice to a moſt worthy Member of the *R. Soc.* Dr *Halley*, he firſt gave the hint, and even propoſed the making Thermometers of quickſilver long before *Fahrenheit*'s time. However, *Fahrenheit* deſerves thanks from the world for having brought theſe inſtruments into uſe, becauſe they will meaſure the greateſt degrees of cold yet known; for no cold hitherto obſerved hath been able to freeze or render *mercury* ſolid: and in meaſuring heat, they go far beyond boiling water, even beyond the melting of tin or lead. *Fahrenheit* begins his ſcale from 0. the point to which the mercury hath been obſerved to fall by the greateſt cold in *Yſland*; and computes, that the mercury then † occupies 11124 parts. This is his point of no heat. Then reckoning upwards from this, he finds that when the mercury is rarefied only 32 parts or degrees more, common water juſt begins to freeze: in a temperate air it will riſe to about 60. The moſt ſultry ſunſhine ſeldom raiſes it to 90; the heat of an animal body to 96; the boiling of *alcohol* to 174; the boiling of water to 212; and before the mercury itſelf boils, it will riſe to 600.

I cannot here forbear giving an abſtract of a very curious and ſurpriſing experiment of *Fahrenheit*'s, concerning the artificial production of cold, as it is related by *Boerhaave* in his *Chemiſtry*. *Fahrenheit* had a mercurial Thermometer made with ſo long a ſtem, that he could carry down the ſcale 76 parts or degrees below 0. With this inſtrument he found, that cold might be produced by gradually pouring ſpirit of nitre upon powdered ice, till the *mercury* would ſubſide to 40° below 0. that is 72° ‖ lower than the cold which freezes common water. *Boerhave*, in his *Chemiſtry*, *** mentions a very pretty way of determining the freezing point: he adviſes to hang the Thermometer free in the open air, not againſt any wall or building; and near it you muſt hang a piece of very fine linen or muſlin juſt dipped in clean water: when this begins to grow ſtiff you will find the *mercury* ſtand at about the 33d degree; and it will alſo ſtand at the ſame height when an hoar froſt appears upon the ground; which he looks upon as a certain ſign of the beginning of freezing.

* See Dr *Martin*'s Eſſays *Med. & Philoſ.* p. 225. Sir *I. Newton*'s Thermometer is made of linſeed Oil. See his Scale of Heat, Vol. IV. P. ii. §. 1.

† See *Boerh. Chem.* Tom. I. *p.* 174.

‖ But what is this to the marvellous natural cold of *Siberia*, 120°. below 0? See the preface to *Gmelin*'s Flora Siberica, *Petrop.* 1747. *4to.*

*** Tom. I. p. 161.

Having

Having thus given an account of the ſeveral ſorts of Thermometers hitherto uſed, and what degrees of heat they are proper to meaſure, we find none of them capable of meaſuring the greater degrees of heat, which are the moſt commonly made uſe of by the Chemiſts in many of their operations. Beſides, all the above inſtruments, being made of glaſs, are eaſily broken by accidents, and as liable to crack of themſelves, by being taken out of a great heat, and too ſuddenly expoſed to cold. I therefore conſidered whether the above-mentioned property of ſolids, and eſpecially of metals contracting with cold, and exapnding with heat, might not be applied to the conſtruction of an inſtrument capable of meaſuring all degrees even of the greateſt cold, as well as the greateſt heat, to the melting copper or iron, which require more heat than any other metals to melt them. Altho' the alterations in metals are but ſmall, in reſpect of thoſe in ſpirits, or even *mercury*, yet it being found, that iron, *e. g.* becomes $\frac{1}{60}$ longer * when red-hot, than when of its natural temperature; and Dr *Derham*, in his laſt paper read before the *Royal Society* concerning the vibration of *pendulums*, ſays, that a rod 39. $\frac{126}{1000}$ inches long, becomes $\frac{1}{10}$ inch longer than its natural dimenſions in temperate air, by being expoſed to heat equal to that of an human body; $\frac{2}{100}$ inch longer in hot ſunſhine; that it was $\frac{2}{10}$ or $\frac{1}{5}$ inch longer than its natural ſtate, by being heated in a flaming heat; that it became $\frac{7}{100}$ ſhorter than its natural length by being quenched in cold water; and ſtill $\frac{3}{100}$ ſhorter, by being put into a mixture of ſalt and ſnow. From which experiments one may conclude, that from *Fahrenheit*'s cold of 40 below 0. to the greateſt heat iron can bear without melting, a rod of 3 feet long will have about $\frac{1}{4}$ inch increaſe; which increaſe of length will be range enough to make all the intermediate degrees obſervable upon an inſtrument.

Fig. 18. Plate IV

Suppoſe in *Fig.* 18 *AB* a rod of iron at its natural length by the heat of the atmoſphere, placed upright upon one end; upon the point of that reſts a bar *CD* moveable on an axis at *a*; and that, by making a fire about the end *B* of the rod, till it is juſt ready to melt, the rod will increaſe in length *Ab*, and conſequently puſh the bar into the ſituation *c d*. Now it is obvious to any one who underſtands ever ſo little of Mechanics, that tho' the elongation of the rod *A b* be even ſcarce perceptible to the eye, yet if upon the bar *CD* the diſtance *a A* from the axis to the place where the rod *B A* puſhes againſt it be very ſmall, and the other part of the bar *aD* very long, the arch *D d* may be increaſed at pleaſure, ſo as to bear to be divided into any number of diviſions that ſhall be found neceſſary: for the arch *D d* will always be to the arch *C c* in the ſame proportion as the diſtance *D a* is to *a C*; and likewiſe the chords of theſe arches *D d* and *Ab* will be in the ſame proportion; γ, δ, is the ſituation of the lever on the level; and if it

* Vide *Sturm. Coll.*

be

be found inconvenient to make the arm *a D* ſo long, as to make very minute alterations in the length of the rod *AB* eaſily obſervable, this inconveniency may be readily removed by having a ſecond bar *EF*, turning on the axis *g*, whoſe arm *g E* bearing up againſt the extremity *D* of the firſt bar or lever, will riſe with it, or be preſſed down by it; and the other arm *g F* being lengthened at pleaſure, the arch *Ff* will be as large as you find convenient; or even a third and fourth lever may be added.

When I firſt deſigned to have an inſtrument conſtructed anſwering to the foregoing principles of *Fig.* 18. I drew a figure of it, wherein I propoſed the lever *AD* to have terminated in two arches of circles made out of one piece of braſs; the ſmaller arch formed on the radius *a A* to be loaded with a quantity of metal ſufficient to overcome all the friction of the ſeveral parts, ſo as to preſs down with a conſiderable weight, and always to reſt upon the point *A* of the upright rod *AB*; at *a* the axis, on which they were to turn; and the larger arch formed on the radius *a D*, was propoſed to be a ſextant, the outward edge of which was to be toothed, which teeth were to play into the teeth of a ſmall braſs wheel carrying a ſteel *index* like the minute-hand of a clock, which ſmall wheel with the hand was to make one revolution nearly by the utmoſt riſe and fall of the ſextant at *D*; or, inſtead of teeth, I propoſed a piece of a watch-chain to be faſtened to the upper limb of the ſextant, and ſo to be brought downwards, and paſſing nearly round the ſmall wheel in one groove, to be faſtened to it: in another groove in this ſmall wheel was another piece of watch-chain to be faſtened, which, being paſſed contrarywiſe round the ſaid wheel, was to have a weight hung to it that would be a counterpoiſe to the ſextant; but, upon conſulting my two ingenious friends Mr *G. Graham* and Mr *John Ellicot*, they each of them perſuaded me to lay aſide that more complexed conſtruction, and to have the inſtrument made in the plain and ſimple manner in which Mr *Jackſon* executed it for me in the year 1736, as is repreſented in *Fig.* 19, and 20.

The deſcription of the inſtrument. Fig. 19.

Fig. 19. *A B* a round rod of ſteel or braſs $\frac{1}{4}$ of an inch thick, and 3 feet 1 inch long: when the rod is of braſs 3 feet long, the point *A* muſt be of ſteel 1 inch long, to prevent its wearing away, or loſing its point; which conical point is made to ſcrew on and off.

I had the firſt rods made 1 $\frac{1}{2}$ inch thick at *B*, and of the ſame thickneſs 6 inches up; but I found inconveniences from that form, and that a rod all of a ſize was better.

CD, *c d*, are two iron ſupporters, joined by a flat croſs bar at bottom *D d* two inches long, in the middle of which is a point $\frac{1}{8}$ inch high under *B*, which goes into an hole at the bottom of the rod *B*, and ſerves to keep the rod in its place at bottom, as the croſs bar ** having an hole in it, thro' which the rod paſſes, does in the middle or about $\frac{2}{3}$ up the ſupporters, and the point *A* goes into a ſmall hole in the under

ſide of the lever; all which keep the upright rod firm and ſteady in its place. The iron ſupporters are flat, or parallel to the front of the machine from *C* to *X* and *c* to *x*, where they are twiſted half-round, ſo that the lower parts *XD*, *x d* ſtand at right angles with the upper parts. This contrivance gives the freer acceſs to the rod for the ſand or fluid into which the machine is ſet to meaſure the heat of it, the ſupporters ſtanding 2 inches aſunder at *D d*; and that the degrees of heat may be compared uniformly in different experiments, the bottom of the rod muſt always be immerſed to the ſame height in the matter to be examined; and therefore I make a mark, a ſmall furrow † quite round the rod, $1\frac{1}{2}$ inch from the bottom *B*. For the deeper the rod is immerſed into any matter, it will be lengthened the more by the ſame degree of heat.

E F, the lever, which turns upon an axis *G*. At *F* is faſtened a ſtring, which, paſſing twice round the ſmall pulley *H*, has a weight *I*, hanging to the other end of it, of about half a pound, being enough to keep the ſtring always ſtretched. At the other end *E* of the lever is hung another weight *L*, which muſt be heavy enough not only to counter-balance the longer arm *G F*, but preſs down upon the point *A* with a weight ſufficient to keep it ſteady.

MNO, is the back part of the plate, like the dial-plate of a clock made of braſs. See the front of it at *Fig.* 20,

The pulley *H* turns upon an axis *C* in *Fig.* 3. which goes thro' the plate, and on the other ſide or front of the dial-plate carries a hand or index *A B* in *Fig.* 20.

N. B. *G* being the *Fulcrum* of the lever, the diſtance *G A* being very ſmall, and the diſtance *G F* being very great, the ſmalleſt motion at *A* will produce a very great one at *F*, and therefore the index will turn very ſenſibly upon the plate.

The proportions of the rod and lever are diſcretionary; my rods both of ſteel and braſs are 3 Feet long in one ſolid piece, but they have each a point or cone of ſteel 1 inch high, that ſcrews upon the top at *A*. The lever has 4 inches from *E* to *A*, $1\frac{1}{5}$ inch from *A* to *G*, and 12 inches from *G* to *F*; the diſtance of *G* above *c* is $1\frac{1}{2}$ inch, the braſs pulley *H* is $\frac{1}{2}$ inch diameter; all the other parts of the machine are of oak. The main ſupport or pillar *P Q* is 1 inch ſquare, $2\frac{1}{2}$ feet high, and at bottom is let thro' a groove at *Q* made in a great heavy block or pedeſtal of wood *R S*. In this groove the pillar may be raiſed higher or lower, in order to adjuſt the height of the pillar to the ſituation, which the bottom of the rod *A B* may require in different experiments; and it is to be fixed in that place by a ſcrew at *T*, which goes thro' the front of the block, and preſſes againſt the bottom of the pillar.

Fig. 20. repreſents the *dial-plate*, or front of the plate marked *M NO*: in *Fig.* 19. it is a plate of braſs, with ſtrong paper glued upon it and may be of what ſize you pleaſe; mine is 11 inches over.

A B

AB is the hand or *index*, which ſlips on very ſtiff upon the axis *C*, that carries the pulley *H* in *Fig.* 19. The outer circle is to be left wide enough to contain the chemical characters or marks which are to be made upon it, the arch *DE* contains the diviſions of *Fahrenheit*'s mercurial Thermometer; the arch *FG* thoſe of *Reaumur*, or the ſpirit of wine Thermometer.

In order to adjuſt this inſtrument for uſe I place the bottom of the rod *B* in *Fig.* 19. immerſed up to the mark † in cold river or rain-water, in a veſſel proper to be ſet over the fire; and when it has boiled for $\frac{1}{4}$ of an hour, I turn the index *AB* in *Fig.* 20. till it ſtands in the horizontal poſition, as at *B*, being the point of boiling-hot water, and which anſwers to diviſion 212 on *Fahrenheit*'s arch. I then take it out of the water, and dry it, by holding it a little over the fire: and now great care muſt be taken, that nothing alters the ſituation of the index upon the axis; even a nut to ſcrew on upon the axis at *C* may be the beſt to keep it fixed. If the inſtrument be left to cool in the air, the index will fall below *B*, ſhewing the degrees of cold, or leſs heat than boiling water; and if put into melting tin, lead, *&c.* it will ſhew the degrees of heat above boiling water. A braſs rod will ſerve for an inſtrument to meaſure the greateſt degrees of cold, and all the degrees of heat, to the melting of ſilver or gold; but if you have a mind to make one to meaſure greater degrees of heat, the rod muſt be of ſteel, or the fineſt iron. A rod of braſs, according to Dr *Muſſchenbroeck*'s experiments, *l. c.* was found to lengthen 377, when one of iron lengthened only 230 parts. An iron rod, being regulated by boiling water, as above directed, will meaſure not only the heat of melted tin and lead, but of ſilver, gold, and copper, and will even ſhew the degree when iron itſelf begins to melt, which will be the greateſt degree of elongation of the rod juſt before the bottom of it runs; and I imagine, that an inſtrument may be conſtructed with ſupporters, and a rod made of tobacco-pipe-clay, which, being regulated by boiling mercury (for it muſt never touch water,) may be adapted to meaſure ſtill greater degrees of heat, till the materials themſelves melt into glaſs.

I ſhould adviſe, that not only the ſcale of this ſort of Thermometer, but likewiſe of all others, be determined by experiments, without regarding any equality as to meaſure between the diviſions, and that in every individual that ſhall be made; for a difference in the length and thickneſs of the rods in this ſort will make a difference in the ſcale, as much or more than the inequality in the cavity of the ſtem, or glaſs tube of other Thermometers, which can never be juſt, if applied to a ſcale whoſe diviſions are made equal; unleſs the cavity of the ſtem, be perfectly equal; which it is impoſſible for any workman to undertake to do, and which is very ſeldom, if ever, hit on by chance. Therefore in theſe inſtruments, let the point *B* in *Fig.* 20. or the horizontal poſition of the index, be the ſituation of the index when the rod has ſtood $\frac{1}{4}$ of an hour in boiling water; there mark ▽ *boiling* on the outer circle; on

Fahrenheit's arch mark 212. then ſet your machine up to the mark + into melting tin, which is the metal that melts eaſieſt. When the rod is arrived to its greateſt elongation in that metal, inſcribe the character ♃ on the outer circle; do the like with lead, and ſet the character ♄ at it. At the boiling of mercury put the mark ☿, and on *Fahrenheit*'s arch mark 600. the utmoſt extent his mercurial inſtruments can meaſure: Then proceed to the melting of ſilver, and ſet the mark ☽; at the melting of gold place the mark ☉; at the melting of copper place the mark ♀; at the melting of iron place the mark ♂, the moſt difficult to melt of all metals.

As the diviſions pointed out by the index will be different with rods of different metals or ſubſtances, you may make different circles upon the plate for the range of the different rods, and mark them; the iron rod, the braſs rod, the clay rod; and ſet the ſeveral marks above ſpecified upon each circle apart; or you may, to avoid confuſion, have a different inſtrument for each kind of rod.

Being obliged to take down my *athanor* and wind-furnace, upon removing twice to different houſes, and not having rebuilt them where I now live, I have not had opportunities yet of fixing the ſcale of my own inſtrument, which was one reaſon why I did not publiſh an account of my invention ſooner; for I hope hereafter to be able to compare the degrees of heat neceſſary for the melting of each metal, and to determine the queſtion whether metals in the higheſt degree of fuſion, are ſuſceptible of greater degrees of heat by increaſing the fire, as water thoroughly boiling can never be made hotter; nor did I intend to have publiſhed any deſcription of this inſtrument till I had compleated tables of the degrees of cold and heat, from *Fahrenheit*'s experiment of cold produced by art 40 degrees below 0 to the heat of melting iron.

According to *Fahrenheit*'s ſcale, the heat of the ſtrongeſt ſunſhine is at about 80. Spirit of wine boils at 176. Water at 212. the *lixivium* of ſalt of tartar at 240. Spirit of nitre at 242. Oil of vitriol at 546. Quickſilver at 600 *.

As all chemical digeſtions, where an equable heat is to be continued for ſome time together, will come in between hot ſunſhine and the boiling of quickſilver, a Thermoſcope of that range will be ſufficient for common uſes; and therefore one fitted with a braſs rod will anſwer theſe purpoſes.

In large furnaces for running down ores, or melting great quantities of metal together, it is not poſſible to place ſuch an inſtrument; but then in lead and tin there may be ſmall outlets contrived, into which ſome of the melted metal may be permitted to flow, and remain in contact with the ſame body of metal within, where the inſtrument may be placed; and for placing a Thermoſcope in iron, copper, or glaſs fur-

* *See* Auguſtin. Griſchow *Thermometria comparata accuratius, & harmonica.* Berolini 1740. 4*to.* p. 10.

naces,

naces, there may be a place contrived, which ſhall not open into the furnace, but have the thickneſs of a ſtone or brick left between, upon which the inſtrument may be placed; and tho' in ſuch a ſituation it will not meaſure the actual heat within the furnace, it will always give the relative or comparative heat in the like circumſtances at different times, and ſo ſhew us how to regulate the heat within.

Although a Chemiſt ſhall have one of theſe inſtruments to meaſure the heat, he ſhall have uſed in any experiment, and have noted down the ſeveral degrees made uſe of, and the time each laſted, he ſtill labours under another difficulty, which is the not being able to command any required heat, and that it ſhall laſt a certain required time, unleſs it be below that of boiling water, which may be procured and continued by various contrivances of lamps, either of ſpirits, or of oil; but how to continue a fire for 12 or 24 hours together, without attendance, which ſhall continually keep quickſilver boiling, lead in fuſion, or may be let down ſo low as not to exceed the heat of ſunſhine, and then be raiſed again, and that without letting out the fire, or moving the veſſels, may ſeem almoſt impracticable; but by an improvement of the furnace the antient Chemiſts called their *athanor*, I hope to ſucceed in it, which may be the ſubject of another paper.

The Rev. *Stephen Hales*, D. D. upon hearing the minutes of my paper delivered in to the *R. Soc.* on *May* 8. 1735. read upon the *Thurſday* following, deſired me to lend him the original for ſome days, telling me he had ſome thoughts of making a Thermoſcope with a rod of lead. After a few days he returned me my paper, with the following obliging letter, and kind remarks.

SIR,

I HAVE read over your *Thermometrical Tract* with ſatisfaction, and believe it will be of good uſe. The want of aſcertaining the degrees of heat and cold is a great and important *deſideratum* in Experimental Philoſophy.

What I intended to do was only this, *viz.* to get a leaden wire, of ſuch a ſize and ſtrength as to bear it's own weight, to have it as long as the longeſt gun-barrel I could procure, and to have it ſuſtain a lever as you have done; then to pour boiling water into the barrel, for a long time, till the lever riſes no more; the water to have vent at the bottom, yet ſo as to have the gun-barrel always full of water; the breech-pin to be out, and the leaden rod to reſt on a piece of wood ſet upright, according to the courſe of it's fibres, not ſideways.

To give at the ſame time to a mercurial Thermometer the heat of boiling water. Then to take the freezing point of the leaden and mercurial Thermometers; and afterwards to graduate all the intermediate degrees, from the mercurial Thermometer upon the leaden Thermometer, as they occur. Thus a ſtandard Thermometer may be made to

graduate

graduate others by; but I will not now ſet about it, ſince you have undertaken the ſubject.

His *Remarks* on the foregoing Paper.

1. *Thermometers* muſt be of excellent uſe in garden-ſtoves; but foreign plants muſt not be kept in an equal degree of heat in ſtoves, to that of their native country; *viz.* becauſe they cannot bear as great a heat in a confined cloſe air, as in an open free air. I have been told of coffee-trees being killed here in *England* by this miſtake: ſuch plants muſt doubtleſs be kept warm *, but not ſo warm as in their native country.

2. [*All ſolid bodies are obſerved to contract with cold*] I have found that wood does not contract or dilate lengthways with heat or cold. I am told that Mr *G. Graham* [is about making] this experiment, as I am alſo, in order to regulate *pendulums*.

3. I fear that *Boerhaave*'s wet linen, which is ſo thin, may begin to freeze before all the *mercury* or ſpirit of wine in the ball of the Thermometer has the ſame degree of cold: though hanging there long before and after freezing will bring it pretty near.

4. [*A rod* of iron 3 *feet long will have about* $\frac{1}{4}$ *inch increaſe*] or $\frac{1}{1\,4\,4}$ part.

A Letter from Maurice Johnſon, *Eſq*; Pref. *of the* Gentlemens Society *at* Spalding, *to* James Jurin, *M. D. F. R. Coll. Phyſ.* London, *and F. R. S. concerning a* Metalline Thermometer, *in the* Muſeum *of that* Society. No. 485. p. 128. Jan. 1747-8, *dated* Spalding, Jan. 16. 1747-8. *Read* Jan. 28. 1747.

3. As I know it muſt give you pleaſure, and, being by you (as I deſire it may be at their next meeting) communicated to the *R. Soc.* may be of ſome credit to the memory of the inventor, the late Mr *Sam. Frotheringham*, a Grazier at *Holbeach* in *Elloe Holland*, *Lincolnſhire*, and of ſome profit to the maker, give me leave to acquaint you, that he (Mr *John Ingram*, of this place, Watch-maker and White-Smith, whoſe father, originally a Black-Smith at *Cowbitt*, and inventor of the Machine for cutting watch-wheels, was alſo a moſt accurate artificer) having made, and, at my inſtance ſome time ſince, fixed up in our *Muſæum*, a metal Thermometer, which we, on experience and obſervation, found to anſwer and go truly, I propoſed to the company, at our laſt meeting in *Dec.* that our *Society* ſhould purchaſe it of him, I ſend you his deſcription thereof, as entered from his mouth in our minutes, which I truſt may be agreeable to you, and the worthy members of that illuſtrious body, for which we here have the higheſt honour: and though Mr *Beridge* (ſome time a Watch-maker at *Boſton*) under the ſame inventor's direction, made and carried up to town a machine ſomewhat of this ſort, which ſeveral of your members may have ſeen, yet I truſt this account may not be unacceptable.

* I ſhould think it beſt to leſſen the heat in ſtoves towards the night, and ſo to keep the plants expoſed to leſs degrees of heat a-nights than a-days, nay to vary the heat daily, or to endeavour by art to procure different degrees of heat, agreeable to the natural viciſſitudes of the climate the plants come from, having regard both to the ſeaſons of the year, and the ſtate of flowering or fructification of the plants; ſo that the beſt way of ranging plants in green-houſes or ſtoves is according to the climates they come from; for which Mr *Sheldrake*'s Tables above-mentioned muſt be of excellent uſe. *C. M.*

It

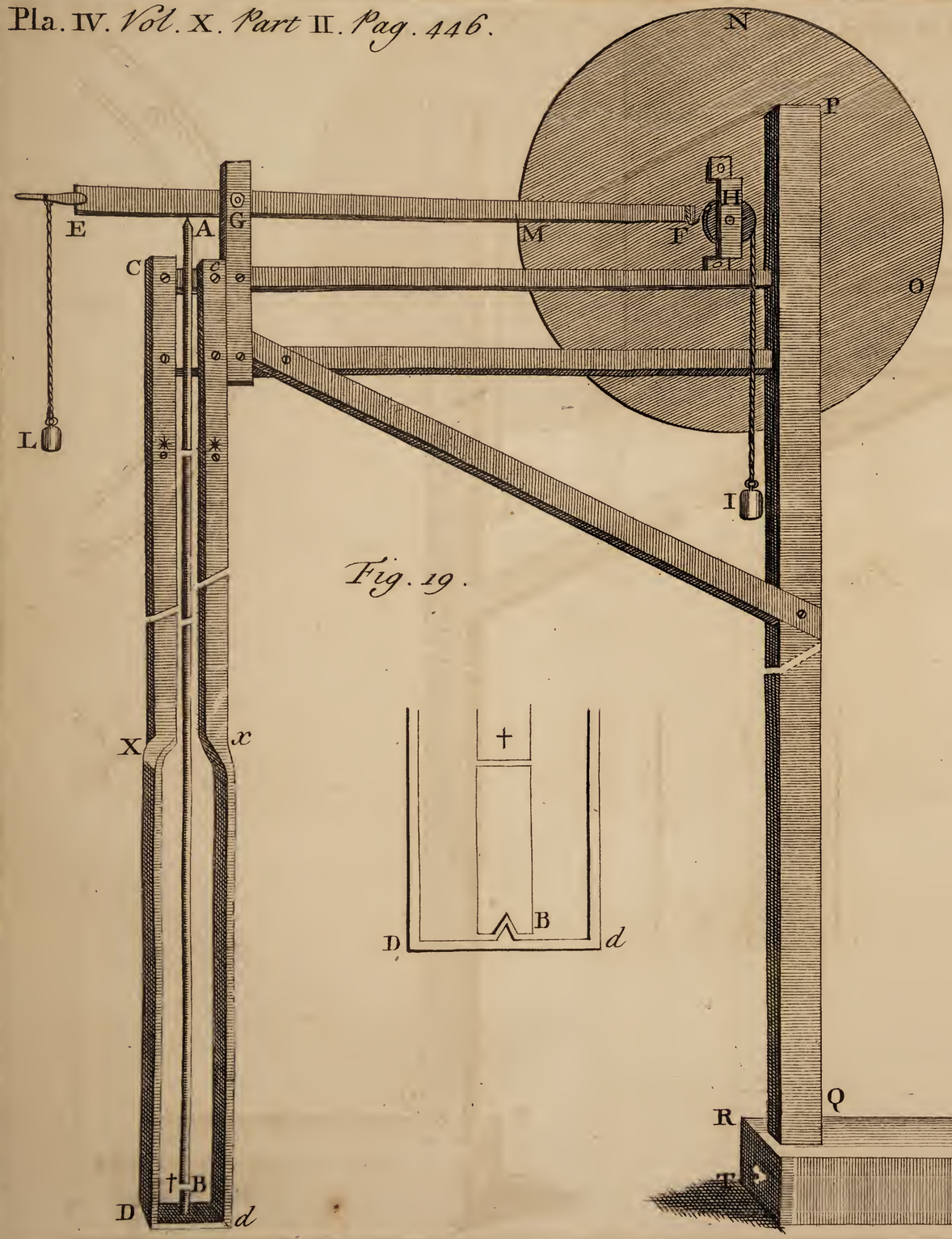


Fig. 19.

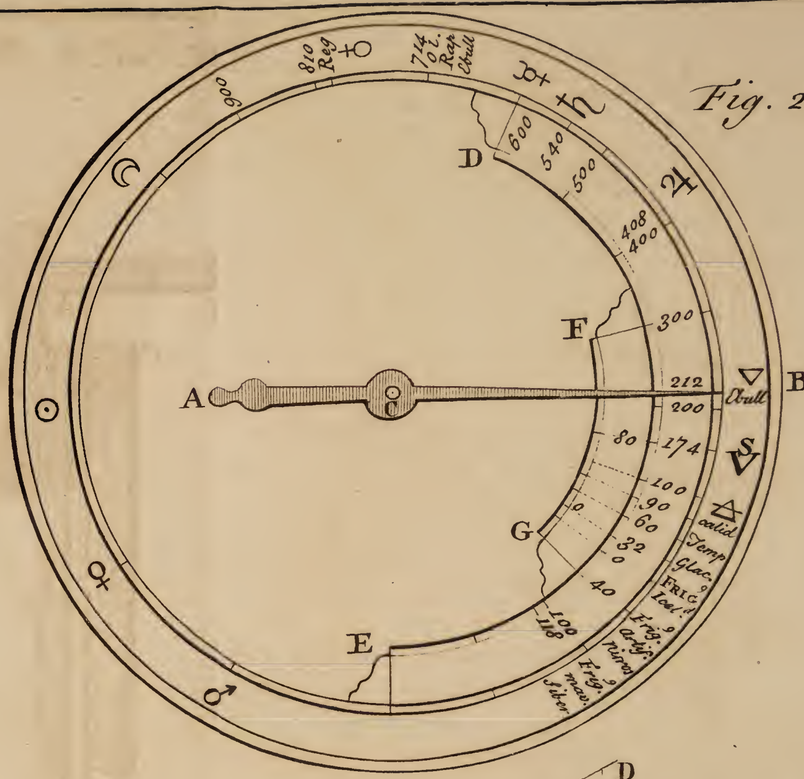


Fig. 20.

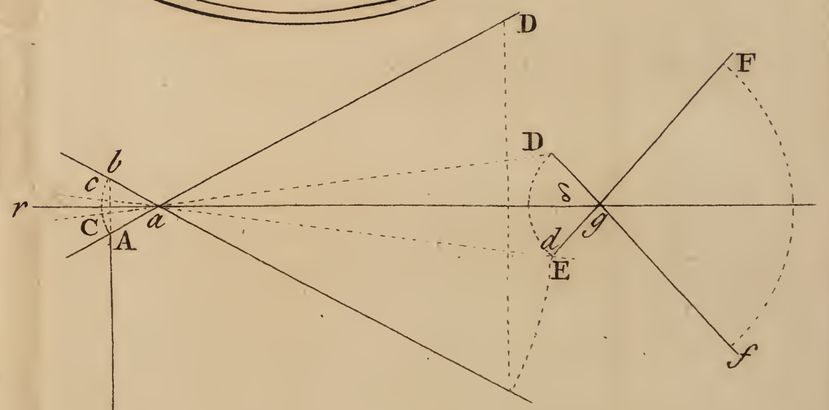


Fig. 18.

It is compoſed of an upright ſtaff or bar *a* of the beſt iron, 4 feet long, and 1 ¼ inch broad, having a poliſhed braſs bar of the ſame length and width ſcrew'd to it before it, with four ſteel ſcrews, and being alſo capped *b* with ſteel, and thereon a lever *c* moving upon a ſtud of ſteel, which communicates with another leſs lever *d* (alſo upon a ſtud) having a chain *e* at the end of it, which laps round an *axis f*, whereto the index is fixed, which ſhews the degrees marked on a ſemicircular arch *g*: Under the ſteel ſcrew-heads there are ſmall ſlits in the braſs bar (except the lowermoſt which is fixed) which admit of its expanding, whereby it protrudes and operates on the firſt-mentioned lever, which being raiſed moves the leſs lever, and thereby draws the chain which turns the axis affixed to the index, which ſhews the degree of warmth of the weather marked on the ſemicircular arch. At *h* is a ſcrew thro' two ſtuds, to draw the great lever backwards and forwards, as occaſion may be; *i* is a counter-balance to the ſmall lever to draw the hand back when the braſs bar ſhrinks *.

A deſcription of the Metal Thermometer *in the* Muſeum *of the* Gentlemens Society *at* Spalding *in* Lincolnſhire. Fig. 21.

4. It has been often complained of, that the theories we have of the air and weather, are ſo imperfect, and that an unfiniſhed one, of the hon. Mr *Boyle*, publiſhed ſince his death, ſhould be the beſt we yet have; perhaps there is equal reaſon for complaint, that the Thermometer firſt introduced into uſe in *England* by the ſame excellent philoſopher, ſhould be ſo little improved for more than half a century, and be made to ſerve a not much better purpoſe than that of amuſement.

For ſome years paſt, ſeveral eminent philoſophers at home and abroad, have applied themſelves to bring this inſtrument to greater perfection, and to render it more uſeful; and among them the great Sir *I. Newton* did not think it unworthy his attention. It ſeems now to be pretty generally agreed, that Thermometers made with quickſilver are preferable to all others; that *extravagant fluid*, as Mr *Boyle* calls it, being moſt eaſily ſuſceptible both of heat and cold, and, when well purified, not liable to be obſtructed in its motion.

I had, by ſome years experience, found both the excellence of them, and the neceſſity of keeping them in the open ſhaded air, before I met with the learned and curious eſſays medical and philoſophical of Dr *George Martine*, in which he ſo much recommends their uſe; and it

A Letter from the Rev. Henry Miles, *D. D. & F. R. S. to* M. Folkes, *Eſq; Pr. R. S. concerning* Thermometers, *and ſome obſervations of the* Weather. N° 491. p. 1. Jan. &c. 1749. *Read* Jan. 12. 1748.

* In the beginning of the year 1735. I invented, and cauſed to be conſtructed, a Thermometer on the ſame principles as this: I found that a rod of braſs 3 feet long was ſenſibly affected by the changes of heat of the weather, having one expoſed in my garden during the hard froſt of the winter 1739 and 40. And my inſtrument was very ſenſible with either a braſs rod or an iron rod, when the bottom of it was placed in a ſand-heat for chemical uſes; but I ſhall refer the reader to the preceding paper, wherein I have given a full deſcription of my invention, and the reaſons why I did not publiſh it before; tho' I have ſhewn the inſtrument to ſcores of people ever ſince *May* 1735. and ſent a deſcription and draught of it to M. *Buffon*, ſuperintendant of the *Royal Phyſick Garden* at *Paris* in the year 1744. in order for his laying it before the *Royal Academy* of *Sciences* at *Paris*.

C. M.

was

was no ſmall ſatisfaction to me, to find that Gentleman had proved, by experiments, that quickſilver both heats and cools faſter than any liquor we know; faſter, I am ſure (ſays he), than water, oil, or even ſpirit of wine, and never freezes, by any degree of cold hitherto obſerved.

Might I be indulged the liberty, I would embrace this opportunity of inviting ſuch gentlemen, as attend to this branch of Natural Philoſophy, to conſider what Dr *Martine* has ſaid to recommend the uſe of Thermometers made with quickſilver, and to place them in the open air, guarded from the ſun's rays; which, ſome obſervations I made *, may ſerve to ſhew the neceſſity of; eſpecially a more remarkable one, lately made, which I ſhall ſubjoin hereto.

There is another particular of great importance, which I fear we may rather wiſh than hope to ſee made a general practice, recommended by the ſame gentleman; that is, the conſtructing all Thermometers with one ſcale. But if this may not be expected, certainly no Thermometer ſhould be made without adjuſting two determinate and ſufficiently diſtant points of heat and cold; ſuch, for inſtance, as thoſe of boiling water, and of water juſt beginning to freeze, and the intervening ſpace divided into a convenient number of equal degrees. By this means we ſhould be able to know what is meant by any ſpecified degrees of heat or cold, and a compariſon might be eaſily made of the ſtate of the air in diſtant places, provided the inſtruments were accurately made.

Dr *Martine* ſeems to think, that the degree of cold which cauſeth water to begin to freeze, is nearly equal in all places, whatever little variation there may be found in that degree of heat which cauſeth water to boil, at different times, from the different weight of the atmoſphere: So that we may look upon theſe two points as ſufficiently determinate.

An account of an obſervation I made of the ſudden change of the temperature of the air on Tueſday *the 22d of* Nov. *laſt, with the ſtate of the Barometer, and other circumſtances.*

On *Monday* the 21ſt in the evening the ſky very clear, the wind N. and a ſmart froſt, the Barometer was 30 inches $\frac{2}{10}$ $\frac{0}{100}$. At near 9^h the Thermometer without my window at 7 *gr.* below 0, or freezing point. The Thermometer within, of the ſame conſtruction with it, and not a yard from it, (the room having had no fire in it this ſeaſon) at 5 *gr.* nearly above 0.

On *Tueſday* morning, at 4^h 20′, when I got up, I found the Barometer at 30. $\frac{2}{10}$ $\frac{6}{100}$ inch; the Thermometer without at 14 *gr.* $\frac{1}{2}$ below 0; that within at 2 *gr.* $\frac{1}{2}$ above 0. I was much ſurprized hereat, and before I had finiſhed my entry, I returned to renew my obſervation, fearing I might have made a miſtake, but found I had not: At 7^h 40′ the ſame morning, upon opening my ſtudy-window, I obſerved the ſky to look red and lowering; this induced me to go up to examine my glaſſes, ſuſpecting there might be a change, and found the Barometer fallen to 30. $\frac{0}{10}$ $\frac{2}{100}$, the Thermometer without riſen to 5 *gr.* below freezing point, but that within fallen to 1 *gr.* above; the wind getting

* See Art. 3. of this Section.

about

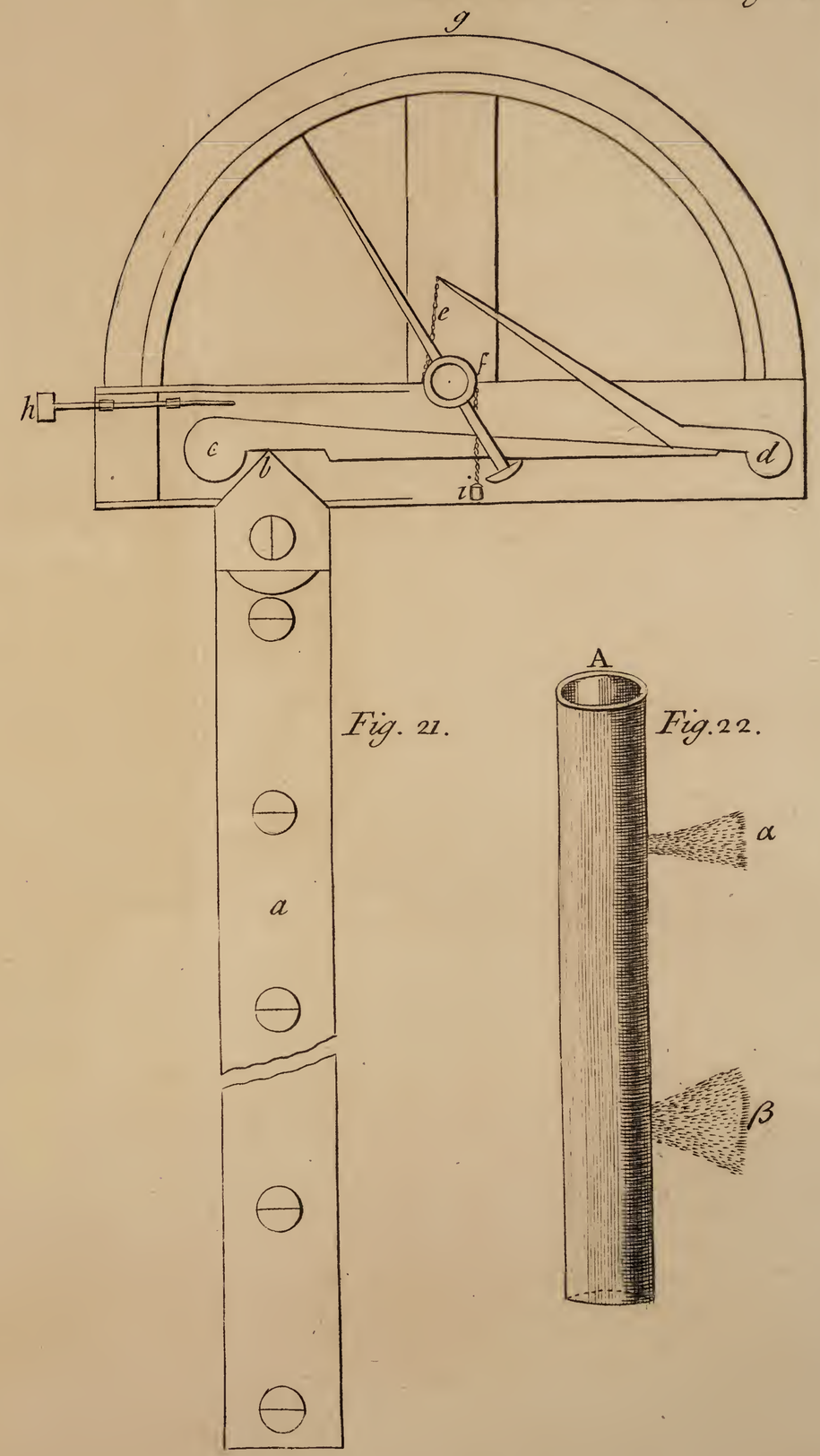


Fig. 21.

*Fig.*22.

to W. and S. W. and before 10 in the Morning we had ſome rain, and this ſevere froſt went off. At this laſt-mentioned hour the Thermometer without had riſen to 5 *gr.* above 0; that within continuing at 1 *gr.* as before. At $8^h \frac{1}{2}$ that evening the Thermometer without was at no leſs than 12 *gr.* above 0, that within at 3 *gr.* above 0: ſo that from that time I made my obſervation at 4^h 20′ in the morning to $8^h \frac{1}{2}$ at night, there was a change in the temperature of the air abroad of 26 *gr.* $\frac{1}{2}$; while the change within doors did not amount to more than $\frac{1}{2}$ *gr.* warmer.

It ſeems probable from hence, that we may have frequently had greater extremes of heat and cold by far, than have fallen under obſervation.

IV. It is well known, that the greateſt degree of heat in common water is that which it acquires by boiling; that is to ſay, if water is put upon the fire, it grows by degrees hotter and hotter, till it quite boils; but, after that, though there be never ſo much fire added, and it ſtand never ſo long upon it, it will never grow hotter than it was on the firſt inſtant, when it began to boil. Hence the degree of heat of boiling water is looked upon as fixed and invariable.

The heat *of* boiling water *varies according to the* weight *of the* air; *by* M. Secondat de Monteſquieu, *of the* Acad. *of* Sc. *at* Bordeaux. *In a letter to* M. Folkes, *Eſq*; P. R. S. N°. 472. p. 32. Jan. *&c.* 1744. *Read* Mar. 8. 174$\frac{4}{5}$.

Fahrenheit, that ingenious maſter in Mechanics, ſo well known by his mercurial Thermometers, is the firſt who has remarked the contrary. He obſerved, that the heat of boiling water was greater when the air was heavy (that is to ſay, when the mercury ſtood higher in the Barometer); and, on the contrary, the heat was leſs when the air was lighter.

Mr *Le Monnier* the younger, who has obliged us with a tranſlation of Mr *Cote*'s Lectures in Nat. Philoſ. with excellent notes upon the ſaid work, has put *Fahrenheit*'s diſcovery paſt all doubt, and has very much improved it.

Oct. 6. 1739, being provided with a Barometer, and a mercurial Thermometer of M. *De Liſle*, he climbed up to the higheſt top of the *Canigou*, a mountain in *Rouſſillon*, which paſſes for the higheſt among the *Pyrenees*: There he found his Barometer to ſtand at 20 inches 2 $\frac{1}{2}$ lines; whilſt at *Perpignan* it ſtood at 28 inches 2 lines. The difference between the heat of the water which he boiled there, and that which he boiled at *Perpignan*, was 15 degrees of his Thermometer.

The ſame Thermometer being ſurrounded with ſnow, the mercury fell down to the ſame degree as pounded ice had made it do at *Paris*. Hence he concludes, that the heavineſs of the air has a ſenſible influence on boiling water; but that it in no way alters the term of congelation. All theſe particulars may be ſeen, *p.* 408. of *Cote*'s Experimental Lectures; and in the Mem. of the Acad. of Sc. of *Paris*, 1740.

This is the ſame experiment which I have repeated on the top of the *Pic du Midy*; thinking that ſo ſingular a fact ought to be obſerved more than once.

I carried two Barometers, the tubes of which the Rev. Father *Francis* had been ſo good as to fill for me with great care. I had likewiſe with me 2 mercurial Thermometers, upon which I ſet the degrees at *Bagneres:* I took the fixed terms of the graduation; that is to ſay, that of congelation, and that of boiling water, afterwards putting nought to the term of congelation. I marked 180° difference between this term and that of boiling water.

Being come to the higheſt top of the *Pic du Midy* on the 9th of laſt *July*, the mercury roſe in one of my Barometers to 20 Inches 2 lines; and in the other, to 20 inches 1 $\frac{1}{2}$ line. I ſurrounded my Thermometer with ſnow, and the mercury fell exactly to the ſame degree as the ſnow had made it fall to at *Bagneres*. Afterwards I plunged it into boiling water; whereupon the mercury roſe to 165° of my graduation; ſo that the difference between the heat of boiling water on *Pic du Midy*, and that at *Bagneres*, conſiſted of 15°.

At my return to *Bordeaux*, I obſerved, that I had marked the term of boiling water at *Bagneres* leſs high by 3 $\frac{1}{2}$ than at the term of boiling water at *Bordeaux*, taken at the time when the Barometer was at 28 inches 2 or 3 lines: therefore having anew graduated my Thermometer the 165th, the degree of the former graduation fell now upon the 162^{d}; ſo that the complete difference between the term of boiling water on the top of the *Pic du Midy*, and that of the ſame at *Bordeaux* the Barometer being at 20 inches 3 lines, amounts to 18 degrees on the Thermometer of *Fahrenheit*.

Now the conformity between the obſervation made by M. *Le Monnier*, and this repetition of the ſame obſervation, can hardly be greater; ſeeing the heights of the Barometers are almoſt the ſame; and the 15 degrees of difference, found by M. *Le Monnier* on *De Liſle*'s Theremometer, amount preciſely to 18 degrees on the Thermometer of *Fahrenheit* which I made uſe of.

Of ſudden freezing; *by* Sam. Chriſtian Hollman, Prof. Pub. Ord. Philoſ. Gotting. N°. 475. p. 239. Jan. &c. 1745. *Read* Jan. 10. 1745.

V. The *phaenomenon* of Mr *Triewald* related in the Philoſ. Tranſ. ſeemed to me ſo extraordinary at firſt reading, that if I had not had ſome farther proof, that all congelations are performed almoſt in a moment of time, it would have ſeemed to ſurpaſs all belief. But I happened to meet with ſomething not very unlike it about the end of 1742, which being at firſt diſcovered by chance, I endeavoured to reduce to a ſure and certain experiment, both in that winter, and the following. About the end of 1742 Dec. 24. N. S. On the coming of a ſudden very great cold, there appeared in 2 conical glaſſes what they call Carteſian devils, of various form and colour, ſome ſpecifically lighter than the water, and others by a greater quantity of water admitted into their cavities, rendered heavier; ſo that being put into the air-pump when the air was exhauſted, they could aſcend in the ſurrounding water as it were of their own accord. I put both the glaſſes in a cold room unſtopped, and covered only with a glaſs phial;

phial; from which ſeeing the ſame on the morning of Dec. 24, I removed them immediatly into a ſtove, and ſet them on a walnut-tree cheſt, being ſtill full of a very fluid and tranſparent water. But as I had juſt recollected that glaſſes, and other ſuch like cold bodies, on being removed into a warm place, uſed to have a remarkable quantity of dew running down from the ſurface in ſtreams; being deſirous to to ſave my cheſt from being damaged, or at leaſt ſtained, I examined the paper which I uſe to lay under my glaſſes, and as I lifted up one of the glaſſes, I found it already covered with a great quantity of dew; but when I took up the other, in which the devils being heavier than water, ſunk to the bottom, I found it dry but quite full of ice. This unuſual and unexpected ſight greatly ſurprized me, and being ſollicitous about my little glaſs images, I took up a brazen tube which happened to be at hand, in order to ſave them, if poſſible, from the ice. But when I found the cakes of ice to be ſurpriſingly interwoven with each other, and to be very thin and ſoft, and that the whole icy cone did not cohere with the glaſs, but only in a ſmall part, and that what was at the bottom of the glaſs, to the thickneſs of an inch, ſwam in the water and began ſenſibly to diſſolve into water at the ſides, I began to enjoy this pleaſing ſpectacle with more ſecurity and delight. All the thin plates of ice, which conſtituted that whole truncated cone, were very thin, and as tranſparent as glaſs, and by their various inclinations, when the whole glaſs was turned about, they ſhone ſurpriſingly like tables of glaſs; and this delightful ſpectacle was not a little increaſed by thoſe which ſtuck about the head of the black Carteſian devil.

I was willing to make uſe of this opportunity to try, whether on melting the ice again, and putting the glaſs again in it's former place, the ſame *phaenomenon* could be again procured. But when I firſt read the experiment of Mr *Triewald*, I was of opinion, that by the preſſure of the bladder tied about his glaſs, the degree of preſſure ariſing from cold was perhaps increaſed, and that thereby that ſudden effect of congelation was produced, and ſo I hoped that by the ſame way, it would alſo ſucceed with me in water ſufficiently cooled.

It would be tedious to relate all the experiments that I made on Dec. 24, 26, and 27, and repeated often with much trouble. I confeſs the experiment did ſometimes ſucceed; but then I was more often fruſtrated of my hope, tho' all the circumſtances were the ſame. The *whole* glaſs [*a*] was ſometimes filled with ice, as I was looking upon it; ſometimes there was a ſort of *icy bladders* [*b*], when I was preſſing the bladder of the glaſs, that roſe to the ſides of the glaſs, which preſently turned into an icy cruſt, that incompaſſed the whole inner ſurface of the glaſs; whilſt the remaining inner maſs of water cloſely ſurrounded the *axis* of the glaſs, and remained fluid; ſometimes *very tranſparent ſingle* thin plates [*c*], almoſt reſembling the form of *ſnow*, appeared in the middle of the clear water, as I was holding the glaſs in my hand, in viewing it in the full light of day, and theſe were ſo

very thin, that on any little ſhaking of my hand they would fall into the water and tremble. But there would be no end, if I ſhould relate all. I will therefore, as briefly as I can, relate only the principal experiments which were often and diligently repeated.

1. When the water had conceived a ſufficient degree of cold, whether the glaſs was removed into a warm place, or was only taken in a warm hand, almoſt the whole water would in a moment be turned into ice. But this happened more often and more eaſily in the former caſe than in the latter.

2. It did not ſignify whether the glaſs was covered with a bladder or not; and in the former caſe, whether the bladder was preſſed with the fingers or not. Nor,

3. Did I find it to ſignify, whether any Carteſian devil, or any other like image was in the glaſs or not; and if there was one, whether it kept at the bottom of the glaſs, or ſwam on the ſurface of the water.

4. When there was any little image in the water, as often as I was able to obſerve the beginning of freezing, it always began from ſome part of it, and thence diffuſed itſelf on all ſides.

5. The experiment ſucceeded according to my mind, only when the glaſs being full of water was ſo expoſed to the cold air, that the whole maſs of water could be equably penetrated thereby. But if,

6. The glaſs was placed before an open window, which admitted the cold air, or on a plaſtered floor, which is uſual here, there was ice indeed generated, but ſuch as I deſcribed above at [*b*]. But if,

7. The glaſs was placed before an open window, where the wind did not blow, on a boarded floor, or on a wooden plank laid over the plaſter, the experiment generally ſucceeded: unleſs perhaps I came a little too late, and the glaſs was already wholly filled with ice. But,

8. That I might more eaſily obſerve the degree of cold, to which the water ought to come, if the momentaneous freezing of it was to happen in a warm place, I filled a broader earthen veſſel with water, and ſet it on the ſame baſe with the glaſs; and obſerved carefully, when the water contained therein began to ſkin over. And tho' theſe congelations did not happen exactly at the ſame time, yet this obſervation often contributed to the better ſucceſs of my experiment. For it is a very troubleſome and tedious experiment, without it. But perhaps,

9. The conical figure of the glaſs, on account of the narrow orifice and broad baſe, does not a little contribute to the force of the cold and heat, which is to be equably diffuſed thro' the whole maſs of water. At leaſt I am perſuaded, that the experiment will ſucceed with more difficulty in any cylindrical veſſel, or account of its orifice being too broad; tho' I have not yet tried it.

The weather did not afterwards give me any opportunity of repeating

ing the ſame experiments that winter. But I repeated them in *Jan.* 1744, with the ſame ſucceſs, and with the ſame *phaenomena* as above.

I will not preſume to aſſign the cauſe, why the water cooled almoſt to the point of freezing, as it were in one moment, turns into innumerable icy plates, croſſing each other in a wonderful manner, and forming one continued body, if the glaſs in which it is contained is ſurrounded by a *ſudden heat*. The *Ariſtotelians* perhaps would here pleaſe themſelves and others with their word *Antiperiſtaſis*. But I am not deſirous to increaſe the number of empty words; and ſhall content myſelf with having related the *phaenomena*, and ſubmitting to the judgment of the *Royal Society*.

Deſcription of an improved Hygroſcope. *From Mr* Will. Arderon, *F. R. S. to Mr* H. Baker, *F. R. S.* N°479, p. 95. Mar. & Apr. 1746. *Read* Feb. 27. 1745-6.

VI. Mr *Boyle* has taken much pains to bring the Hygrometer to perfection; and Mr *Pickering* has lately made an improvement to it: But, as the inſtrument I uſe differs from them both, I ſhall beg leave to deſcribe it to you.

Some years ago I applied my thoughts to conſider the nature of Hygroſcopes, and compared many different ſorts together, in order to determine which I might employ with the greateſt certainty; when none appeared to me to come nearer the truth than that recommended by Mr *Boyle*, of weighing a piece of ſponge in a pair of gold ſcales. But the difficulty and time, which I found, upon trial, were requiſite to adjuſt the weights, and diſcover the true ſtate of the air, ſet me upon contriving another method, whereby at all ſeaſons I might perceive, by inſpection only, the moſt minute alterations with reſpect to moiſture or dryneſs; and the following drawing will, I believe, ſufficiently deſcribe what I found moſt effectual for that purpoſe.

A repreſents a thin piece of ſponge, ſo cut as to contain as large a ſuperficies as poſſible. This hangs by a fine thread of ſilk, upon the beam *B*, and is exactly balanced by another thread of ſilk at *D*, ſtrung with the ſmalleſt lead-ſhot, at equal diſtances, and ſo adjuſted as to cauſe the index *E* to point at *G*, in the middle of the graduated arch *F*, *G*, *H*, when the air is in a middle ſtate between the greateſt moiſture and the greateſt dryneſs. *I* ſhews a little table or ſhelf, for that part of the ſilk and ſhot which is not ſuſpended, to reſt upon. Fig. 23.

Improvement of the Weather-cord. *In a letter from Mr* Arderon, *F. R. S.* *to Mr* Baker, *F. R. S.* N°. 479. p. 169. Mar. *&c.* 1746. *dated* Dec. 21, 1745. *Read* Apr. 24. 1746.

VII. The Weather-cord is an Hygrometer of a very ancient invention, and, if properly conſtructed, may be made uſe of with very good ſucceſs, to ſhew the various alterations of the atmoſphere, in reſpect to moiſture and dryneſs; but, as commonly made, it never riſes or falls ſufficiently to point out ſuch minute changes as the curious would be deſirous to know. A ſenſe of this defect ſet me upon endeavouring to find out ſome method of removing it; and how far I have ſucceeded, will beſt appear upon caſting your eye upon the drawings, *Fig.* 24. and 25.

In

Fig. 24. In the firſt of theſe Hygrometers which I made, as in *Fig.* 24. I only fixed the end of the index *A B* faſt to the ſilk *C E* at *A*, leaving it lying looſe upon the point *D*; and in this manner the other end of the index would nearly deſcribe the arch *F G H*: But then I ſoon perceived, that the centre of motion, whereon the index turned, was changed whenever it moved ever ſo little; and, conſequently, that the arch ſtruck by the end *B* muſt be irregular.

Fig. 25. On conſidering this, I toothed two pieces of braſs, as 1, 2, and 3, 4, (*See Fig.* 25.) to fit each other ſo exactly, that, upon the leaſt motion of the one, the other would move; then, fixing the index upon the centre *C*, it's motions were rendered much more regular.

I placed likewiſe a little collar of braſs at *B*, upon the cord *S R*, and to that collar tied the ſilk, which gave motion to the index, that the cord *S R* might twiſt and untwiſt without any impediment.

If there is no weight placed at bottom, as in *Fig.* 24. X the piece of braſs 1, 2 muſt be ſo heavy as to keep the cord *S B R* at a convenient tightneſs, and alſo to counterbalance the end of the index *C E*, provided it be heavier than the other.

The length of the cord *S B R*, it's thickneſs, and the manner of preparing it, are already deſcribed in ſo many books which treat of Hygroſcopes, that to mention them would ſeem unneceſſary.

An Hygrometer made of a Deal Rod; *by the ſame.* N°. 480. *p.* 184. May *&c.* 1746. *Read* May 8. 1746.

VIII. In *Oct.* laſt I contrived and made an Hygrometer; the firſt hint whereof I received from obſervations on the ſwelling of deal doors againſt rain. I perceived this wood expanded itſelf very conſiderably, laterally, or acroſs its grain: and this I imagined, if properly made uſe of, might ſhew, not badly, the different degrees of moiſture or dryneſs in the air. Theſe thoughts ſet me upon ſearching the *Philoſ. Tranſ.* to ſee if any ingenious perſon had recorded his opinion upon this ſubject: and I found * that an anonymous author had made ſeveral attempts to conſtruct Hygrometers of deal boards †; and again ‖, that Mr *J. Coniers* had added ſome improvements thereto; but, as the method taken by theſe two gentlemen ſeemed liable to ſome objections, I determined to make a trial on a plan and form intirely different from theirs; and have been ſo fortunate to find it ſucceed greatly beyond my expectations.

My way was thus: I procured a piece of coarſe deal board; moſt of it, if not all, ſap. From this I ſawed 7 pieces croſs the bate or grain, 10 inches long and an inch broad; and as the board was juſt an inch in thickneſs, I thereby conſequently obtained 7 parallelopipeds of an inch ſquare each.

* *See Vol.* II. *Chap.* I. § xvi. 1.

† This author ſays, Poplar would do much better; but of that I have had no trial.

‖ *See Vol.* II. *Chap.* I. § xvi. 2.

Theſe

Theſe 7 pieces of deal I joined together, lengthways, with ſtrong glue; which made a ſquare rod of 70 inches long. I found it neceſſary to place theſe ſmall pieces in ſuch a manner, when I glued them together, in reſpect to their grain, as is repreſented in the two figures annexed, to prevent their forming themſelves into a ſort of curve; which they naturally do, if they are placed all the ſame way; and I found myſelf obliged to fix the rod in ſuch a number of brackets as appear in the drawings, in order to keep it ſtrait.

I placed this rod, at firſt, perpendicular to the horizon, betwixt two pieces of wood of the ſame thickneſs, and nailed againſt the cieling of my room; but then I had one ſide only expoſed to the air: however it acted tolerably well, which encouraged me to try to make it more perfect; as you will find delineated, *Fig.* 26. and 27. Fig. 26, 27.

Both theſe deal rods were placed againſt the cieling of my room with brackets, and were buttoned down into ſquare mortiſes in each bracket with ſmall pieces of deal, that fitted their tops exactly. Hereby all their 4 ſides became expoſed to the air; and the only difference between them is, the increaſing the effect of their variation by two different methods.

To the rod at *Fig.* 26. I added 2 levers: The firſt of which *A B D* had it's ſhorter end *A B* but 3 inches in length, and it's longer *B D* 12; conſequently the end *D* moved through 4 times the ſpace that the end *A* did.

The ſecond lever *E F G*, I fixed to act with the other before mentioned. The ſhorter end *E F* of this lever was 3 inches, and the longer end *F G*, 45 inches; whereby the effect of the other lever was increaſed 15 times, and that of the deal rod 60 times. So that if the rod lengthens but one tenth of an inch, the point of the lever *G* moves 6 inches; and if the rod lengthens but one inch, the point *G* moves 60.

The longer end of the ſecond lever in *Fig.* 26. muſt be made ſo much heavier, that it may move down freely by its own gravity whenever the bar ſhortens.

To this Hygrometer I fixed a ſmall index, ſuch as is common in Mr *Hauksbee*'s Barometers, to ſlip up and down on a wire, as is repreſented at *K*.

Fig. 27. repreſents another method I employed to increaſe the power of the deal rod. This may be fixed in a much ſmaller compaſs, and yet is no leſs capable of ſhewing the minute differences in the moiſture or dryneſs of the air than the other before deſcribed.

The deal rod in this was managed and faſtened in the ſame manner as was ſhewn before. I likewiſe applied a lever *A B D* to the top thereof, exactly of the ſame dimenſions as in the other; but, inſtead of a ſecond lever, I placed a graduated circle, with an index thereto like that of the minute-hand of a clock. This I fixed to a ſmall *axis*, which was moved one way by a ſilken thread wrapped twice or thrice round it, whereof one end was tied to the longer end of the lever at *D*, and

the

the other way by the gravity of the weight *W*. And here, if the length of the index *R S* be 15 times as long as the ſemidiameter of the *axis* which the ſilk turns upon, it is evident that our ſenſibility of the rod's alteration will be increaſed 60 times, *&c.* The deal rod is ſtrongly nailed down at *N*, both in the firſt and ſecond draught; but, in all other parts, they have free liberty of contraction or dilatation.

Unleſs I am too much prejudiced in favour of this Hygrometer, it far exceeds all that I ever ſaw; and I may take upon me thus far to aſſert, that, by acting 10 or 12 hours before the apparent change of weather happens, as this has done ſince I got it finiſhed; it may very much aſſiſt to form a true judgment on the impending changes, when the wind is in or near the E. or W. point, when the Barometer is of little or no ſervice.

I obſerve, that heat and cold have a conſiderable power of lengthening and ſhortening the deal rod, as well as the moiſture and dryneſs of the air; and this, at firſt ſight, would induce one to imagine, that it would thereby be rendered almoſt uſeleſs; but it is really far otherwiſe; for, by placing it near a Thermometer, it is eaſily rectified with reſpect to its expanſion or contraction, by heat or cold, at the ſame time that it truly ſhews the various degrees of moiſture or dryneſs in the air. In ſhort, it is an inſtrument made very eaſily, of materials to be got almoſt every where, and of little coſt. It is capable of being ſerviceable either by ſea or land, and may be placed in any direction.

I have lately finiſhed one of theſe Hygrometers, the bar whereof is but a foot in length, yet, by making the two levers of metal, I eaſily make the end of the ſecond lever riſe and fall 8 inches. At this ſize it becomes portable, and anſwers the end or purpoſe as well as the other.

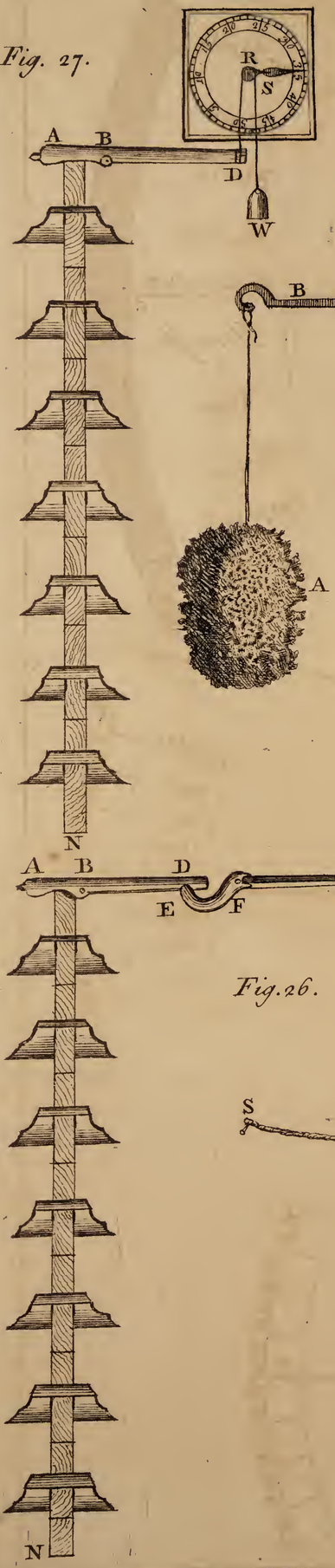
P. S. Since my writing this, I have been turning over a little treatiſe of Mr *Boyle* upon the Hygroſcope, wherein I find he had made ſeveral trials with different ſorts of wood; but then they were turned into the ſhape of bells; which, he ſays, anſwered very well, by comparing their weights: That their different weight increaſed or diminiſhed, according to the different degrees of the air's moiſture; and that he had taken notice of the expanſion of the wood in doors and door-caſes: but he proceeds no farther than to recommend the different kinds of wood to be examined by their weight.

Scheme *of a* Diary *of the* Weather; *together with draughts and deſcriptions of* Machines *ſubſervient thereunto*; *by Mr* Roger Pickering, *F. R. S.* N°. 473. *p.* 1. May *&c.* 1744. *Read* May 3. 1744.

IX. A ſenſe of the importance of obſerving the weather induced *Hippocrates*, in his remarks upon the Epidemic Diſeaſes in *Thaſos*, to premiſe a general hiſtory of the weather preceding them; and with the ſame view did our great Mr *Boyle* turn his thoughts ſo cloſely upon the ſame ſubject: whoſe example, being followed by ſeveral judicious inquirers into nature, both abroad and at home, has brought the *Natural Hiſtory* of the *Air* to a ſurpriſing degree of perfection, beyond what the Antients ever could pretend to, or even thought of. Had but each county in *England* gentlemen of ſuch ſentiments, who would charge

themſelves

Fig. 27.



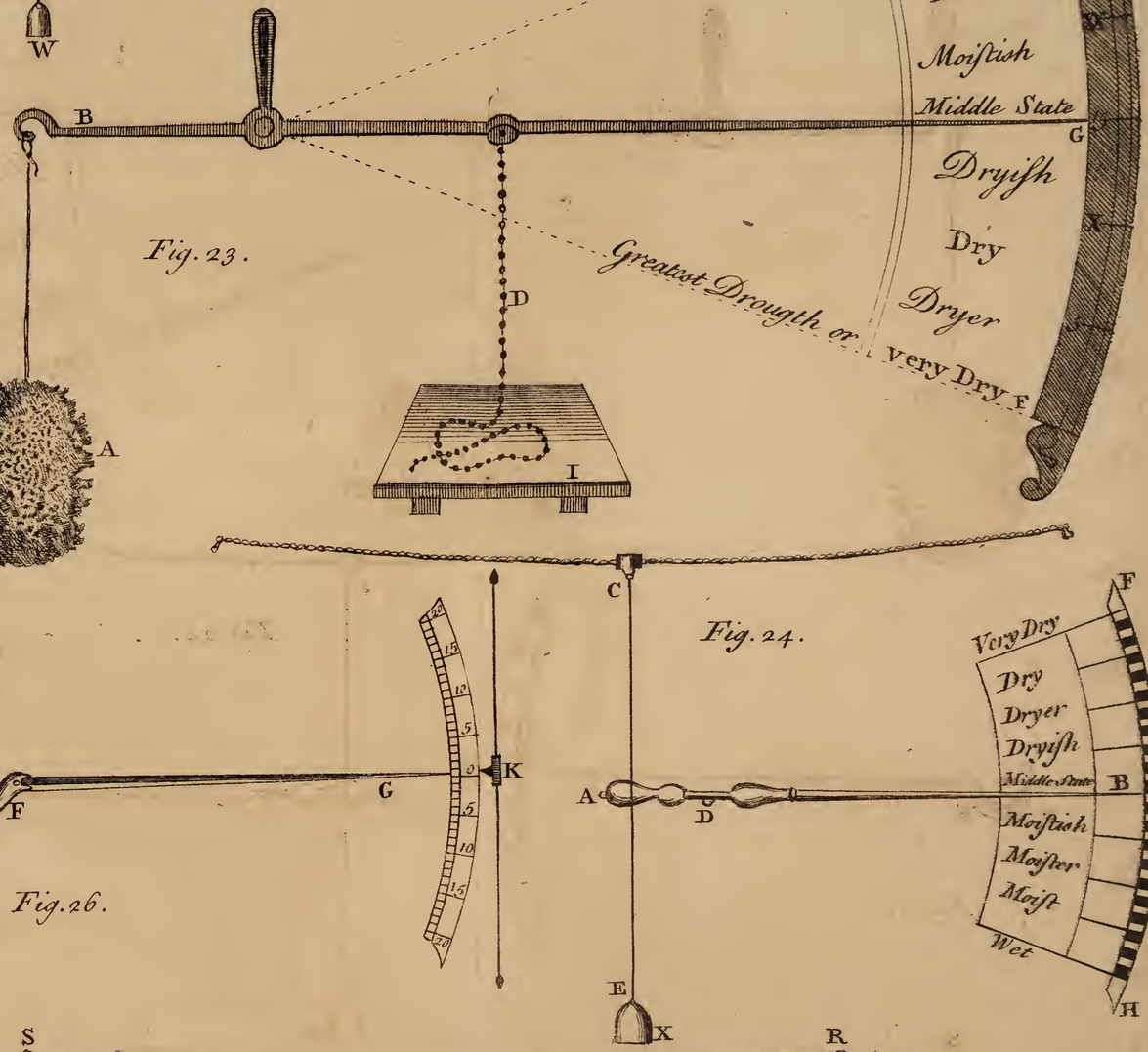


Fig. 23.

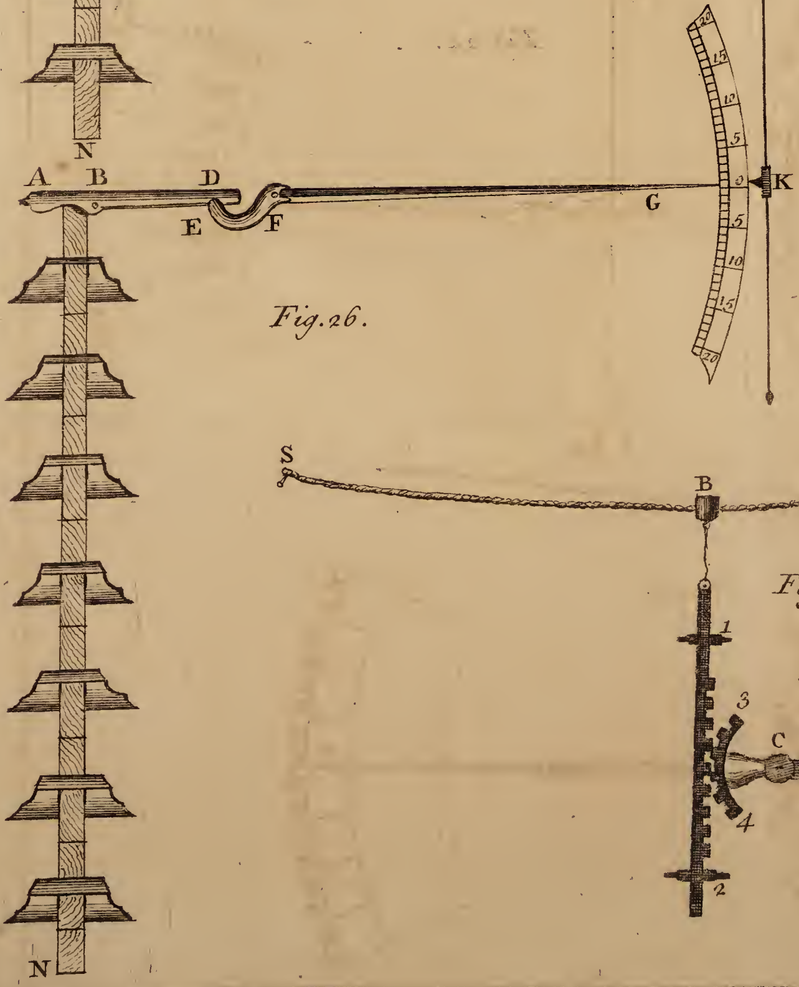


Fig. 26.

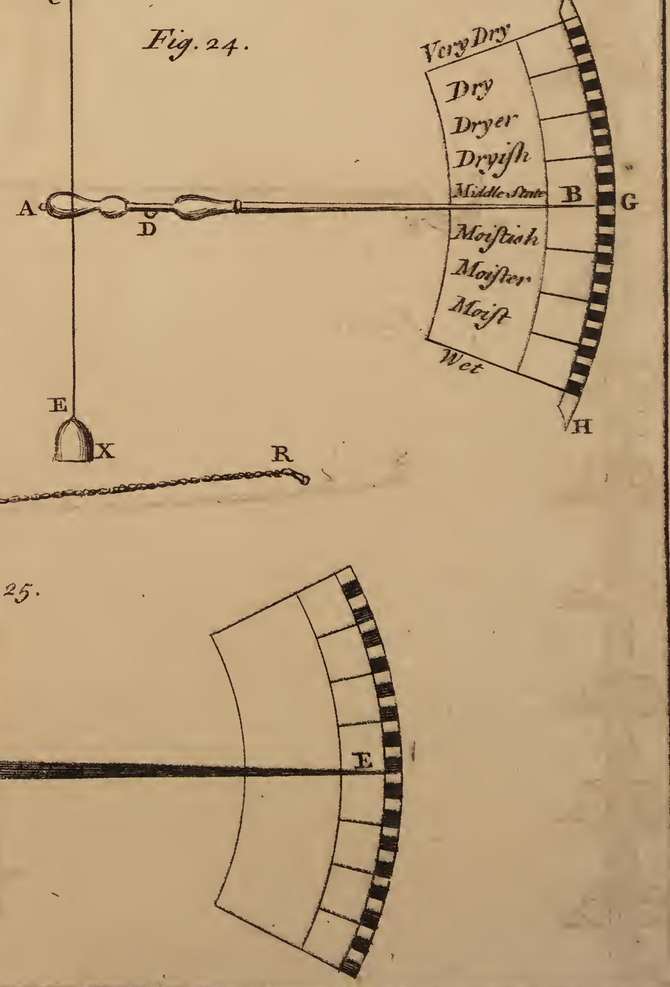


Fig. 24.

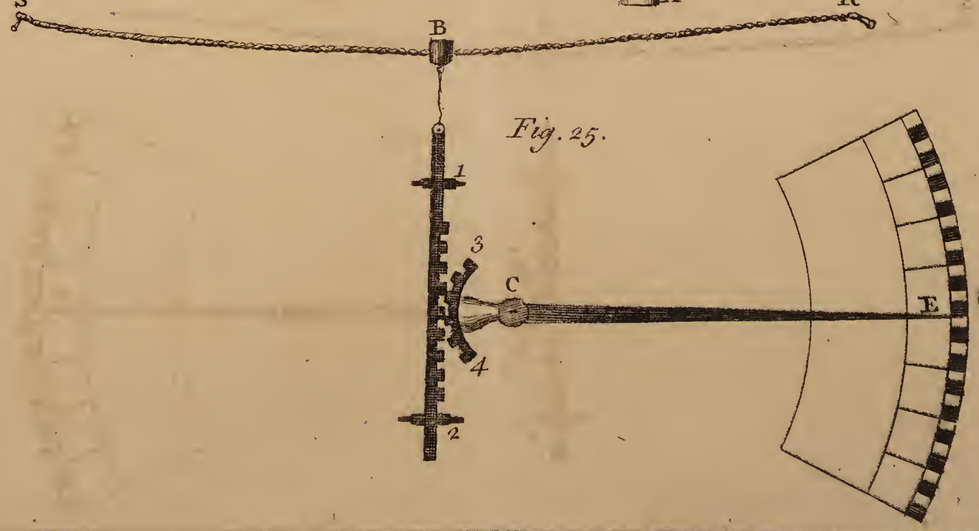


Fig. 25.

themſelves with the annual trouble of ſending a regular account of the *weather* to this *learned body*, by it to be compared and digeſted, to what degrees of accuracy may we not ſuppoſe a knowledge of the nature and affections of the *atmoſphere* may be brought; and how well may we not hope to be guarded againſt the diſorders, which, as *iſlanders*, we are expoſed to, by ſuch a cloſe inquiry into the nature of that neceſſary fluid in which we breathe! Not to mention the advantages which ſeveral important branches of trade may receive from ſuch meaſures: and were the digeſted obſervations of the *R. Soc.* compared with thoſe of foreign ſocieties, formed upon the ſame plan, how ſhort a time would bring this part of Philoſophy to the greateſt degree of demonſtrable certainty!

The trouble of making and keeping ſuch *meteorological regiſters*, which, in all probability, prevents ſeveral gentlemen from performing this piece of ſervice to the public, might be rendered very inconſiderable, by the propoſal of an eaſy, as well as comprehenſive, method for a *diary*, and a ſet of ſimple and convenient *machines* for making the neceſſary obſervations.

The plan of the *Ephemerides Ultra-jectinæ*, though comprehenſive, is, with ſubmiſſion, very perplexed; and the ſeveral others, mentioned in the *Philoſ. Tranſ.* perhaps, do not include all the particulars of which ſuch a *Diary* ſhould conſiſt. The Society of *Edinburgh* has prefaced to their *Medical Eſſays* a ſcheme (which I had not the pleaſure of ſeeing till a great while after I had fallen into the following method) the moſt calculated for uſefulneſs; but their *machines* are neither ſo ſimple nor accurate, as ſuch a work requires; not to mention their being entirely without one for obſerving the *force* of the *wind*.

Sect. 1. *An account of the* Diary *in general.*

On a page of a folio paper-book, opening broad-ways, are drawn, at proper diſtances, 9 *horizontal*, and 7 *perpendicular* lines; in the void ſquare ſpaces of which the particulars of the *diary* are written down. The firſt of the horizontal lines is for the days of the *month* and *week*, on which the examination is made: The ſecond for the *hour* of the *day*: The third for the *weight* of the *air*: The fourth for its *heat*: The fifth for its *moiſture*, or *dryneſs*: The ſixth for the *quarter* of the *wind*: The ſeventh for its *force*: The eighth for the *weather*; as whether it be *rainy*, or *cloudy*, or *clear*: The ninth for the *quantity* of *rain*; and the ſpace between the laſt line and the end of the paper, for the *bill of mortality*.

The 7 *perpendicular* lines are for the 7 *days of the week*; which, in our *Diary*, begins with *Sunday*. If you therefore carry your eye along the paper from left to right, you may, at one view, ſee the *weight* of the *air*, and the degrees of *heat* and *moiſture*, *&c.* for the whole *week*. If you carry your eye from top to bottom down the column, for any one day, you ſee regularly the whole of the obſervations in one line for that day. Four pages, or weeks, we allow to each month, and then leave a void page for the *obſervations* made in that month; and the overplus *calendary* days are carried on to the page allotted for the next

month; only taking care to deſcribe in every ſuch page, where the ending and beginning of two different months are to be found, the names of both the months, directly over their final and initial day.

The abſtract of the *weekly bill of mortality* is apparently a part of obſervation peculiar to this plan, under which article all *acute caſes*, depending on the *ſtate* of the *air*, are ſet down. Perhaps the ignorance of the *ſearchers*, appointed to inſpect dead bodies, as to the preciſe diſeaſes people die of, may lay this method open to objection: To which it may be ſufficient to anſwer, That this being obviouſly a requiſite article for a *Diary*, we muſt be content to take our advices on this point from ſuch hands, rather than none; eſpecially, as all *political arithmetick* has always been allowed upon no more certain a foundation.

Sect. 2. *A view of the machines in general.*

The *machines* neceſſary to the making obſervations for a *diary of the weather*, are theſe five:

1. *The Barometer.*

I have found thoſe with *open ciſterns* more ſenſible than the *portable* ones. That with which I make my obſervations, is with an *open ciſtern*, furniſhed with a *Micrometer*, that divides an *inch* into 400 parts; by which I am capable of perceiving the moſt minute alteration of the *gravity* of the *air*: It was made by Mr *Bird* of the *Strand*; whoſe accuracy in graduation deſerves, I think, notice and encouragement.

2. *The Thermometer.*

Mine is one made by *Fahrenheit*'s ſcale on one ſide, with it's correſpondence to the graduation of the *alcohol* Thermometer on the other.

Of the three next *machines*, two are *new*, and the other conſiderably altered, and, I hope, improved, from one offered to the Society a great while ago.

Note, All the *machines*, except the *Barometer*, are expoſed to the open air. The *Thermometer* and *Hygrometer* are placed in a little *ſhed*, made for their reception, againſt my *ſtudy-window*, where I can ſee the graduation through the glaſs; and, by lifting up the ſaſh, can take them in, as occaſion requires.

Sect. 3. *Of the Hygrometer.*

I had, for ſome time, made uſe of Dr *Hooke*'s *Hygrometer*, made of the *beard of a wild oat*, ſet in a *ſmall box*, with a *dial-plate* and an *index*; but I ſoon found an inconvenience, without the remedying of which no dependence could be had on this *machine*, *viz.* its making more than one *revolution* in a night. I endeavoured to remedy this by the following method, deſcribed in *Fig.* 33.

At the vertical point, from which *moiſture* and *dryneſs* are graduated, I cauſed a ſmall *circle* to be deſcribed; the lower *arch* of which ſhould juſt interſect with that *arch*, round which the *index* of the oat deſcribed its circuit. In the centre of this ſmall circle I placed a *pin*, eaſily turning in the central cavity, and furniſhed with a flat piece of thin *ivory* on its head. This piece of *ivory*, interſecting with the index of the oat, by it was turned either to the moiſt or dry ſide of its graduation, as the *index* made a double *revolution*. I flattered myſelf with ſucceſs; but

ſoon

ſoon found, in the great fogs we had laſt winter, that the *wild oat* is not a ſafe material to make an accurate *Hygrometer* of: For,

1. In the great fogs it grew limber; ſo as that the weight of the *index* brought it down upon the plate, where its friction prevented it's further motion.

2. It ſoon loſes its *ſenſibility*, grows harſh, and is abſolutely unfit for uſe. So I immediately turned my thoughts upon ſome other for my *diary*, and reſerved this for my ſtudy; where, or in any incloſed place, it does well enough, and may be very uſeful in the following reſpects; as,

1. To examine, in caſes of ſickneſs, the *dampneſs* of *rooms*.

2. To examine damps in *ſubterraneous cavities*, being let down with a *weight*, where a light would ſometimes ſet the place on fire.

3. To obſerve the proper ſtate of dryneſs in *ware-houſes*, *wine-vaults*, *ſtudies*, where damps would be detrimental and pernicious.

4. To examine the ſtrength of *ſudden fogs*, and the *comparative* dampneſs of particular ſituations.

As a *ſuccedaneum* to this, I thought upon a *ſtatical* one; it recurring to my mind, that the weight and moiſture of the air being but two properties of one and the ſame body, a *ſtatical Hygrometer* (*cæteris paribus*) promiſed the beſt aſſiſtance towards a more complete knowledge of the *Barometer*, which acts upon ſtatical principles; and that theſe two machines muſt have a reciprocal correſpondence with each other. I then remembered, that Mr *Boyle* had mentioned ſomething of this nature; after conſulting whom, I made the the following *machine*, acting upon his principles, but formed in a manner differing from his.

I cauſed a *balance* to be made to turn with $\frac{1}{2}$ *a grain*, ordering that the *axis* of the *balance* ſhould, on one ſide, be drawn out to the length of one inch, and its end to be furniſhed with a *male ſcrew*, to which a light *index* with a *female ſcrew* might be fixed. I had this *balance* faſtened in a *wainſcot box*, 12 inches in length, 9 in diameter, and 4 in the depth at top, but gradually widening towards the bottom, with a back to ſlide up and down in a groove. The *axis*, already mentioned, of an inch length, came through a *hole* in the front of the box, and then had the *index* faſtened on, which deſcribed the *ſegment* of a *circle* upon a braſs plate, ſilvered and graduated into 180 gr. as if it had conſiſted of a perfect *ſemicircle*, or two *quadrants*. The reaſon why the graduation did not begin exactly with the diametrical line was, to prevent the friction of the *brachia* of the balance, with the little drop placed at the bottom of the *axis* already mentioned.

My next concern was to *charge* this *balance*. The beam turned, as has been ſaid, with $\frac{1}{2}$ *a grain*; and every ſuch turn, after repeated trials, moved the index ſomewhat more than one *degree* of the 180 deſcribed upon the *plate*; ſo I immediately pitched upon a 4 *penny-weight* all but 6 *grains*, which contained as many $\frac{1}{2}$ *grains* as there were *degrees*. This weight I fixed with a thread to one *brachium* of the *balance*, with-

out any *ſcale*, the ſeveral threads or ſilk ſtrings of which, as they would imbibe more moiſture, would make the *machine* leſs accurate; and the other *brachium* I charged with a *ſponge*, ſuſpended likewiſe by a thread, of ſuch a weight, when reduced to abſolute dryneſs, as made an *equilibrium*; and then ſcrewing on the *index* to the firſt degree of the 180, and expoſing the *machine*, thus ordered, to the open *air*, in one night's time the *index* had got to the 70th degree; which, as the *ſponge* had been abſolutely dry, muſt have been the true *ſtate* of the *air*, as to *moiſture*, at that time.

I find this *machine* extremely ſenſible and accurate; it will alter 10 degrees in a night, and as many in a day; and has, I think, the following advantages:

1. It is more *portable* than any, except that of the *wild oat*; and, upon any accident, more eaſily and ſpeedily rectified than it, or any other whatever.

2. Being graduated from abſolute *dryneſs*, it is beſt calculated for the diſcovery of the true ſtate of the *air*, as to *moiſture*.

3. The near correſpondence between the degrees on the graduated *plate*, and the weight of the *moiſture* neceſſary to be imbibed or exhaled, to make either *brachium* of the balance preponderate every ſuch degree, gives it the preference to any other.

For a more perfect idea of this machine, ſee *Fig.* 28. where it is viewed on the inſide, the back being ſlid up. At *Fig.* 29. is repreſented the *Plate* with its graduations and index, as it ſhould appear on the front of the caſe.

Sect. 4. *Of the Anemoſcope.*

The *Anemoſcope* is a machine $4\frac{1}{4}$ feet high, conſiſting of a broad and weighty *pedeſtal*, a *pillar* faſtened into it, and an iron *axis*, of about $\frac{1}{2}$ an inch diameter, faſtened into the *pillar*. Upon this *axis* turns a wooden *tube*, at the top of which is placed a vane, of the ſame materials, 21 inches long, conſiſting of a *quadrant*, graduated and ſhod with an iron *rim*, notched to each degree; and a *counterpoiſe*, of wood as in the figure, on the other. Through the centre of the quadrant runs an *iron pin*, upon which are faſtened two ſmall round pieces of wood, which ſerve as moveable *radii* to deſcribe the degrees upon the quadrant, and as handles to a *velum* or ſail, whoſe plane is one foot ſquare, made of canvas ſtretched upon four battens, and painted. On the upper batten, next to the ſhod *rim* of the quadrant, is a ſmall *ſpring*, which catches at every notch correſponding to each degree, as the wind ſhall, by preſſing againſt the *ſail*, raiſe it up; and prevents the falling back of the *ſail*, upon the leſſening of the force of the wind. At the bottom of the *wooden tube* is an *iron index*, which moves round a circular piece of wood faſtened to the top of the *pillar* on the pedeſtal, on which are deſcribed the 32 points of the *compaſs*. The figure of this machine may be ſeen *Fig.* 30. Its uſes are the following

1. Having a circular motion round the *iron axis*, and being furniſhed with a *vane* at top, and *index* at the bottom, when once you have fixed the

the artificial *cardinal points*, deſcribed on the round piece of wood on the *pillar*, to the ſame *quarters* of the heavens, it gives a faithful account of that *quarter* from which the wind blows.

2. By having a *velum* or ſail elevated by the wind along the *arch* of the quadrant, to an height proportionable to the power of the column of wind preſſing againſt it, the *relative force* of the wind, and its *comparative power*, at any two times of examination, may accurately be taken.

3. By having a ſpring fitted to the notches of the *iron* with which the quadrant is ſhod, the *velum* is prevented from returning back upon the fall of the wind; and the machine gives the force of the higheſt blaſt, ſince the laſt time of examination, without the trouble of watching it.

I have carefully examined the dependence that may be had upon this machine, during the late ſtorms in *February* $174\frac{3}{4}$, by comparing the height to which the wind then forced the *velum*, with the *Deal* letter. The 19th of *Feb. Sabbath*, 8 *a. m.* the Anemoſcope was at 75: The *Deal* letter for that day called it a ſtorm. The *Saturday* following, being the 25th, at 8 *p. m.* the machine was at 79: The *Deal* letter called that a violent ſtorm. The *Wedneſday* following, the laſt of *Feb.* it was at 84: The *Deal* letter called that a violent ſtorm. So that it appears, that, in ſuch as the ſailors allow to be *violent ſtorms*, the machine has hitherto anſwered well, and has had ſix degrees to ſpare for a more violent guſt, before it comes to an horizontal poſition.

It is certainly to be depended upon in ordinary weather, the *velum* being hung ſo tender, as to feel the gentleſt breeze. But, after all, I muſt freely own, that I fear the expoſing this machine to all winds, for a continuance, muſt ſoon diſorder it; and that irregular blaſts and ſqualls cannot fail in a ſhort time to impair it. It may not therefore be amiſs, to prevent this, for gentlemen to take the machine in in violent weather; and, by taking the *tube* off the iron *axis*, to make their obſervations with the *tube*, *vane*, and *velum*, in their hands; which, as it is very light, and far from cumberſome, is eaſy to do, as I have often experienced.

This machine conſiſts of a tin *funnel*, whoſe ſurface is an inch ſquare, a flat *board*, and a glaſs *tube* let into the middle of it in a groove (the length and breadth of both board and tube being *ad libitum*), and an *index*. My board is about 3 feet long, to anſwer the height of the rails that go round the top of my houſe, to one of which it is hung, clear of any obſtacle to prevent the free fall of the rain, with 4 little ſtaples that ſlide over as many tenter-hooks. The *bore* of my tube is about $\frac{1}{2}$ an inch; which, at a medium, is the beſt ſize, a larger bore obliging you to make your graduation the more contracted, and, conſequently, the leſs plain and accurate; and a leſſer not permitting you to return the water out of the tube when full, without the adheſion of a great deal to its ſides; which, when you have placed the tube in its perpendicular ſituation, ſubſides, and ſometimes fills up $\frac{2}{3\,2}$ of an inch; which, without

Sect. 5. *Of the Ombrometer.*

our care, muſt neceſſarily make great miſtakes in the *diary*. The method of *graduating* the board is this :

I had a veſſel of tin made, whoſe contents were exactly a *cubic inch*. With this veſſel, filled with water exactly to it's ſurface, I frequently gauged the tube, till, by repeated trials, I had found the height to which a *cubic inch* of water would riſe in it. The ſpace anſwering to this on the board I had graduated into 32 equal parts, and took the ſame method with the reſt of the tube, till in the ſame manner I had graduated 4 ſuch inches. Now the ſurface of the funnel being, as has been ſaid, exactly a ſquare inch, no rain can by it get into the tube, but ſuch as falls within the ſquare of one inch; which, as the ſhower is more or leſs, has its exact quantity ſhewn upon the board, on which a moveable *index* is placed.

This machine has highly anſwered my expectation; its form being very ſimple, and eaſily repaired, if any accident happen. For, ſhould the tube be broke, 'tis only rubbing out the graduation, which is marked with a black-lead pencil upon the board painted white, and gauging your new tube with the cubic meaſure for a new graduation, and your machine is again complete. I had one tube broke, and about 3 hours pains ſet all to-rights. In winter it will be neceſſary to let no depth of water remain in the tube; for, ſhould there be a froſt, the expanſion of the ice will certainly break it. The machine will equally ſerve for diſſolved hail and ſnow. See *Fig.* 32.

Sect. VI. *Of the monthly obſervations.*

The vacant page at the end of every 4 weeks, reſerved for obſervations occurring in the preceding month, and giving a ſummary account of the greateſt difference of the weather in it, is a method peculiar to this *Diary*; and one which, I flatter myſelf, will be allowed exceeding pertinent and uſeful. The great end of this, and all *Diaries*, is to furniſh materials for a ſet of ſound obſervations upon which to build a thorough knowledge of the *atmoſphere*, and its effects upon mankind: and it is eaſy to ſee what great advantage to this part of *natural knowledge* muſt ariſe from a variety of obſervations, made by different men of application and judgment, upon one and the ſame ſubject. Beſides, in this portion of our deſign may be included, what could not well without perplexity be thrown into the columns of the Diary, all the meteorological appearances of the *Aurora Borealis*, lightning, thunder, *&c.* together with abſtracts of the moſt authentic accounts of ſuch *phaenomena*, as at any time in the preceding month have been ſeen in different parts of our own country, or abroad. But this article muſt be left to every gentleman's judgment; it opening a fair field for the moſt happy advancements of many parts of *natural knowledge*.

An explanation *of the* characters *in the* Diary.

This —— line implies the machine's being at the ſame degree as it was in the preceding obſervation. This *O* character in the ſpaces for the force of the wind implies a *calm*.

Note, 1ſt, None, but the cardinal and ſubcardinal *points* of the *compaſs* are commonly marked, unleſs in caſe of a *ſtorm*.

2dly,

APRIL 1744.

Days of the Month and Week.	1 Sabbath.	2 Monday.	3 Tueſday.	4 Wedneſday.	5 Thurſday.	6 Friday.	7 Saturday.
Hours of the Day.	8 *a. m.* 8 *p. m.*	8 *a. m.* 8 *p. m.*	8 *a. m.* 8 *p. m.*	8 *a. m.* 8 *p. m.*	8 *a. m.* 11 *p. m.*	8 *a. m.* 8 *p. m.*	8 *a. m.* 8 *p. m*
Barometer.	29 196/400 29 192/400	29 126/400 29 45/400	29 144/400 29 246/400	29 297/400 29 305/400	29 146/400 29 116/400	29 132/400 29 335/400	29 392/400 29 378/400
Thermometer.	37 —	36 —	37 35	38 35	40 45	40 34	55 45
Hygrometer.	70 77	— 79	81 80	— 74	81 74	77 74	— 69
Anemoſcope Quarter.	W.	N. W.	—	—	S. E.	N. W.	W.
Anemoſcope Force.	28	30	74	16	20	16	0
Weather.	Sleet. Rains.	Snow. Sleet.	— Cloudy	— Starlight.	Rain. Cloudy.	Overcaſt. Starlt.	Fine. Overcaſt.
Ombrometer.	13	3 13	22 3		2 1	13	
Bill of Mortality.	Buried.	Males - - 176 Females - 217 Total - - 393 Decreaſed 70	Died of	Apoplexy — 1 Aſthma — 8 Colic — 1	Fever — 52 Gripes — 4 Lunatic — 2	Small Pox 22 Suddenly 2	

Scheme of a Diary of the Weather.

2dly, In the abſtract of the *bill of mortality*, which comes out on a *Tueſday*, the account in each week is to be compared from the *Tueſday* of the week before, to the *Tueſday* in that week where the abſtract is placed.

OBSER-

OBSERVATIONS in APRIL 1744.

Days of the Month.	Days of the Week.	Hours of the Day.	
		M.	
19	Monday.	12	LAST Night, as 8 ½ Hour, carefully watched, whether the lunar Eclipſe had any Effect upon the Hygrometer; but could not, after ſeveral Examinations, perceive that it had any.
		P. M.	
20	Friday.	8	This is the firſt Day of our being favoured with warm Spring Weather. The Thermometer at 8 this Morning was at 65.
		P. M.	
24	Tueſday.	10½	It now lightens towards the S. E. This is the firſt we have had this Year.

A SUMMARY of the greateſt Difference of the WEATHER in APRIL 1744.

Days of the Month.	Days of the Week.	Hours of the Day.			
		A. M.			
21	Saturday.	8	Mercury	Higheſt being then	30 167/400
		P. M.		Loweſt	29 116/400
5	Thurſday.	11			
		P. M.			
21	Saturday.	8	Thermometer	Hotteſt	75
		P. M.		Coldeſt	34
6	Friday.	8			
		A. M.			
3	Tueſday.	8	Hygrometer	Moiſteſt	81
		P. M.		Drieſt	65
21	Saturday.	8	Anemoſcope	Quarterly moſt from S. E.	
		A. M.		Force greateſt from N. W.	74
3	Tueſday.	8	Weather very unconſtant.		
			Ombrometer	Moſt Rain on 7th in the Night	24/32
				Total Rain 5 Inch and	6/36
			Mortality	Greateſt in the 3d Week	432
				Leaſt 1ſt Week	393
				Total	1702

Deſcription *of the* Figures.

Fig. 28. *aaaa.* The *Hygrometer* ſeen in the inſide. *bb.* The balance. *c.* A ſmall piece of wood, by which the balance is faſtened to the box. *d.* The ſponge. *e.* The weight. *ff.* Two little rings, by which the Hygrometer is hung up.

Fig. 29. The graduated plate on the front of the machine, with it's *Index* and *Diviſions.*

a. The

a. The pedeſtal. *b*. The pillar, in which the iron axis is ſitted. *c*. The circle of wood, on which are deſcribed the 32 points of the compaſs. *d*. The index. *e*. The wooden tube upon its axis. *f*. The *velum*. *g*. The graduated quadrant. *h*. The counterpoiſe of the vane. Fig. 30. The *Anemoſcope*.

The plane of the *velum*. *b*. The ſpring. *cc*. The wooden *radii*. *dd*. The holes, thro' which the pin, in the centre of the quadrant, goes. Fig. 31. *The Velum taken off*.

aa. The board. *bb*. The tube. *c*. The graduation. *d*. The funnel fixed in the tube. *e*. The funnel one *Inch* ſquare. Fig. 32. The *Ombrometer*.

a The box and plate. *b*. The wild oat, with the index upon it. *c*. The pin, with a ſmall piece of ivory on its head. Fig. 33. The wild-oat *Hygrometer*.

X. The viciſſitudes of the weather, with reſpect to heat and cold, are perhaps no where greater than in *Carolina*; and our ſummer's heat is probably not inferior to that under moſt places of the Equator; nor is our winter's cold much leſs at ſome times than that in *Britain*.

From near 8 years obſervation, the greateſt increaſe of the heat of the air which I have diſcovered in 24 or 30 hours, in ſpring, ſummer, autumn, and winter, was 19, 24, 13, and 16° of *Fahrenheit*'s Thermometer; and the greateſt decreaſes of heat, in the ſame ſpaces of time, in thoſe ſeaſons, were 35, 32, 27, and 27° reſpectively. It frequently happens, that one day is 10° or more warmer than the preceding day; but the decreaſes of heat are always greater and more ſudden than its increaſes. *Jan.* 10. 1745. at 2 *p. m.* the mercury in the Thermometer was at 70; next morning it had ſunk to 26, and on the 12th in the morning it was at 15, which was the greateſt and moſt ſudden change I have ſeen. *A Letter from Dr* John Lining, *to* C. Mortimer, *M. D. Sec. R. S. concerning the weather in* South-Carolina; *with abſtracts of the tables of his* Meteorological obſervations *in* Charles-Town N°. 487. p 336. Apr. &c. 1748. *Read* May 6. 1748.

In ſummer, the heat of the ſhaded air, about 2 or 3 in the afternoon, is frequently between 90 and 95°; and on the 14th, 15th, and 16th of *June* 1738, at 3 *p. m.* it was 98; a heat equal to the greateſt heat of the human body in health. In winter I never but once ſaw the Thermometer ſo low as 15: therefore the difference between the moſt intenſe heat and cold of the ſhaded air, in this province, is 83°; which is a much greater range than could well have been expected in this latitude; and taking the mean between thoſe extremes, 56 ſhould be the temperate degree of heat in this province: but the ſum of the thermometrical altitudes, divided by the number of obſervations which I made for ſome years together, gives 66, which may therefore more juſtly be reckoned the temperate heat in *Carolina*, which exceeds 48, the temperate heat in *England*, more than that exceeds the freezing point.

The mean heat of the ſhaded air, in ſpring, ſummer, autumn, and winter, taken from the mean nocturnal heat, and from the mean heat at 2 or 3 *p. m.* is 61, 78, 71, and 52°.

The mean heat of the ſhaded air at 2 or 3 *p. m.* in ſpring, ſummer, autumn, and winter, is 65, 82, 75, 55°, and the mean nocturnal heat in theſe ſeaſons is 57, 74, 68, and 49°. Therefore our winter's nocturnal heat, at a medium, coincides nearly with the temperate heat in *England*.

The Thermometer, when ſuſpended 5 feet from the ground, and expoſed to the direct rays of the ſun, and to thoſe reflected from our ſandy ſtreets, has frequently riſen in a few minutes, from 15 to 26°, above what was at that time the heat of the ſhaded air (but I have never yet made that experiment when the heat of the ſhaded air was above 88): when we are therefore expoſed in the ſtreets to the ſun in ſummer, we inſpire air from 4 to 28 degrees warmer than the heat of the human body.

The Thermometer, when buried in the ſands of the ſtreets, when the heat of the ſhaded air was 88, roſe in 5′ to 108, tho' there was at the ſame time a moderate wind.

In *June* 1738, when the heat of the ſhaded air was 98, the Thermometer ſunk one degree in my arm-pits; but continued at 98 in my hand and mouth: from which we ſee what little concern the air has in cooling the blood in the lungs. Two men who where then in the the ſtreets (when the heat was probably 124 or 126 degrees, as the ſhaded air's heat was then 98) dropped ſuddenly dead; and ſeveral ſlaves in the country, at work in the rice-fields, ſhared the ſame fate. I ſaw one of the men immediately after he died; his face, neck, breaſt, and hands, were livid,

From the barometrical table it appears, that the barometer's mean altitude, taken from it's greateſt and leaſt height, is 30.09 inches; and that its range is only 1.22 inch. Wherefore our atmoſphere varies only $\frac{1}{25}$ part in its weight. In the warm months, the mean barometrical ſtation, taken from it's greateſt and leaſt altitudes in theſe months, is 30.09 inches; and I have never yet ſeen it's range in theſe months exceed $\frac{58}{100}$ parts of an inch: therefore the changes of our atmoſphere's weight, in the warm months, will have but little effect upon human conſtitutions, as the difference between it's greateſt and leaſt preſſure is but $\frac{1}{5}$ part of that in cold climates, where the range of the Barometer is 3 inches. May not the great height of the Barometer in the warm months in this climate, proceed from the vaſt quantity of water, which is at that time ſupported in our atmoſphere, as the exhalation is then very great; or may it not proceed from the rarefaction of the mercury? for the weight of the mercurial column, at equal altitudes, will be different under different degrees of heat; and the mercury may therefore be ſupported at equal heights by columns of air of unequal weights.

It appears, from the barometrical table, that our eaſterly or northerly winds elevate the mercury, and that our ſoutherly or weſterly winds depreſs it; and I have as yet never obſerved the contrary.

A TABLE

Fig. 28.

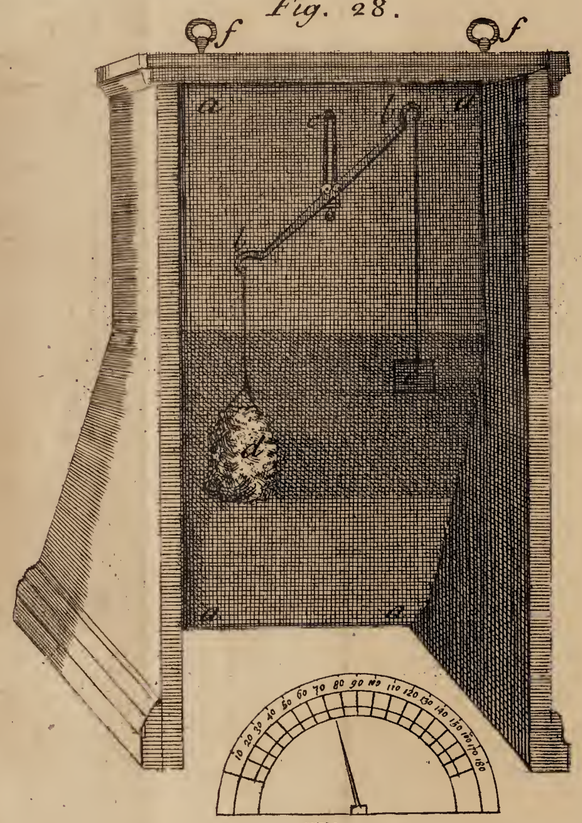


Fig. 29.

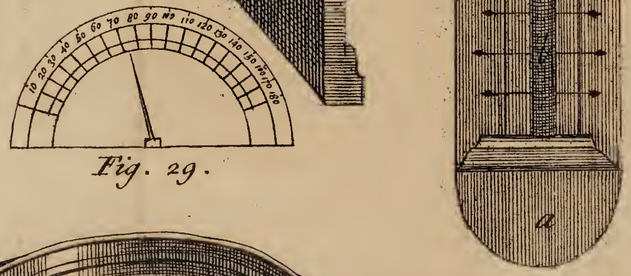


Fig. 32.

Fig. 30.

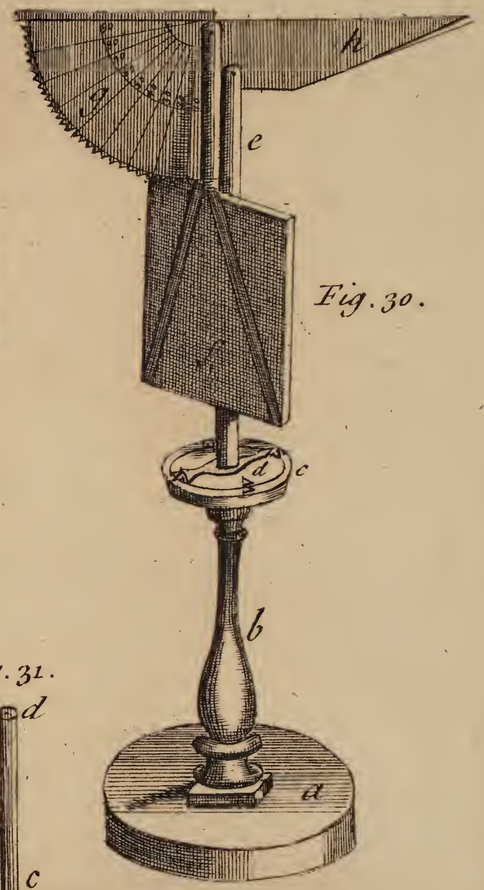


Fig. 31.

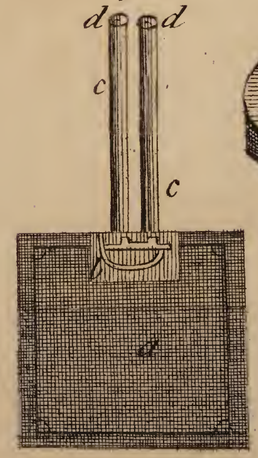
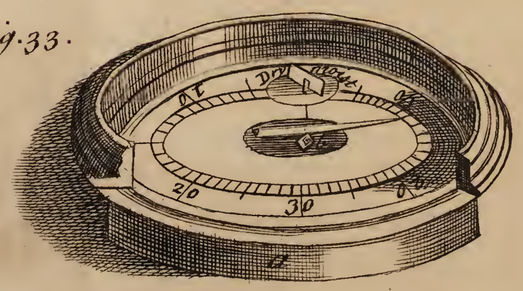


Fig. 33.



A TABLE *of the highest and lowest Stations of* Fahrenheit's *Mercurial Thermometer in the shaded Air, with the mean meridian and nocturnal Heat, taken after Dr* Jurin's *Method.*

	1738 Highest	1738 Lowest	1739 Highest	1739 Lowest	1740 Highest	1740 Lowest	1742 Highest	1742 Lowest	1738 Mean Altit. about 10 p. m.	1738 Mean Altit. in the Heat of the Day	1739 Mean Altit. about 10 p. m.	1739 Mean Altit. in the Heat of the Day
January	71	36	69	19	67	30	68	34	60	53	53	45
February	72	34	75	38	75	27	68	25	57	51	63	55
March	74	32	79	40	80	34	77	32	62	56	65	59
April	86	50	84	50	83	51	88	59	75	65	69	65
May	91	52	86	62	87	56	88	63	79	70	77	73
June	98	59	87	69	90	66	90	65	87	77	81	74
July	91	70	89	68	91	70	95	73	85	77	82	75
Auguſt	89	64	87	60	90	67	93	69	82	75	82	75
September	83	62	88	59	84	56	86	43	76	71	71	68
October	74	41	79	43	73	35	78	42	65	59	68	60
November	70	37	69	30	67	32	66	32	57	51	57	51
December	67	28	70	30	69	21	69	34	53	50	59	53
The Means									70	63	69	66

	1740 Mean Altit. about 10 p. m.	1740 Mean Altit. in the Heat of the Day	1742 Mean Altit. about 10 p. m.	1742 Mean Altit. in the Heat of the Day	Mean Altitude in the Heat of the Day	Meridian Heat warmer than the Nights	Mean Heat taken from the Mean Meridian and nocturnal Heat	R. Society's Thermometer 1739 Higheſt	R. Society's Thermometer 1739 Loweſt	R. Society's Thermometer 1740 Higheſt	R. Society's Thermometer 1740 Loweſt
January	49	43	55	49	54	48 6	51	40	74	44	67
February	59	49	52	45	58	50 8	54	36	60	40	69
March	63	54	60	53	62	56 6	59	35	63	34	65
April	74	65	77	67	74	66 8	70	32	55	33	55
May	78	70	79	70	78	71 7	75	31	46	31	50
June	83	74	83	72	84	74 10	79	31	42	28	44
July	86	76	86	79	85	77 8	81	29	42	27½	39
Auguſt	81	74	87	75	83	75 8	79	31	48	29	42
September	78	72	76	69	75	70 5	73	31	49	32	50
October	63	56	66	58	66	58 8	62	35	58	38	62½
November	56	50	53	46	56	50 6	53	42	67	42½	64
December	48	41	57	48	54	48 6	51	37	66	41	72
The Means	68	60	69	61	69	62 7	65½				

A TABLE

A TABLE *of the highest and lowest Barometrical Stations; with the Directions which the Wind then had.*

× A Northerly or Easterly Wind } preceded or succeeded
♪ A Southerly or Westerly Wind }

Jan.						30.48	N	29.88	SW	.60
Feb.						30.38	NE	29.68	S	.70
March						30.26	SE	29.58	S	.68
April	30.42	E	29.48	W	.94	30.38	W×	29.78	WSW	.60
May	30.23	NE	29.85	S	.38	30.35	E	29.80	W	.55
June	30.20	NE	29.85	W	.35	30.30	E	29.98	SW	.32
July	30.13	SSW×	29.83	SW	.30	30.38	E	30.00	SW	.38
Aug.	30.18	E	29.88	SW	.30	30.38	NE	29 98	SW	.40
Sept.	30.33	NNE	29.85	SE	.48	30.38	E	29 88	NW	.50
Oct.	30.33	E	29.83	WNW	.50	30.45	E	29.68	W	.77
Nov.	30.58	N	29.72	S	.86	30.35	NE	29.58	W	.77
Dec.	30.60	N	29.93	W	.67	30.58	N	29.75	NNW♪	.83

Jan.	30.70	N	29.50	NW♪	1.20	30.46	NNE	29.76	W	.70
Feb.	30.55	N	29.85	W	.70	30.54	NNE	29.72	WSW	.82
March	30.50	SE	29.65	W	.85	30.40	ENE	29.60	W	.80
April	30.32	E	29.75	N♪	.57	30.48	E	29 58	W	.90
May	30.28	E	29.85	S	.43	30.30	S×	29.90	SSW	.40
June	30.18	S×	29.86	S	.32	30.28	ESE	29.90	NE♪	.38
July	30.08	SSE×	29.85	SSW	.23	30.22	W	29.98	SW	.24
Aug.	30.26	E	29.85	W	.41	30.25	NE	29.95	N	.30
Sept.	30.28	NE	29.85	NE♪	.43	30.36	NE	29.86	S	.50
Oct.	30.32	NNE	29.72	SW	.60	30.50	N	29.95	W	.55
Nov.	30.51	N	29.72	S	.79	30.55	NNW	29 73	SW	.82
Dec.	30.60	ENE	29.86	SW	.74	30.58	NNE	29.65	WNW	.93

A TABLE

A TABLE *of the Depth of Rain, in Inches and milleſimal Parts, which fell in* Charles-Town.

	1738	1739	1740	1741	1742
January	1 097	2 310	4 873	4 492	2 189
February	4 416	2 875	3 084	4 615	1 650
March	4 532	5 609	1 141	5 713	5 203
April	1 082	0 195	1 092	1 308	0 918
May	3 127	5 120	5 612	4 841	5 898
June	1 567	15 839	4 648	5 538	3 250
July	10 660	5 452	3 013	3 399	1 252
Auguſt	4 104	12 211	7 301	7 144	7 647
September	10 792	4 834	3 200	6 734	2 895
October	1 358	6 593	1 258	3 399	0 759
November	2 656	1 235	1 848	2 964	3 388
December	3 877	3 689	2 736	1 919	0 957
Total Depth	49 .268	65 .962	39 .806	52 .066	36 .006

	1743	1744	1745	The Means	1746
January	3 172	1 994	0 863	2 624	1 144
February	2 435	3 063	7 739	3 735	2 701
March	0 621	0 582	3 229	3 329	1 628
April	5 292	2 866	3 842	2 074	1 128
May	2 535	2 871	1 832	3 979	3 988
June	1 903	5 814	9 510	6 009	4 109
July	7 738	8 437	6 771	5 840	9 895
Auguſt	3 767	4 202	9 339	6 964	6 114
September	4 686	5 657	0 754	4 944	0 932
October	1 672	1 595	2 962	2 450	
November	3 220	1 562	0 682	2 194	
December	2 706	9 680	2 623	3 523	
Total Depth	39 .747	48 .323	50 .146	47 .666	

XI. *June* 10. ſuſpecting a froſt that night, I ſat a *China* ſaucer full of water upon the graſs-plot, in the garden; and the next morning, a little before ſun-riſing, I found the water frozen over, of ſuch a conſiſtence, as that I forced a hole through the centre of it with my finger, without breaking it elſewhere, and carried the cake of ice into the houſe, where it remained a good while not diſſolved. Wind N. W. On ſome following days there were ſeveral conſiderable froſts, the wind continuing the ſame way; the fatal effects of which are ſufficiently known throughout the kingdom.

July 2, at 12^h 20′ my Thermometer of *Farenheit*'s ſcale, in the ſhaded air, ſtood at 88, and at 2^h *p. m.* at 87. At which laſt number two others of the ſame ſort ſtood exactly, at that hour, in *London*.

Having agreed with Mr *John Canton* of *Spital-Square*, to make obſervations of the temperature of the air here, and in *London* at a ſtated hour: we procured Thermometers, made exactly alike, by that accurate workman Mr *Bird*; and having found, by hanging them firſt together a ſufficient time, that they perfectly agreed, we began our obſervations in *April*, and have continued them ever ſince.

The Thermometers are of the ſmaller ſize, the bulbs being but about $\frac{47}{100}$ of an inch diameter, and are immediately affected with any mutations of the air; ſo that I have frequently been entertained with obſerving, in ſome circumſtances of the weather, that the mercury has not been ſtationary, but has ſucceſſively riſen and fallen for a good while; and Mr *Canton* has informed me, that he has ſeveral times obſerved the ſame.

I have annexed a paper, containing an extract from my journal of the weather, in which I have ſet down the extremes of the Barometer and Thermometer, obſerved at 2^h *p. m.* for ſix months; and Mr *Canton* has been ſo kind as to communicate a like extract from his journal; which is likewiſe put into your hands.

It appears by a more general compariſon which we have made, as well as by this particular one, that the difference in the temperature of the air, as to heat and cold, is very little between this place and *Spital-Square*. Sometimes my Thermometer has been higher than his; more times upon an equality, but moſt times lower. And I have reaſon to think the difference, many times, may have been owing to accidental cauſes: for Mr *Canton* has informed me, that he has found 2 Thermometers, when removed but a few yards from each other, have differed 2 or 3 deg. for which no apparent cauſe could be aſſigned. So that upon the whole, it may reaſonably enough be concluded, that the difference between the temperature of the air in the two places, is imperceptible to ſenſe.

P. S. Upon my having obſerved that the days, in which my Thermometer and Mr *Canton*'s ſtood at the extremes, in ſome months did not coincide, I was deſirous of knowing, how much the Thermometers differed, when the extremes did not happen on the ſame day: but,

A Letter from the Rev. Hen. Miles, *D. D. F. R. S. to Mr* Hen. Baker, *F. R. S. concerning a very* cold day, *and another a very* hot day, *in* June *and* July 1749, *and of the near* agreement *of* Thermometers *in* London *and at* Tooting. N°. 493. p. 208. Oct. &c. 1749. *Read* Nov. 16. 1749.

but, upon a compariſon, the difference was never conſiderable, except on *Aug.* 4. at 2 *p. m.* when my Thermometer was about 6° higher than Mr *Canton*'s. This being ſomewhat remarkable, he, upon hearing it, had recourſe to his regiſter, and found, that at the time of obſervation a heavy ſhower of rain fell; whereas we had none here: but about 6 *p. m.* came on a thunder ſhower attended with rain; ſo that it ſhould ſeem the falling rain had ſo great an effect upon the air as to render it cooler, by the degrees mentioned: and perhaps the difference between the two inſtruments, at other times, may have been owing to the ſame, or a ſimilar cauſe, rather than to a *ſtated* different temperature of the air, in the two places.

EXTRACT *from a Journal of the Weather, made at* Tooting, *in* Surry, *in which the Extremes of the Barometer and Thermometer are noted at* 2^h p. m. *equal Time, for* May, June, July, Aug. Sept. *and* Oct. *this preſent Year* 1749.

MAY. 1749.

Day	Bar.	Day	Ther.
14	higheſt 30,04	17	higheſt 76
Day	Bar.	Day	Ther.
24	loweſt 29,15	3	loweſt 50

JUNE. 1749.

Day	Bar.	Day	Ther.
8	higheſt 30,10	28	higheſt 79
Day	Bar.	Day	Ther.
17	loweſt 29,25	3	loweſt 46

JULY. 1749.

Day	Bar.	Day	Ther.
8	higheſt 29,95	2	higheſt 87 *
Day	Bar.	Day	Ther.
20 22	loweſt 29.44	30	loweſt 62½

AUGUST. 1749.

Day	Bar.	Day	Ther.
15	higheſt 30,02	22	higheſt †
Day	Bar.	Day	Ther.
2	loweſt 29,20	31	loweſt 62½

SEPTEMBER. 1749.

Day	Bar.	Day	Ther.
26	higheſt 30,37	5	higheſt 71
Day	Bar.	Day	Ther.
17	loweſt 29,3	12	loweſt 53

OCTOBER. 1749.

Day	Bar.	Day	Ther.
10	higheſt 30,44	4 7	higheſt 61½
Day	Bar.	Day	Ther.
28	loweſt 29,51	27	loweſt 43

† Being

† Being abſent *Aug.* 22d, at 2 *p. m.* I could not obſerve the ſtate of the Thermometer; but am well ſatisfied that day was the hotteſt in the month, from the obſervation I made at other times of the day, and particularly from the account I had from Mr *Canton*, of the ſtate of his, which ſtood at 80½ at 2 *p. m.*

It may be proper to obſerve, that the Barometer made uſe of ſtands $\frac{2}{10}$, or more, lower than others of the ſame conſtruction (which is the common upright make) during the warmer ſeaſon of the year, and uſually as much higher than they do in the colder; but is made uſe of (as it has been for more than 10 years) becauſe I have always found it to riſe and fall ſooner than any other I have compared it with, and in particular than a very good one, made by the late Mr *Siſſon*, which has always hung by it, and is conſtantly compared therewith.

EXTRACT *from a Journal of Obſervations on the* Barometer *and* Thermometer, *made in* Spital-Square, London, *in which the Extremes of each are noted, at* 2^h p. m. *equal Time, for* May, June, July, Aug. Sept. *and* Oct. *this preſent Year* 1749.

MAY. 1749.			
Day	Bar.	Day	Ther.
13	higheſt 33,23	13	higheſt 76½
Day	Bar.	Day	Ther.
25	loweſt 29,33	4	loweſt 59

JUNE. 1749.			
Day	Bar.	Day	Ther.
27	higheſt 30,32	28	higheſt 80
Day	Bar.	Day	Ther.
17	loweſt 29,30	3	loweſt 48¾

JULY. 1749.			
Day	Bar.	Day	Ther.
8	higheſt 30,35	2	higheſt 87
Day	Bar.	Day	Ther.
24	loweſt 22.66	18.30 31	loweſt 64½

AUGUST. 1749.			
Day	Bar.	Day	Ther.
15	higheſt 30,25	22	higheſt 80½
Day	Bar.	Day	Ther.
2	loweſt 29,41	4	loweſt 59½

SEPTEMBER. 1749.			
Day	Bar.	Day	Ther.
26	higheſt 30,44	5	higheſt 70½
Day	Bar.	Day	Ther.
17	loweſt 29,37	2	loweſt 54½

OCTOBER. 1749.			
Day	Bar.	Day	Ther.
10	higheſt 30,49	7	higheſt 61
Day	Bar.	Day	Ther.
28	loweſt 29,43	27	loweſt 43

Two Letters from the Rev. Hen. Miles, *D. D. F. R. S. to Mr* Hen. Baker, *F. R. S. concerning the* Heat *of the* Weather *in* July *and* Sept. *laſt.* N°. 496. p. 571. Nov. &c. 1750. *Read* Nov. 22. 1750. *Firſt Letter.*

XII. *July* 11. The morning at 4 had nothing remarkable: at 2 *p. m.* the heavens moſtly clear, and no indications of a ſtorm; the Barometer having fallen but $\frac{6}{100}$ inch ſince 4 *a. m.* it then ſtood at 30,20, the Thermometer at $87\frac{1}{2}$, and before 3 *p. m.* at $88\frac{1}{4}$, which is the hotteſt temperature of the air I ever knew.

At 4 *p. m.* we had very diſtant thunder; ſoon after it came a little nearer, and was one continued murmur, without any perceivable intermiſſion for great part of an hour: the lightning accompanying it, not much. The wind was nearly S. W. and dark clouds paſſed by on each ſide of us till they united in the N. forming one of the blackeſt clouds I ever ſaw, over the city, as near as I could gueſs. We had not one drop of rain, nor did there fall either rain or hail for near 3 miles to the N. of us towards *London*: a few hail-ſtones, I am informed, fell in ſome parts of *Clapham*; what the extent of the ſtorm might be on other ſides of the city, I have not heard.

By the obſervation I made, there did not appear any conſiderable change in the ſtate of the air, as to the weight or heat of it. The Barometer fell little, and the Thermometer no more than uſual at that time of the evening. Mr *Canton* writes me, that his Thermometer in *Spital-Square* (of the ſame conſtruction with mine, and kept too in the open air) fell no leſs than 17 degrees.

Second Letter. At 4 *a. m.* the wind being eaſterly, and blowing ſtrong, accompanied with ſeveral ſhort ſhowers of rain, the Barometer being at 29,97, I obſerved my Thermometer abroad to ſtand at 61: a degree of heat exceeding any I have taken notice of during the whole ſummer at that time of the morning.

Tooting, Sept. 2, 1750. *H. Miles.*

Extract of a Letter from Mr William Arderon, *F. RS. to Mr* H Baker, *F. R. S. concerning the* hot Weather *in* July *laſt.* Ibid. p. 573. *Read* Nov. 22. 1750.

XIII. The beginning of this heat was on the 8th of *July*; on which (though the whole day was cloudy) the ground was ſo uncommonly hot, that I could not bear to walk on it long together without much uneaſineſs; and many were ſenſible of the ſame inconvenience as well as myſelf.

On *Wedneſday* the 11th, which was the hotteſt day of all, my Thermometer in the ſun's rays ſtood 11° above the heat of human blood; and in the ſhade, in my houſe, only 8° below it. The diſtance between freezing and the heat of human blood being divided into 100 parts.

An inch of tallow, $\frac{6}{10}$ of an inch in diameter, liquefied in the ſun in leſs than 30′. A piece of reſin, $\frac{5}{10}$ of an inch in diameter, became ſo ſoft as to be liable to take any impreſſion in the ſame time.

But, that you may form a better judgment of the heat at *Norwich*, on the ſaid 11th of *July*, and for 3 days before and after, you will ſee below how *Hauksbee*'s Thermometer ſtood at different times in each of thoſe days.

July

July 8	Morning	7	33	
	Evening	10	25	
July 9	Morning	7	26	
	Evening	2	15	
	Evening	10	18	
July 10	Morning	7	24	
	Evening	2	11	
	Evening	10	16	
July 11	Morning	7	19	*Reaum. Fahr.*
	Evening	2	$8\frac{1}{2}$	= 1026 = 83
	Evening	10	16	
July 12	Morning	7	18	
	Evening	2	12	
	Evening	10	16	
July 13	Morning	7	18	
	Evening	2	12	
	Evening	10	16	
July 14	Morning	7	32	
	Evening	2	13	
	Evening	10	29	

I obſerve 3 in the afternoon, when the ſky is clear, is the hotteſt part of the day; but clouds moſtly came on about that time on theſe days.

Many people here, who judged by their outward ſenſes only, without paying any regard to Thermometers, have thought the 11th of *June* 1748 was hotter: but I imagine the reaſon to be, that the heat this year came on gradually from day to day; whereas in the year 1748 it was much more ſudden; the Thermometer then riſing 22° more in one day than the preceding; which, conſequently, would make the difference between one day and another appear the more extraordinary. But, by my obſervations on the 11th of *June* 1748, *Haukſbee*'s Thermometer ſtood at $14\frac{1}{2}$; full 6° cooler than on the 11th of this preſent *July*.

P. S. Several horſes have dropped down dead under their maſters, overcome by this violent heat.

XIV. The preceding day had been remarkably hot, and in the afternoon very cloudy, with the uſual indications of an approaching ſtorm, in the evening. At 9 at night, the wind ſoutherly, my Barometer ſtood thus; one, which is the moſt ſenſible, at 29 inches $\frac{7}{10}$ $\frac{3}{100}$; the

A Letter from the Rev. H. Miles, *D. D. F. R. S. to the* Preſ. *concerning the* Storm *of* Thunder, *which happened* June 12. 1748. *at* Streatham *in* Surry, N°. 488. p. 383. June 1748. *Read* June 23. 1748.

other at 30. $\frac{2}{10}$ 0. The Thermometers (of *Siſſon*'s conſtruction) one without-doors, at 43°, another within, at 49°, above 0. or the freezing point.

At 1 next morning, a perſon apprehenſive of the thunder, upon looking out at window, was ſurprized to find an unuſual clear ſky, every-where equal to what is obſerved in froſty weather, or after a high wind, except that in a few places ſome thunder-clouds ſhewed themſelves juſt above the horizon. At 2 we heard thunder at a diſtance: at 3^h 30′, when I got up, I perceived the ſtorm approaching apace from the S. where the wind then was, but the darker clouds ſeemed to bear off chiefly to the E. and W. of us, ſo that I did not think we ſhould hear of any miſchief near us. At 4 we had a ſmart ſhower of rain, and about 5 two loud claps of thunder over our heads, but pretty high; the lightning was very pale, and the flaſhes large, deſcending in a ſpiral form, almoſt perpendicular to the horizon to the eaſtward of us, which is the ſituation of *Stretham*, and at about 2 miles diſtant from us. At a little before my Barometers ſtood thus, 29. $\frac{7}{10}$ $\frac{3}{10}$, and 30. $\frac{2}{10}$ $\frac{2}{100}$; and continued ſucceſſively riſing and falling during the ſtorm, but very inconſiderably.

Upon hearing 2 houſes were damaged, ſituate at the foot of the hill on which the mineral wells are, fronting the eaſt, by the wood-ſide, I went next day to view them. The houſe to the S. which is a publick houſe kept by Mr *Howard*, ſeemed to have received the greateſt ſhock. Some of the family being up, the front door ſtood partly open, when the ſtorm began: the upper half was of glaſs, framed like a ſaſh-window, having two ſliding ſhutters, one on each ſide, which had not been taken down. The glaſs between them was ſhattered to pieces, but the ſhutters no-ways touched, except that a nail in one of them was forced in a little way. To the door-poſt, on the left hand, hung by an iron pin an iron bar, which ſerved to faſten the door at night: this pin was driven out of the poſt, and the bar conſiderably bent, and in divers places melted in ſmall ſpots, as were the hinges of the door, chiefly upon the edges in both, and the door-poſt ſplit. A ſheet of lead on the pediment, or ſhelter over the ſaid door, was raiſed, and partly rolled up at one corner; the cornice underneath being torn off without being ſplit, a good part of the tiling near the eaves and over the pediment was looſened, and ſome tiles beat off, and the lathing and ſome of the moldings of the windows had taken fire.

In a bed-chamber fronting the road, on the ſecond floor where Mr. *Howard* lay, 3 boards of the lining of the room, on the E. ſide, were driven inwards 5 or 6 inches at one end; but at the other the nails were a little looſened only. In a garret over this bed-chamber, the upper part of a bed-poſt was ſhivered; and nearly over where this bed ſtood, a large hole was broke in the roof, on the W. ſide, juſt by where one of the chimneys goes up; the chimneys having all additional funnels of brick-work on the top, of a roundiſh form, and plaſtered: theſe were

ſtruck

ſtruck, and inclined to the N. eſpecially that which was on the ſouth end of the houſe, the plaſter being beat off, and ſome of the bricks broke down. There were about 13 perſons in this houſe, none of which received any hurt; though a lad, who was in the kitchen, into which the door opened, before-mentioned, and the window of which (near where he was ſtanding) had ſeveral panes of glaſs broke, muſt certainly be much expoſed. He informed me, among other things, that the fire flew about him in ſparks, like thoſe which fly out of burning charcoal, but larger, and ſnapping as they do. Some pieces of glaſs were ſhewed me, which I found to have been melted, one of which I take the liberty of laying before you.

The adjoining houſe, inhabited by Mr *Figgins*, had the plaſtering beat off in the front in patches, and one of the chimneys cracked for a great length In the kitchen window frame, one of the croſs pieces, near the middle of the window, had a chip ſtruck off from it about 5 inches in length, and at one end about $\frac{1}{4}$ of an inch thick, but thin at the other, and near the width of the frame, but none of the glaſs broke, nor the lead bent, though in a manner contiguous with the ſplinter beat off. The ſame thing happened to a parlour-window, on the other end of the houſe; both the ſhivers were found directly oppoſite to the windows, at ten or twelve yards diſtant in the road.

In a ſmall garret (which is next to Mr *Howard*'s houſe) where two maid-ſervants lay, the plaſter was broken, to appearance, inwards, on oppoſite ſides of the room, and near the feet of the bed, which ſtood on each ſide about $\frac{3}{4}$ of a yard from the wall. The breach on the E. ſide, near a window (ſome panes of the glaſs of which were broken) was oppoſite to the vailings of the bed, which were ſinged, and a hole burnt through them big enough to receive the end of one's fore finger. On the oppoſite ſide, juſt by the chimney, another breach was made, of the ſame height, in the wall, which was continued downwards for about a yard, but the curtains not at all ſinged. Directly againſt this breach, one of the maids (who had got up) ſat on the bed's ſide, who was inſtantly ſtruck down, but received no hurt: Upon enquiring of her, whether ſhe ſeemed to receive a blow on any particular part of her body? ſhe replied, ſhe was ſtruck all over alike.

But the moſt remarkable, though the leaſt terrible effect, appeared on the frame of a pannel of wainſcot, about 5 feet long, and about $1\frac{1}{2}$ wide, in the parlour fronting the E: on this pannel a landſcape is painted, and the moulding belonging to it had been gilt, but on the laſt painting the room, the gilding was covered with the ſame paint: that which covered the gilt moulding was ſtripped off in irregular ragged ſtreaks throughout, ſo that the gilding appeared as freſh as it may be thought to have looked when it was painted at firſt: and as the gilding does not ſeem to have been affected, ſo neither does the paint appear to have been cracked any-where, but where the gilding lay under.

If it be ſuppoſed, that the lead in the paint was melted by the lightning, it will be difficult to account for it, that it ſhould *not at all affect* the paint contiguous with that which was upon the gilding; though we ſuppoſe a reſiſtance to have been made by the leaf-gold, and to have contributed to the producing the mentioned effects.

Of the burning of the ſteeple of Danbury *in* Eſſex, *by* Lightning; *by* Smart Lethieullier, *Eſq*; *F. R. S. to the* Preſident. N°. 497. p. 611. *Read* March 1. 1749-50.

XV. As I think there was ſome miſtake in the account of the late burning of *Danbury* ſteeple by lightning (read laſt *Thurſday* before the *Royal Society*) which miſtake ſeemed to give room to imagine it had ſome connexion with the late earthquake, I take the liberty of ſending you the particulars of the aforeſaid accident, as they were tranſmitted to me by two gentlemen of diſtinction in the pariſh.

Monday, the 5th of this inſtant *Feb.* about 3 in the afternoon, a very great and black cloud paſſed over the hill on which *Danbury* ſtands, and broke into a violent ſtorm of thunder, lightning, and rain. No immediate danger was apprehended that night; but, between 4 and 5 next morning, ſome perſons perceived the ball on the top of the ſpire (which was of wood, and on which the weather-cock ſtood) to be on fire, they immediately alarmed the neighbourhood, and, by the help of a large fire-engine, fetched from *Chelmsford*, they got the maſtery of the fire by about 11 at noon; tho' it broke out twice afterwards. It burnt downwards with great fury, and has entirely conſumed 11 feet of the ſpire, and damaged 8 feet more, as likewiſe the beams on which the ſpire was fixed.—— Some of the burnt timbers and melted lead have hurt the roof of the chancel, but not very conſiderably.

The great height of the ſituation of this church probably expoſes it to accidents of this nature; for *Walſingham* relates, " that, on *Corpus* " *Chriſti* day anno 1402. the devil entered this church in the likeneſs " of a Friar-Minor, where he raged, to the great terror of the pariſhi- " oners, and at the ſame time, by the violence of thunder and a tem- " peſt, the whole body of the church was broken."

This devil, or friar, was, I conclude, no more than a flaſh of ſome fiery meteor, which the fruitful fancy of thoſe times could immediately cloath with the firſt habit that occurred to them.

The appearance of a fiery Meteor, *as ſeen by Mr* Cradock, *communicated to the* R. S. *by Mr* H. Baker, *F. R. S.* N°. 473. p. 78. May, &c. 1744. *Read* June 7. 1744. Fig. 34.

XVI. The head and body emitted an extremely lucid and white flame. The tail appeared of a tranſparent blue, like the flame of ſulphur.

This *phænomenon* was ſeen on *Sunday*, *May* 27. 1744. at $11^h\ 11'$ *p. m.* It's direction from *S. E.* to *N. W.* or thereabouts; it's height ſeemingly not $\frac{1}{2}$ a mile.

It was ſeen, as here deſcribed, from the terrace in *Somerſet-Gardens*, by me,

Zach. Cradock,
Of *Somerſet-Houſe*.

XVII.

XVII. As I was coming from my Living, juſt before I reached a place called *Stanlake Broad*, and a little before 8 in the evening, I was on a ſudden ſurprized to ſee a long ſtream of fire, of a colour reſembling molten glaſs, *Fig.* 35. which ſhot down from *A* to *B*, in length, I gueſs, about 20°, and ſeemed immediately to run up again from *B* to *A*; where it turned to a ſort of ſmoke, or rather to a fine lambent flame like that of an *Aurora Borealis*; which continued for ſome time in a ſort of oblong ſhape, *Fig.* 36. but afterwards by degrees, changed into *Fig.* 37. and at laſt into *Fig.* 38. under which, parallel to the horizon, it grew fainter and fainter, till it intirely vaniſhed about 9.

There was a fine gentle breeze all this time; but I could not obſerve that it affected the *phænomenon* ſo far as to make it change it's place, which was to the eaſtward of the N. Perhaps this change of figure might, in ſome meaſure, be owing to it. Being on horſeback, I ſaw it from the beginning to the end; but having no watch with me, I only gueſſed, by my riding, that it continued about an hour. I heard afterwards, from ſome who had their watches, that it laſted an hour and one minute.

When I came home, I put down what I had obſerved; it being, as I thought, a very ſingular appearance *.

Part of a Letter from the Rev. Mr Geo Coſtard *to Mr* John Catlin, *concerning a fiery* Meteor *ſeen in the air on* July 14. 1745. N°. 477. p. 522. Aug. &c. 1745 *dated* Aug. 2. 1745. *Read* Nov. 7. 1745.

Fig. 35.
Fig. 36.
Fig. 37.
Fig 38.

XVIII. *Sept.* 15. 1749. a remarkable meteor was ſeen in *Rutland*, which I ſuſpect to have been of the ſame kind as ſpouts at ſea.

It was a calm, warm, and cloudy day, with ſome gleams and ſhowers; the Barometer low and falling, and the wind S. and ſmall. The ſpout came between 5 and 6 in the evening; at 8 came a thunder-ſhower, and ſtorm of wind, which did miſchief in ſome places; and then it cleared up with a briſk N. W. wind.

The earlieſt account I have was from *Seaton*. A great ſmoke roſe over or near *Gretton*, in *Northamptonſhire*, with the likeneſs of fire, either one ſingle flaſh, as the Miller ſaid, or ſeveral bright arrows darting

An extraordinary Meteor *ſeen in the County of* Rutland, *which reſembled a* Water-Spout, *communicated to the* Preſ. *by* Tho. Barker, *Eſq*; N°. 593. p. 248. Oct. &c. 1749. *Read* Dec. 14. 1749.

* As I was returning home from the *Royal Society* to *Weſtminſter*, on *Thurſday*, *Dec.* 16. 1742. 8h. 40′. *p. m.* being about the middle of the parade in *St James's Park*, I ſaw a light ariſe from behind the trees and houſes in the S. by W. point, which I took at firſt for a large ſky-rocket; but when it had riſen to the height about 20°. it took a motion nearly parallel to the horizon, but waved as in *Fig.* 39. and went on to the N. by E. point over the houſes. It ſeemed to be ſo very near, that I thought it paſſed over *Queen's-Square*, the iſland in the park, croſs the canal, and I loſt ſight of it over the *Hay-market*. It's motion was ſo very ſlow, that I had it above ½ a minute in view; and therefore had time enough to contemplate it's appearance fully. *A* ſeemed to be a light flame, turning backwards from the reſiſtance the air made to it. *B B* a bright fire like burning charcoal, incloſed as it were in an open caſe, of which the frame *C C C* was quite opaque, like bands of iron. At *D* iſſued forth a train or tail of light flame, more bright at *D*, and growing gradually fainter at *E*, ſo as to be tranſparent more than half it's length. The head ſeemed about ½° in diameter, the tail near 3° in length, and about ⅛ of a degree in thickneſs.

Fig. 39.
Fig. 40.

C. M.

down

down to the ground, and repeated for ſome time, as others ſay. Yet ſome who ſaw it, did not think there was really any fire in it, but that the bright breaks in a black cloud looked like it. However, the whirling, breaks, roar, and ſmoke, frightened both man and beaſt. Coming down the hill, it took up water from the river *Welland*, and paſſing over *Seaton* field, carried away ſeveral ſhocks of ſtubble; and croſſing *Glaiſton*, and *Morcot* lordſhips, at *Pilton* town's end, tore off two branches, and carried one of them a good way. In a hedge-row in the meadow, at right angles to the ſpout's courſe, ſtood an oak and an aſh 15 yards aſunder; the oak a young ſound one, 16 inches thick, it ſplit two yards down, and one half fell to the ground, but was not quite parted from the other; the aſh, about 8 inches thick, was torn off in the middle, and carried 10 or 12 yards. Between and on each ſide of theſe trees were other ſmaller ones, which were not hurt: I heard of no harm it did after, but breaking and ſcattering a few boughs. I ſaw it paſs from *Pilton* over *Lyndon* lordſhip, like a black ſmoky cloud, with bright breaks; an odd whirling motion, and a roaring noiſe, like a diſtant wind, or a great flock of ſheep galloping along on hard ground; it was divided into two parts all the way it went, and though there was no wind, moved apace from S. by W. to N. by E. As it went by a quarter of a mile E. from me, I ſaw ſome ſtraws fall from it, and a part, like an inverted cone of rain, reached down to the ground. Some who were milking, ſaid it came all round them like a thick miſt, whirling and parting, and, when that was paſt, a ſtrong wind for a very little while, though it was calm both before and after. It then paſſed off between *Edithweſton* and *Hambleton*, but how much further I do not know.

An account of an extraordinary Fire-ball burſting at Sea, communicated by Mr Chalmers. N°. 494. p. 366. Jan. &c. 1750. *Read* March 22. 1749.

XIX. *Nov.* 4. 1749. in lat. 42° 48′, long. 09° 03′, the Lizard then bore, N. 41° 05′, about the diſtance of 569 miles. I was taking an obſervation on the quarter-deck, about 10′ before 12: one of the quarter-maſters deſired I would look to windward, which I did, and obſerved a large ball of blue fire rolling on the ſurface of the water, at about 3 miles diſtance from us: we immediately lowered our topſails, and had our fore and main clew-garnets manned to haul up our courſes; but it came down upon us ſo faſt, that before we could raiſe the main tack, we obſerved the ball to riſe almoſt perpendicular, and not above 40 or 50 yards from the main chains: it went off with an exploſion as if hundreds of cannon had been fired at one time; and left ſo great a ſmell of brimſtone, that the ſhip ſeemed to be nothing but ſulphur. After the noiſe was over, which I believe did not laſt longer than half a ſecond; we looked over head, and found our maintopmaſt ſhattered into above an hundred pieces, and the mainmaſt rent quite down to the heel. There were ſome of the ſpikes, that nail the fiſh of the mainmaſt, drawn with ſuch force out of the maſt, that they ſtuck in the main deck ſo faſt, that the carpenter was obliged to take an iron crow to get them out:

Pla. VIII. *Vol.* X. *Part* II. *Pag.* 480.

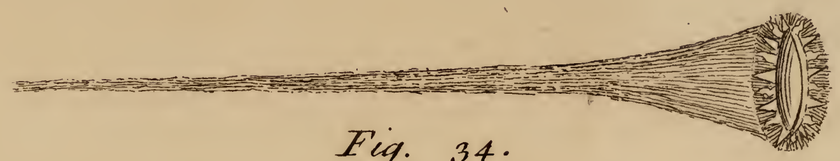


Fig. 34.

Fig. 35.

Fig. 36.

Fig. 38.

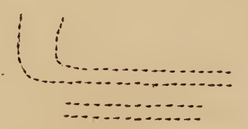


Fig. 37.

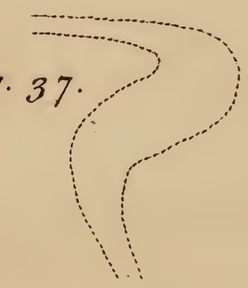
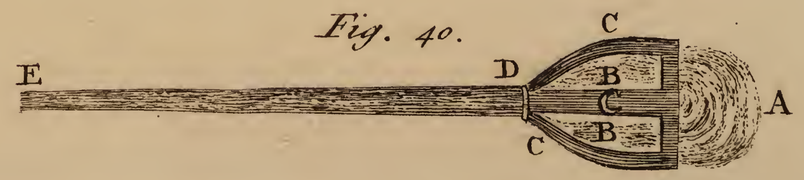


Fig. 39.

Fig. 40.



out: there were five men knocked down, and one of them greatly burnt, by the explosion. We believe, that when the ball, which appeared to us to be of the bigness of a large millstone, rose, it took the middle of the main topmast, as the head of the mast above the hounds was not splintered: we had a very hard gale of wind, from the N. by W. to the N. N. E. for two days before the accident, with a great deal of rain and hail, and a large sea: from the northward we had no thunder nor lightning, before nor after the explosion. The ball came down from the N. E. and went to the S. W.

This account was given by Mr *Chalmers*, who was, when the above-mentioned accident happened, on board his Majesty's ship the *Montague*, under the command of admiral *Chambers*.

XX. On the 8th of *March* 1746-7, near 8 in the morning, as I was riding within 3 miles of *Brentwood* in *Essex*, there appeared a singular *phænomenon* in the Heavens; the sketch may give some idea of it.

An observation of an uncommon Gleam of Light proceeding from the Sun, by Mr Peter Collinson, F. R. S. N°. 483. p. 456. Mar. &c. 1747. *Read* March 19. 1746-7. Fig. 41.

The morning was fine and clear, the sun shone bright, no cloud to be seen, but the air a little hazy: where the *phænomenon* appeared, which was a bright cloudy spot, seemed a very small portion of a rainbow, only the colours very faint. It was in a horizontal direction north of the sun, and from it projected a long luminous ray, which terminated in a point.—It continued very strong for more than half an hour after I saw it, and then vanished away by degrees.

XXI. *July* 11. 1749. The sun's rays shone through the clouds at the same time, as they frequently do when the sun is near the horizon. The drawing, which represents the whole, makes any farther description of it needless.

A Halo or mock Sun, observed by Mr W. Arderon, *F. R. S.* Fig. 42.

N°. 493. p. 203. Oct. &c. 1749. *Read* Nov. 2. 1749.

XXII. *Monday July* 18. 1748. about $\frac{1}{4}$ before 7 in the evening, the weather being temperate, and the wind about N. N. W. as I was walking in the fields, beyond *Islington*, I saw a distant rainbow which appeared to take in a large portion of the Heavens; but had nothing remarkable, and vanished by degrees.

A description of an extraordinary Rainbow, *observed* July 15. 1748. *by* Peter Daval, *Esq; Sec. R. S.* Ibid. p. 193. *Read* Oct. 26. 1749.

Continuing my walk, about 20′ after the disappearing of the first rainbow, a rainy cloud crossed me, moving gently with the wind, which exhibited to me a more perfect and distinct rainbow, than I had ever before seen; wherein I could plainly distinguish all the secondary orders of colours taken notice of by the late Dr *Langwith* in his letters to Dr *Jurin*; that is, to say, within the purple of the common rainbow, there were arches of the folowing colours.

1. Yellowish green, darker green, purple. 2. Green, purple. 3. Green purple.

This innermoſt arch Dr *Langwith* calls faint vaniſhing purple, and I likewiſe found, that it ſometimes appeared and diſappeared alternately; but during about 2′ it ſeemed to me to be as permanent as any of the other colours.

I ſtood ſtill, and looked attentively at this appearance, during the whole time of it's continuance, which was near 8′, and could for the greateſt part of that time diſcern all the above-mentioned colours, except the innermoſt purple in the upper parts of the bow; but could not diſtinguiſh any of them in thoſe parts of it which were near the horizon, tho' they were extremely vivid, as was likewiſe the outer bow, in which the colours appeared as bright, though not ſo well defined, as in moſt inner rainbows I had ſeen.

As I had read Dr *Langwith*'s letter a ſhort time before I ſaw this beautiful appearance, and as I compared his account with what I had ſeen, the ſame evening, and again the next morning, I can the better be aſſured of the exact agreement of our obſervations.

On my firſt ſeeing this phænomenon, I was ſurprized, that the diameter of the bow appeared to me very ſmall, compared with that I had ſeen a little before. The occaſion of this I think muſt have been, that the legs of the firſt-mentioned bow appeared to me to terminate at diſtant places: whereas in the latter appearance I could plainly ſee both ends of the inner and outer bows terminated in the neighbouring fields, at a very ſmall diſtance from each other: hence, and from my being involved in the ſhower which occaſioned this rainbow, I conclude it was very near me; which might be one cauſe of the great vividneſs of it's colours, and of my diſtinguiſhing the inner arches. But whether this was the only cauſe of thoſe appearances, or whether they might not be owing to ſome particular diſpoſition of the atmoſphere at that time, I much queſtion: as well becauſe I have often ſeen rainbows which have been very near me, and oppoſed to a bright ſun, wherein I could not diſcern theſe inner orders of colours, as that I have heard from ſome intelligent perſons, that ſome very bright rainbows were ſeen ſoon after the ſolar eclipſe which happened on the 14th of *July* 1748. particularly that an unuſually vivid and diſtinct rainbow was obſerved at *Twickenham* 3 or 4 days after that eclipſe, which agrees with the day on which I ſaw the above-mentioned appearance.

A luminous arch, by the Rev. W. Cowper, *D. D. Dean of* Durham. N°. 497. p. 648. *Read* Feb. 16. 1749.

XXIII. A luminous arch appeared *Feb.* 16. 1749. about 9 at night. It had *Arcturus* in it's eaſtern limit, which was then low in the horizon, and extended to the bright ſtar in the right ſhoulder of *Orion*, then bearing S. W. It was exceedingly bright, regular, and well defined, and about the breadth of the rainbow; which it reſembled in every thing, but it's variety of colours. It continued thus for almoſt 20 minutes, declining gently ſouthward, and then gradually ſeparated and diſappeared.

XXIV.

XXIV. *Oct.* 20. at night, the ſky was darkened by a ſlight fog, thro' which the moon appeared of a fiery red colour, till 8^h. 40′. when the fog was thoroughly diſperſed, and the Heavens were overcaſt with a whitiſh ſtreaky cloud. At the ſame time there appeared round the moon a halo (*Fig.* 43. *A B C D*) accompanied with four other ſegments of circles, two of which *E A F* and *G H* of 10°, were concentric, ſo as to have their common center at the Zenith. The ſegment or arch *I P L* on the N. ſide, of 7°, was concentric with the great lunar circle, and conſequently had the moon for it's centre; and in fine the arch *M C N*, which faced the horizon, was of 12°.

An obſervation of an extraordinary lunar circle, *and of two* paraſelene's, *made at* Paris, Oct. 20. 1747. N. S. *by* Aug. Nath. Greſchow, *Memb. of the* R. Acad. *of* Sc. *at* Berlin, *&c.* No. 489. p. 524. Oct. & Nov. 1748. *Tranſlated from the* French *by* T. S. *M. D. Read* Nov. 10. 1748.

Beſides theſe 4 ſegments, what was moſt remarkable was a mock-moon or *paraſelene B*, ſhaped like a mock-ſun or *parhelius*. The diameter of this mock-moon, tho' ill-determined, was of 35′ at leaſt, with a tail *B P* oppoſite to the moon, as the tail of a comet is oppoſite to the ſun. This tail varied in it's degree of light from time to time, extending as far as the arch *I P L*, which, as well as the arch *G H*, was 4° diſtant from the lunar circle *A B C D*. The *paraſelene B* had the ſame colours with a common *parhelius*, excepting that they were not ſo lively, but they very much inclined to the tawny, eſpecially on the ſide, which faced the moon. This *paraſelene* was in the ſame altitude as the moon. It's tail was much more faint and tranſparent; inaſmuch as *Capella* appeared thro' this luminous tail. The lunar circle *A B C D* was much weaker to the ſouth, and there appeared no *paraſelene* on that ſide. This meteor did not ſeem to undergo any alteration till 9^h. 18′. when the atmoſphere was covered with thick clouds.

Fig. 43.

The clouds being diminiſhed at 9^h. 32′. the meteor appeared again, but very different from what it was before; for, inſtead of ſeeing a lunar circle with 4 other arches of circles, I ſaw the lunar circle *D A B D* and on the ſouth ſide a faint arch *Q R* of 4°, having the moon for it's centre in common with the great lunar circle. There were likewiſe two *paraſelene's*, one of which *B* was to the N. and the other *D* to the S. as they are expreſſed in *Fig.* 44. Theſe two *paraſelene's* did not caſt ſo ſtrong a light as that which had appeared before, nor were they ſo diſtinctly formed. On the contrary, the lunar circle was very beautiful, and remarkably bright, until 9^h. 50′. when the whole *phænomenon* diſappeared, and the ſky grew clear by degrees. The moon's diameter was 30′. 30″. On the ſame night a very beautiful lunar circle was obſerved at *Berlin*, but without *paraſelene's*.

Fig. 44.

XXV. 1. On *Friday* the 16th there was a bright *Aurora Borealis*, the northern part of the ſky being entirely filled with a pale light, in which frequent coruſcations were viſible. Beſides theſe lights, there was a perfect uniform arch, extending from E. to W. the colour of it was the ſame with that of the *Aurora*; with which however it did not ſeem to have any communication, being placed ſeveral degrees to the ſouthward. The ſhoulders of *Orion* were viſible thro' this luminous arch,

An Aurora Borealis *ſeen at* Chelſey, Feb. 16. 1749-50. *by* John Martyn, *F. R. S.* Prof. Bot. Cantab.

N°. 494. p. 345. Jan. &c. 1750. *Read* Feb. 22. 1749 50.

in the weſtern part of it, and *Cor Leonis* in the eaſtern part. I did not happen to ſee it till about a quarter before ten; and at ten it diſappeared. The weather was then, and has been ever ſince, exceedingly warm for the ſeaſon. The walls are covered with bloſſoms; and the hyacinths, daffodils, *&c.* are blown before the uſual time.

——*at* Tooting, Jan. 23. 1750-1, *by the Rev.* Hen. Miles, *D. D. F. R. S.* ibid. p. 346 *Read* Feb. 22. 1749.

2. On *Tueſday Jan.* 23. laſt, I was called out, about ſix in the evening, to ſee a ſtrange appearance in the ſky, in the weſt. Suſpecting it to be an ordinary *Aurora*, I did not make great haſte—When I came out, I ſaw a cloud (not large) of an obſcure red colour, but much deeper than any I had ever ſeen before, which, I was informed, roſe from the S. W. it was then advancing apace to the N. E. and quickly reached the zenith, when, it being intercepted by the houſe, I haſtened to the other front, which regards the N. E. by which time there appeared a luminous zone, about the breadth of the galaxy, it's edges regularly defined, compaſſing the hemiſphere, from the horizon in the N. E. to the zenith, in the ſame direction, in which the above-mentioned cloud had paſſed (as far as I ſaw it's courſe) from the S. W. The colour was much fainter, and more luminous, reſembling the uſual colour of an *Aurora*, and the *laminæ* or ſtreamers ſoon appeared—upon this, not being well fenced againſt the wind, which blew briſk, I went in, to purſue my intention of viewing the 2 beautiful planets, *Jupiter* and *Venus*, with a reflecting teleſcope, made by my ingenious friend Mr *Short* of *Surry-ſtreet* (the greateſt magnifying power of which is about 200 times); and after I had viewed them to my ſatisfaction, and ſhewed them to ſome friends, when I was about to put up the inſtrument, a cloud, of near the ſize of the firſt, but not ſo deep a colour, appeared, riſing up from the S. W. which proceeded in a line with the planets, and, in a little time, ſurrounded both: *Venus* appearing ſtill, to the naked eye, in her full luſtre, I immediately viewed her with the teleſcope, without altering the *focus*, and ſaw her much more diſtinctly than ever I had done, on that evening, or on any other, and of the ſame opinion were all my friends as to the ſight they had of her, on that occaſion: we all ſaw her ſpots plain, reſembling thoſe in the moon; which I was never ſo happy as to have a ſight of before: and this, while the cloud ſeemed to ſurround it, as much as ever: but whether the vapour might be really rarer near the planet, than it was at ſome diſtance, no judgment could be made, becauſe of her too powerful light.

Many have obſerved the fixed ſtars to appear thro' the vapour with an undiminiſhed light oftentimes: and our great Dr *Halley* tells us, in his account of that remarkable *Aurora*, which was ſeen in *March*, 1715-16. that he obſerved "one of the *laminæ* paſs ſucceſſively over "all the ſtars of the Little Bear, without effacing the ſmaller ones, in "the tail, of the fifth magnitude; ſuch was the extreme rarity and "perſpicuity of the matter whereof it conſiſted."

I this week received a letter from Dr *Short* of *Sheffield*, in which he ſays, "The 23d paſt, at ſix at night, the ſky being overclouded, all

"the

" the clouds over the hemiſphere, turned of a ſudden to a deep blood-
" red colour, for 15′. then ſucceeded red ſtreamers for half an hour."

3. *Feb.* 15. 1749-50. in the evening there was a very vivid northern light, which darted forth ſeveral beautiful, crimſon, and fiery-coloured rays; wind N W *b* N 1, Barometer 30.2; 50 minutes paſt 8 a ſurpriſingly bright and exceedingly white arch, about the breadth of a common rainbow, appeared in the Heavens, extending nearly from E. to W. it reached within 5 or 6° of the weſtern horizon, and ended about 8 or 10 above the eaſtern. It paſſed exactly between *Caſtor* and *Pollux*, and directly over *Aldebaran*, which appeared plainly through it. Near the top of the arch ſeveral very lucid, white, ſhort, vibrating columns were attached to it; none of them ſeemed above 6 or 7 degrees long, and did not appear to communicate in the leaſt with the *Aurora borealis*. *Fig.* 45. ſomewhat repreſents it. About 9^h. 12′ the arch vaniſhed; but ſeveral white, bright, coruſcating *nubeculæ* remained here and there in the zodiac for 12 or 15 minutes longer. The *Aurora borealis* continued more or leſs till midnight: the next morning the wind was E. 1^x, Barom. 30.1.

——*at* Plymouth, Feb. 15. 1749-50. *by* John Huxham, *M. D. F. R. S.* N°. 495. p. 472. Apr. &c. 1751. *Read* June 21. 1750.

Fig 45.

4. *Feb.* 16. about 7 *p. m.* we had another *Aurora borealis*, though not quite ſo fiery and luminous as that of the night before: it continued till near 11. At 8^h 56′ *p. m.* exactly, ſuch another arch appeared, very nearly of the ſame extent and direction, but not altogether ſo broad or lucid. This at firſt alſo paſſed between the two bright ſtars of *Gemini*, but declined more and more to the ſouthward, till it was 2 or 3° to the S. of *Pollux*. It's weſtern limb, about 9, paſſed thro' the N. ſhoulder of *Orion*: it quite diſappeared about 10 or 12 minutes after. This had no columns attached to it, as the former; tho' it was ſomewhat jagged and unequal towards the N. near the *vertex*. The wind this evening was E. 2; the Barometer 29.9. The next morning the wind was SW*b*W 1, the Barometer 29 9. Neither any part of the arch, or the attached rays were coloured, but perfectly white, and exceeding bright.

——Feb. 16. *by the ſame.* Ibid. p. 473.

5. On *Monday Jan.* 23. laſt, ſome unuſual appearances were obſerved in the ſky, at *London*, and the towns about it, by thouſands of people during the whole evening, of which ſome accounts were laid before the *R. S.* and as appearances of the like kind were obſerved in the Heavens, the ſame evening, at great diſtances from *London*, I take the liberty to preſent you a deſcription of what was ſeen at the city of *Norwich*, by Mr *W. Arderon*, F. R. S.; and alſo of what was obſerved at *Wells* (a little ſea-port town in the ſame county of *Norfolk*, about 30 miles nearly due N. from *Norwich*) by Mr *Joſeph Sparſhal*, and ſent by him to Mr *Arderon*, who communicated it to me.

——*at* London, Jan. 23. 1749-50. *by* Mr Hen. Baker, *F. R S.* ibid. p. 499. *Read* June 28. 1750.

6. The wonderful appearances of the *Aurora borealis* on *Jan.* 23, laſt, have been taken notice of in moſt parts of *England*, though in different forms. At *Norwich* I believe it was as extraordinary as at any place whatever: but the weather being very cold, and myſelf ſomewhat indiſpoſed,

——*at* Norwich, Jan. 23. 1749-50. *by* Mr W. Arderon, *F. R. S.* ibid. p. 500.

diſpoſed, I did not make all the remarks I could have wiſhed: the few I did make are as follow:

This wonderful *Aurora* began at 6 in the evening, with a blackiſh cloud in the N. E. out of which ſprung up a ſtreak of ſcarlet-coloured rays, of a ſurprizing beauty and vividneſs. This preſently extended to within a few degrees of the S. W. horizon, paſſing directly thro' the *zenith*, and ſo continuing near a quarter of an hour, when red and yellow columns began to riſe upwards from every quarter.

At 7 a black cloud roſe up in the S. E. and quickly put on a ſemicircular form, with light yellowiſh vapours aſcending out of it's upper edge, and repreſenting a glory of an uncommon brightneſs.

At 8 the black cloud was diſperſed, but the yellow glory remained; and round that ſprang up another circle of red, which made the whole appear very tremendous.

The reddiſh ſtreams, as well as this laſt-mentioned circle, were ſometimes ſo denſe, that even ſtars of the firſt magnitude could not be ſeen through them.

There was now-and-then ſome of the flaſhing *Aurora* in different parts of the firmament, though not ſo common as I have obſerved at other times.

The night was full as light as it is when the moon is about 8 days old; but I could compare it neither to the light of the ſun nor moon, ſome of the original colours ſeeming to be wanting: and the beſt deſcription I can give of it is, to liken it to that light produced in a dark room, when one of the ſeven original colours is ſeparated from the reſt, after they have paſſed through a priſm, and been collected together again by a convex lens.

This evening the Barometer was 30.1 falling. *Haukeſbee*'s Thermometer 63. Wind E. $\frac{2}{10}$ force. The morning miſty, and very cold, but all the day clear.

——*at* Wells *in* Norfolk, *by Mr* Joſ. Sparſhal, ibid. p. 502.

7. On *Tueſday Jan.* 23, the air at *Wells* was clear and ſerene during the greateſt part of the day, with a freſh breeze of wind at S. S. E. which terminated in an evening extremely remarkable for appearances in the Heavens of an uncommon *Aurora borealis*.

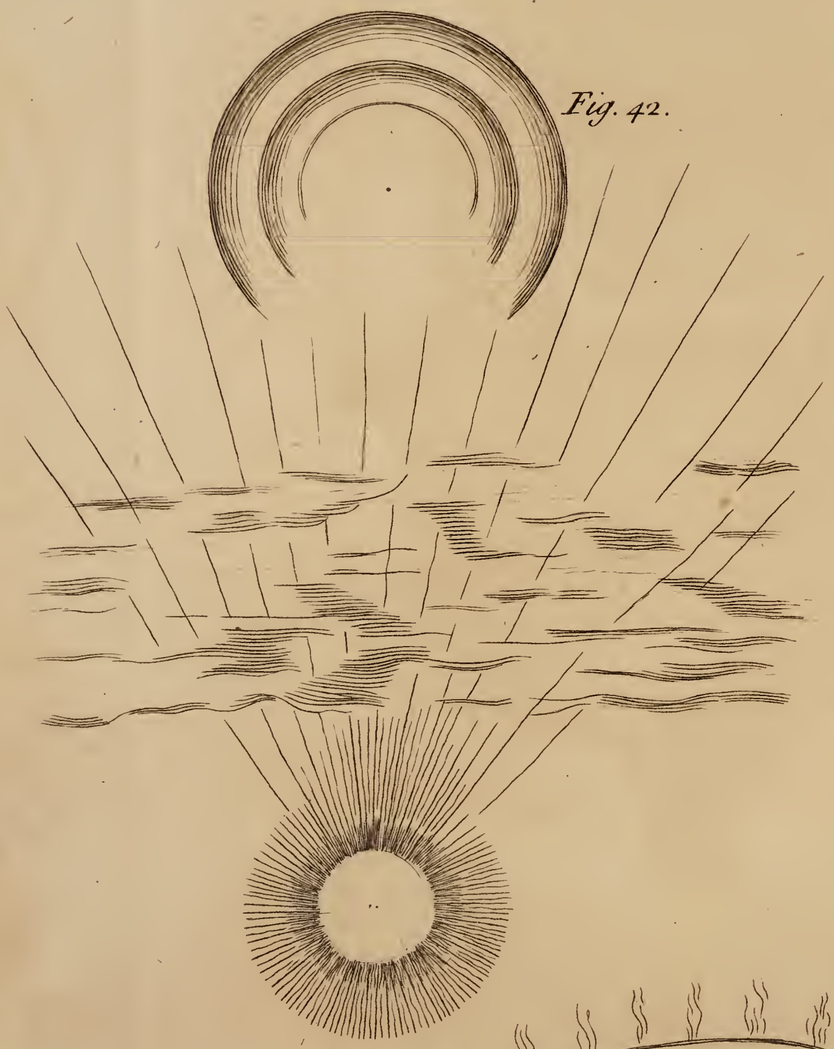
At 15′ paſt 5, I firſt took notice of the foot of an arch, which formed an angle of about 10° with the N. E. part of the horizon. This arch ſhot out pointed ſtreams like pyramids, of a fiery red colour, which generally aſcended within a few degrees of the *zenith*, then vaniſhed, and were immediately ſucceeded by others, from the N. E. where the principal magazine ſeemed to be. They continually ſhifted towards the E. and S. W. with ſudden flaſhings and dartings; but towards the weſt the appearances ſeldom altered.

At 30′ paſt 5, a luminous ſtream, of a bright flame-colour, ſhot up on the N. ſide of the fiery arch, which ſtill kept ſomewhat of that form, though frequently interrupted by ſhooting flaſhes from the N. E.

At

Fig. 41.

Fig. 42.



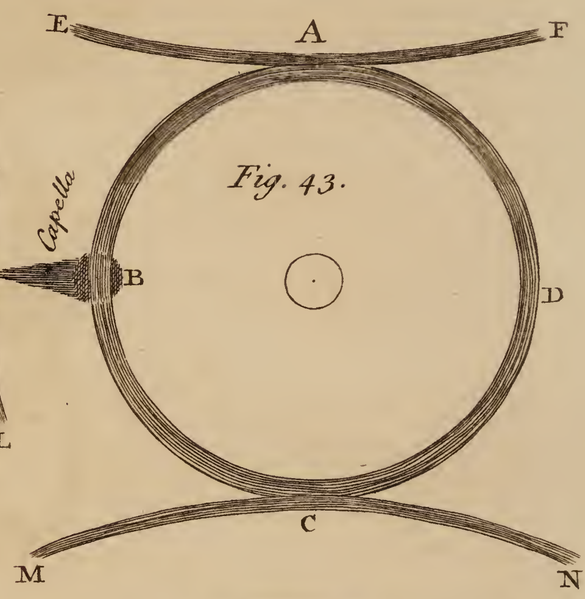


Fig. 43.

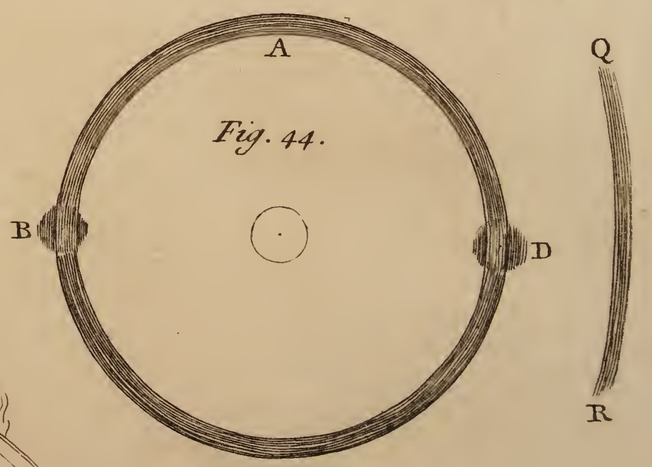


Fig. 44.

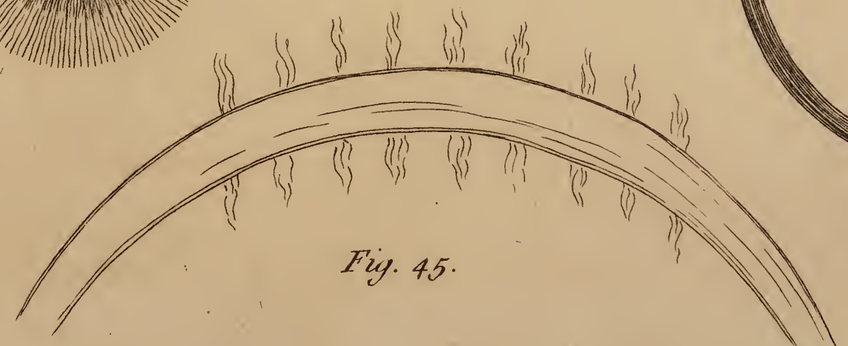


Fig. 45.

At 40′ paſt 5, there appeared ſuddenly in the N. E. an elliptical *corona*, of an amazing brightneſs, elevated about 9° above the horizon, and having it's longeſt diameter parallel thereto. There ſhot up perpendicularly from this, ſtreams reſembling columns of flame intermixed with others of bright red.

At 50′ paſt 5, part of the hemiſphere, included between the N. E. and S. E. was ſtrongly illuminated, with a vaſt number of pointed rays of crimſon and flame-colour, darting towards the *zenith*. Theſe vaniſhing in about 4′, were ſucceeded by many whitiſh ſtreaks, ſhifting from the N. to the S. E.

At 55′ paſt 5, there ſprang up in the N. 3 bright pyramids of flame-colour, aſcending to the height of about 70°, not perpendicular to the horizon, but inclining towards the E. and theſe were joined at the upper parts by an equal number of a blood-red colour from the S. E.

At 6, a pyramid of a crimſon colour roſe from the N. E. to the height of about 60°. This ſoon diſappeared; but a diffuſed redneſs remained; and from the N. E. to the S. W. appeared an arch, crowned at the *vertex* with ſomething reſembling a Glory, with a round body of light in it's centre.

At 20′ paſt 6, the redneſs was contracted into a narrower compaſs, but was much deeper in colour, forming an arch from S. E. to S. W. and appeared at the *zenith* like a fiery ſheet ſpread towards the S; the N. being at the ſame time illuminated with white ſtreams, like the common *Aurora borealis*.

At 30′ paſt 6, a whitiſh ſemicircular arch was formed to the ſouthward, encompaſſing the red lights in the *zenith*, and extending from the E. to the S. W. But this appearance continued only a few moments.

At 40′ paſt 6, the redneſs quite diſappeared in the N. E. and that to the ſouthward became much paler. The common *Aurora* appeared very plain in the N. E. till 7, and then totally diſappeared.

At 8 the common *Aurora* appeared again very bright from N. to E.

At 30′ paſt 8, appeared another large ſemicircular arch, extending from E. to W. pointed columns of a bright red ſhooting up from each ſide of it's baſe; with other ſmaller ones on the upper part of it, ſuch as the common *Aurora*. At the ſame time aroſe in the N. from within a very few degrees of the horizon, a bright pyramidal ſtream of light, of a ſurpriſing magnitude. This appearance continued near $\frac{1}{4}$ of an hour very regular, and exceeding beautiful.

At the beginning of theſe lights the mercury ſtood at 29.9, but quickly fell to 29.8. The wind at S. E. $\frac{7}{10}$ force.

During part of the time I obſerved an uncommon motion in the magnetic needle; but was too cloſely engaged in obſerving the Heavens to take much notice of it.

I ſaw

I saw this evening those meteors called falling stars; particularly one, which, on taking fire, left a long train of sparks behind them.

My situation being quite open to the sea on all sides, except the S. afforded me as favourable an opportunity of viewing the above particulars as I could wish; and I gave up my whole attention to them.

Aurora Australis, *seen* Jan. 23. 1749-50. *at* Chelsey; *by* John Martyn, *F. R. S.* Prof. Bot. Cantab. N°. 494. p. 319. Jan. &c. 1750. *Read* Jan. 25. 1749-50.

XXVI. *Jan.* 23. $17\frac{49}{50}$. at about half an hour after 5 in the evening, casting my eye accidentally toward a window which looked to the S. S. W. I thought I saw a reddish light about the planet *Venus*, which then shone exceedingly bright. Being suspicious of some fire in the neighbourhood, I went immediately to a window on the stair-case, where I saw a reddish light, which shone with such exceeding brightness, that the lustre of the fine constellation of *Orion* was almost effaced. I then went to a window facing the N. N. E. where I presently saw a very broad band of crimson light, like that which I observed from the same window, *March* 18. 1738-9; * but in the former the red band was bounded on the N. by streams of a greenish blue; whereas the band now observed was entirely of a deep crimson colour, being of a much darker red than the former.

Thence I withdrew into my garden, where I plainly saw a band or arch, of a very deep crimson colour, in appearance about 15° broad, the southern edge of which passed just above *Canis minor*, and the shoulders of *Orion*. It was terminated to the westward, near *Venus*, then about 20° high: but it extended to the eastward as far as I could see, and the farther it went that way, the deeper was the colour, and the broader the band. About a quarter before eight, there was formed a crown about 30° to the southward of the zenith; for I could plainly perceive the *Pleiades*, which had then just passed the meridian, very near it, when the light was faint. From this crown a great many rays darted to the E. S. and W. but not toward the N. where only some whitish streaks were to be seen, but very faint. Presently after this, the part of the arch extending to the E. seemed to be suddenly kindled, as if some train had been fired; grew extremely bright and vivid; and as if all the red matter had been then consumed, put an end to the phænomenon before eight.

During the rest of the evening, a pale light covered the S. part of the Heavens, as if the moon had shone.

A Letter from the Rev. Mr John Forster, *to Mr* Henry Baker, *F. R. S. concerning an* Earthquake *at* Taunton, N°. 488. p. 398. June 1748. *Read* June 15. 1748

XXVII. In answer to your inquiries concerning the earthquake, which happened last year on the first day of *July*, when I was at *Taunton* in *Somersetshire*, after taking some pains to inform myself more particularly what other people observed in different places, you may depend on the truth of what follows.

Between ten and eleven o'clock at night, on the said first day of *July* 1747. being myself in some company at *Taunton*, we were suddenly

* See Vol. VIII. P. ii. chap. i. § liv. 1.

surprized

ſurprized with a rumbling noiſe like diſtant thunder, which was followed immediately by ſo conſiderable a motion of the earth, that the chair whereon I ſat rocked under me. The noiſe and ſhaking ſeemed to come from a diſtance, and approached gradually, in ſuch a manner as if a loaded waggon had paſſed along; and continued nearly the ſame time as ſuch a waggon would require to go about 100 yards. The motion went from S. E. to N. W. which being the direction of the ſtreet, on one ſide whereof the houſe ſtood, ſome of us imagined at firſt that a waggon had really gone along; but, upon inquiring, we found there had been no waggon: and indeed, as we were ſatisfied afterwards, no waggon could have been heard or felt in the back room where we ſat, on account of its too great diſtance from the ſtreet.

Notwithſtanding this happened between 10 and 11 at night, when moſt of the town were in bed, the ſhock was ſo ſenſible, that many people got up very much terrified; and they waking others, the conſternation ſoon became general; inſomuch that, although it was a rainy night, numbers of people ran out into their gardens, and ſpent the night there, being apprehenſive of other ſhocks. The account then newly brought us of a dreadful earthquake at *Lima*, being freſh in every body's mind, contributed to increaſe the ſurprize.

A worthy clergyman, who lives 5 miles from *Taunton*, informed me, that the china and glaſſes upon the cupboards in his houſe rattled and ſhook as if they would fall down, and the bells in his houſe rang. A perſon who was at that time coming on foot to *Taunton* likewiſe told me, that the noiſe ſeemed to him like the diſcharge of cannon at a diſtance, and came rumbling onwards, till the earth moved under him in ſuch a manner that he could hardly keep upon his legs: ſeveral others alſo that were abroad aſſured me they had much ado to ſave themſelves from falling.

The extent of this earthquake, as far as I can learn, was from ſea to ſea; that is, from the *S. Chanel* to the *Severn*. It moved from S. E. to N. W. and was felt in every pariſh through this whole courſe, which is in length about 40 miles: nor was its breadth much leſs; for it was felt at the ſame time both at *Exeter* and *Crookhorn*, which lie from one another about the ſame diſtance of 40 miles, in a line directly acroſs its before-mentioned courſe.

P. S. I have heard it reported that there were flaſhes of lightning at the time of the earthquake; but I neither ſaw any myſelf, nor have met with any body that could affirm he did.

XXVIII. 1. As I was walking along *Chancery-lane* to-day, towards *Holbourn*, about 40′ paſt 12, people came out of ſeveral houſes to their doors in great ſurprize, complaining of the ſhaking of their houſes, and imputing it to the fall of ſome great timber, or other heavy body, which they ſuppoſed at ſome little diſtance from them, and which they came out to inquire after.

—*at* London, Feb. 8. 1749-50. *by Mr* Henry Baker, *F.R.S.* N°. 497. p. 601. *Read* Feb. 8. 1749-50.

When I was got into *Holbourn*, I found the people under the ſame conſternation, and expreſſing themſelves nearly in the ſame manner.

Going to *Gray's-Inn*, many people were got together in the great ſquare, talking about the ſhock they had felt; and in particular a lamp-lighter was giving an account, that, being on his ladder, pouring oil into a lamp, he was in danger of falling off, by ſomebody's ſhaking the ladder, as he at firſt imagined.

I then went to a gentleman's chambers under *Gray's-Inn* library, where I was told the ſhaking had been ſo much, that they thought a clock would have been thrown down; and imagined at the time, ſome large box or heap of books had been tumbled down over head.

The people in all the ſtreets, as I returned home, were talking of this matter; and ſome of the women complaining that the motion had made them ſick.

My own family, in *Catherine-ſtreet* in the *Strand*, had been no leſs ſurprized, and had ſent to the neighbours to inquire if ſome heavy thing had fallen down, to occaſion the ſhaking of the houſe, which Mrs *Baker* deſcribed as very great: ſhe ſat in the dining-room, which is to the ſtreet, and her belief at the inſtant was, that the ſervant was fallen all along in the backward room of the next ſtory higher, thereby ſhaking the houſe, and making a conſiderable noiſe.

My ſon felt the ſame ſhock at the *Tower*; where alſo a gentleman, who was ſitting at a table to write, in his houſe in the Mint, was thrown out of his chair with a conſiderable force towards the table; and where every body was much ſhocked with the apprehenſion of ſome exploſion of gun-powder.

What therefore ſo many people, in different ſtreets, at great diſtances from each other, have been ſurprized at, cannot be only fancy, but muſt be owing to ſome real cauſe; and if no powder-mill, or magazine of powder, has beeen blown up, it muſt have been an earthquake, or ſome tremor of the earth itſelf.

I was pretty curious to inquire of people in different places, to judge the better from their ſeveral reports; and I found them agree, almoſt in general, in the firſt ſuppoſition of ſome weighty body falling; moſt ſaid with a noiſe, but ſome ſeemed uncertain as to that. I endeavoured likewiſe to learn its courſe, by comparing the accounts of people in different ſituations: it ſeems to have lain E. and W. and to have paſſed from the W. eaſtward. I felt nothing of it myſelf as I walked the ſtreet, nor do I find that many who were walking did.

—*by* Gowin Knight, *M.B. & F.R.S.* Ibid. p 603. *Read* Feb. 8. 1749-50.

2. To-day, betwixt 12 and 1, the houſe in which I live in *Lincoln's-Inn-Fields* was ſhaken violently for a moment. The room where I was ſhook very much, but nothing was thrown down. In another room the grate was ſeen to move, and the fire-ſhovel was thrown down. A maid-ſervant that was above-ſtairs was much frightened with the ſhock and noiſe: ſhe ſaid, ſhe heard a ſound like thunder, which ſeemed to come from below: that the window-curtains and bed ſhook very ſenſibly, and the

the latter was moved from it's place nearer to the wall. Three other persons in the house both heard the noise and felt the shock; but I did not take notice of the noise myself, being intent upon something else at that time.

Soon after this happened, a servant came from his Grace the Duke of *Newcastle*, to inquire if we had perceived what had happened; and said, that his Grace's house had shaken all over.

I sent to two or three houses in the neighbourhood, and was informed, that they were equally sensible both of the shaking and the noise; and at one of the houses it had thrown down a firkin of butter from a shelf. I was further informed, that several of the neighbouring inhabitants ran out of their houses; and some gentlemen that were playing at tennis ran out of the tennis-court.

A woman reports, that she felt a like shock the night before last about 7 o'clock, which made the candle jump off the table at which she then sat.

Whilst I was writing, a man came in from *Greenwich*, who said, he was at *Dockhead* when the shock was felt there, and the noise seemed to him like that of a cannon at a distance: that all the way he came, as far as *London-Bridge*, the people were alarmed at it.

3. As I passed thro' the *Mews* to-day, a little before 1, I felt a shock like an earthquake, and I thought I heard a hollow deep noise. Several women thereabouts came running out of their houses much terrified.

At *Leicester-House* they were apprehensive the foundation was giving way, and were going to send immediately for the surveyor.

All the way in my return home, I saw many groups of people together, and discoursing upon this shock that had just happened; some imagining it was occasioned by some houses being blown up in *Gold-Lane*, where there was a great fire, and others from some powder-mills blowing up; the same thing having been observed about 9 years ago, from the like accident at the mills at *Hounslow*. If neither of these causes appear, it can be no other than an earthquake.

——*by* Jo. Freeman, *Esq*; *F. R. S.* Ibid. p. 605. *Read* Feb. 8. 1749-50.

4. On *Thursday Feb.* 8. 1749-50. at about half an hour after 12, as I was sitting reading with one elbow on the table, on the ground-floor, in my house at *Eltham* in *Kent*, I felt two shocks from E. to W. which I immediately thought was an earthquake, as I had felt something like it once at *Naples*; and was confirmed in my opinion, by my wife's running down-stairs frighted, and declaring it was an earthquake, she having felt one in the *West Indies*. She was in the room over me, in which room there was china standing on a cabinet, which, she says, shook in such a manner that she expected it to fall. My children, who were in the room over her, seem to have felt it stronger; as they say, they apprehended a chest of drawers in their room was falling. The servants that were in the kitchen, which has no room under it, seem to have felt but little of it. One that was writing says he felt the dresser move, and the wall, but thought it was only the shutting of a door. Other

——*by* Will. Fauquier, *Esq*; *F. R. S.* Ibid. *Read* Feb. 15. 1749-50.

ſervants in the ſame room felt nothing at all of it. My gardener, who was at work in the garden, felt nothing of it.

The wind was at S. W. and had been high in the night and morning, but was very much abated; and after this, for ſome time, it was quite calm; which I believe it is generally obſerved to be, in thoſe countries where earthquakes are more frequent. A flight of pigeons I have, ſeemed to be much frightened.

Eltham is about 8 miles S. S. E, from *London-Bridge*, and ſtands on a hill.

This account was written before I had heard any thing from *London*.

——*by the Rev.* Henry Miles, *D. D. F. R. S.* Ibid. p. 607. *Read* Feb. 15. 1749 50.

5. I find, upon inquiry, that the earthquake on *Thurſday* laſt was felt in a gentleman's houſe in this place, * pretty ſenſibly, in two chambers, and in another over one of them, by a tremor of the wainſcot and utenſils, and a ſmall ſhock ſucceeding; but was not perceived at the other end of the houſe, in a room on the ſame ſtory with the chambers. I ſhould have thought, when it was felt ſo near us, as about 150 yards, our houſe, which I look upon as very ſuſceptible of impreſſions, ſo as to be ſhaken by the winds, would have been affected: but if it had, 'tis hardly poſſible but ſome of the family muſt have perceived it, conſidering the parts they were in, and their being chiefly in a ſitting poſture. I conclude therefore it could not be very conſiderable here. I have endeavoured to learn whether it extended any further to the S. of us, but cannot yet find it did.

The wind has been chiefly S and S. W. for ſome months paſt, much longer than is uſual at any time of the year; and yet we have had but a ſmall quantity of rain hereabout for the ſeaſon.—I give you, on the other ſide, the ſtate of the Barometer and Thermometer on the 8th, and two days before and after that, as obſerved at *London* by Mr *Canton*, and by myſelf here, at 2^h *p. m.* and at 8^h *p. m.* each day. I ſhall only add, that yeſterday (the 13th) my Thermometer abroad at 1^h *p. m.* was at 59 = to 27° above the freezing point: a degree of warmth exceeding what we had ſeveral days in laſt *June*, at 2^h *p. m.*

At *London*.

Day.	At 2 *p. m.* Barom.	Thermom.	At 8 *p. m.* Barom.	Thermom.
6	29,14	48	29,27	43
7	29,90	48½	29,99	43
8	29,83	54	29,95	52
9	29,97	55½	29,96	52½
10	29,90	54	30,03	45½

* Tooting *in* Surry.

At

At *Tooting*.

Day.	At 2 *p. m.* Barom.	Thermom.	At 8 *p. m.* Barom.	Thermom.
6	29,18	49	30,32	42
7	29,94	49	30,03	38½
8	29,88	55	29,98	52
9	30,—	56	29,99	52½
10	29,88	55½	30,04	45

6. It was felt here * at 40′ after noon. All the houſes were violently ſhaken, eſpecially thoſe which are neareſt to the river. I was ſitting in my ſtudy, which fronts the S. W. up one pair of ſtairs. I imagined that ſomething heavy had fallen down in the room below me. The ſervants, who happened to be diſperſed in ſeveral rooms, each of them thought one of the others had thrown down ſome heavy cheſt or cabinet. A maid-ſervant, who happened to be paſſing from one of the under offices to another, felt the ground ſhake under her. As the place on which her feet were, was full 6 feet below the ſurface, I immediately concluded, that ſuch a motion muſt be occaſioned only by an earthquake.

——*by* J. Martyn, *F. R. S.* Prof. Bot Cantab. Ibid. p. 609. *dated* Chelſey Feb. 14. 1749-50 *Read* Feb. 15. 1749-50

However, as moſt of the neighbours were confident that the ſhock was occaſioned by the blowing up of the powder-mills at *Hounſlow*, I diſpatched a meſſenger thither on horſeback, who brought me word, that the ſhock was not felt either there or at *Brentford*; and that he could not learn that it had been felt farther weſtward than *Kenſington*-turnpike.

I have alſo ſince been inform'd, that it was felt at *Fulham*; but a friend, who lives a little beyond *Richmond*, has ſent me word, that they did not feel it there.

Of thoſe who were in the ſtreet, or upon the river, ſome felt it, and others not.

I am credibly informed that letters from *Calais* and *Boulogne* mention it's having been felt on that coaſt.

It ſeems therefore to have extended itſelf far to the E.; but to have terminated in the W. about two miles beyond this place.

7. The earthquake was not felt at *Ingatſtone*, nor at *Colcheſter*, nor at this town†.

——*by Mr* Trembley, *F. R. S.* Ibid. p. 610. *Read* Feb. 15. 1749-

On *Thurſday* laſt the lightning fell on 2 ſteeples not far from *Colcheſter*. ‖

8. With 50.

* Chelſey.

† Harwich.

‖ Mr *Trembley*, who was going the next day for *Holland*, was at the time of the earthquake with Mr *Folkes* in his ſtudy in *Queen's-Square*; where were alſo the Earl of *Macclesfield* and the Hon. *Charles Bentinck*; they all felt themſelves at the ſame inſtant ſtrongly lifted up, and preſently ſet down again: they alſo heard a noiſe over their heads as of ſome heavy piece of furniture being thrown down, whilſt thoſe who were in the room over them were frighted, and apprehended the like accident had happened below-ſtairs. The coachmen on the boxes of 2 coaches then ſtanding at the door, were extremely ſenſible of the ſhock, and apprehended the houſe was going to fall upon them.

——*by* Smart Lethieullier, Eſq; *F. R. S.* Ibid. p. 613. *Read* Mar. 1. 1749-50.

8. With regard to the extent of the late earthquake this way (I mean due E. from *London*) I cannot learn that it paſſed the little river *Rodden* that runs by my gardens, and croſſes the *Harwich* road at *Great Ilford*. My own houſe ſtands about ½ a mile N. from the 6 mile-ſtone. I was in my gardens with ſeveral workmen, and none of us were ſenſible of any thing; but my wife, who was in her dreſſing-room, felt the houſe tremble ſo much, that, upon my coming in, ſhe told me ſhe verily believed there had been an earthquake; the motion of the houſe under her being exactly like what ſhe had often heard deſcribed when ſhe was in *Italy*.

——*at* Plymouth, *about one in the morning, between* Feb. 8. *and* 9. 1749-50. *by*

XXIX. It is proper to obſerve, that the following relations are not made by mean, ignorant, or fanciful people, but by perſons of good ſenſe, whoſe veracity is unqueſtionable, and whoſe judgment in this caſe is, I think, rational and juſt.

the Rev. Mr W. Barlow. Ibid. p. 692. *Read* May 24. 1750.

The Earthquake at *London* was on *Thurſday*, *February* 8. about noon.

The declaration of the Hon. Philip Vanbrugh, *Eſq; Commiſſioner of his Majeſty's dock-yard near* Plymouth, Devon.

That, in the night, betwixt *Thurſday* the 8th of *Feb.* 1749. and the day following, towards one as he was reading in bed, he was ſenſibly affected by a ſudden ſhake: that, looking about, he obſerved betwixt the foot-curtains, which were partly open, ſome drawings which hung on the wall, alſo the ſide-curtains, in a trembling motion: that it immediately brought into his mind the earthquake in *Jamaica* in 1692. which ſeveral years ago had been, at *Jamaica*, particularly deſcribed to him, by a perſon who was in the iſland at the time of that earthquake.

The declaration of Mrs Vanbrugh, *Siſter of the Commiſſioner, living in the ſame houſe.*

That, the night above-mentioned, at what hour ſhe cannot ſay, ſhe was greatly ſurprized by an unuſual motion of her bed; which immediately brought to her thoughts the ſhock of an earthquake ſhe felt ſeveral years before, to which in her mind ſhe reſembled the preſent ſhake.

The declaration of Mrs Slade, *Daughter of Mr* Slade, *Maſter-Shipwright of his Majeſty's dock-yard,* Devon.

That, in the night above-mentioned, towards one ſhe perceived a ſenſible motion of her bed, and (there being a light in the room) ſaw the curtains ſhake. This ſhe thought to be occaſioned by the wind (which ſhe then heard blow ſtrong), ſuppoſing the ſaſh not to be cloſe down: accordingly ſhe roſe, and went to the windows, but found the ſaſhes intirely cloſe.

N. B. Both the Commiſſioner and the Ladies believe there was only a ſingle ſhake. Miſs *Slade*'s houſe joins the Commiſſioner's.

On account of the dead time of the night in which it happened, not many people would be ſenſible of the motion: of thoſe who were, moſt would naturally attribute it to the high wind, which then blew; and a month being paſſed before any inquiry has been made, it is not to be wondered at that, but few recollect any thing concerning it.

The Commiſſioner, by reaſon of frequent returns of the gout, does not ſleep up-ſtairs, but in a very ſmall room behind the houſe, joining to the

the houſe, and communicating by a door with one of the back parlours. This building is ſo low (being only a ground-floor, without any room over it), and is ſo ſkreened by higher buildings, that hardly any wind can affect it; eſpecially the wind which blew that night; as the body of the Commiſſioner's houſe was betwixt that and the ſtroke of the wind.

The dock is about 2 miles W. from *Plymouth*, and about 220 almoſt weſt from *London*.

XXX. 1. At very nearly half an hour after 5 this morning, being then in bed, but perfectly awake, I felt a very ſtrong ſhake, or rather 3 or 4 ſucceſſive ſhakes of an earthquake, as I immediately took them to be. I judge the whole *phænomenon* to have laſted about 3 or at moſt 4″; and the ſhocks appeared to differ from what I had felt on the 8th of laſt month, in this particular, that I did not now perceive that lifting upwards, and ſudden ſettling again, which I was then ſenſible of; nor did I hear that diſtinct noiſe, as of a great weight falling, which moſt people were then ſurprized with: but what now moſt affected me was the ſenſation of very quick ſhakes, or of a *Tremor*, as it appeared in a horizontal direction; and I heard, during all the time, a ſort of crackling of the wainſcot, window-frames, and floor, with ſuch a rattling in the chimney, as I have ſometimes heard upon a ſudden and ſtrong guſt of wind.

—March 8. 1749-50. *at* London *by the* Preſident. Ibid. p. 613. *Read* March 8. 1749-50.

I inſtantly jumped out of bed, to ſee if there was any damage done; and going to my chamber-door, I met my daughter running in a fright from her room, who ſaid ſhe was waked with ſuch a ſhock, that ſhe thought her room had been falling; two men-ſervants alſo, who lay in the garret, and whom I had called to, anſwered me whilſt I was talking to my daughter, that they were both awakened by the ſhock, and that they felt, as they both expreſſed it, ſuch a motion, as they had ſometimes known given to a child in a cradle.

Preſently after I had got on my clothes, I ſpoke to ſome of my neighbours, who all gave me very nearly the ſame account as this I have been juſt giving of what I had obſerved myſelf; only ſome added, particularly a gentleman who lives in an older houſe than mine, that he apprehended, from a crackling noiſe over his head, that a chimney had been thrown down, and was then breaking thro' the tiles and lathing of his houſe.

I ſent a ſervant about 7, and he met a countryman, who was bringing a load of hay from beyond *Highgate*, and who was on the other ſide of the town when the ſhock happened; he did not, he ſaid, feel it, as he was driving his waggon; but that the people he ſaw in the town of *Highgate* were all greatly ſurprized, ſaying they had their houſes very much ſhocked, and that the chairs in ſome were thrown about in their rooms.

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The chamber I lie in is up two pair of ſtairs forwards, and my bed ſtands N. W. and S. E. I took particular notice, that there was neither cart nor coach going by, but that every thing was entirely quiet at the time.

——*by the Rev. Mr Tho. Birch. F. R. S.* Ibid. p. 615. *Read* March 8. 1749-50.

2. This morning, *Thurſday March* 8. 1749-50, about 18′ before 6, according to equal time, or half an hour after 5 by the ſun, I felt, in my bed-chamber, on the ſecond ſtory of my houſe in *Norfolk-ſtreet*, adjoining to the river, a ſhock of an earthquake, much ſtronger, and of longer duration, than that which I had felt on *Thurſday Feb.* 8. I was full awake, and had juſt begun to dreſs myſelf, when I was alarmed with the trembling of the room, attended with a noiſe ſomewhat different from that of the former ſhock this day month, which ſeemed to be occaſioned by ſome great weight falling upon the floor above me; whereas the noiſe of this latter appeared to me cauſed only by the tremulous motion of the whole room and the adjoining ones, the walls, wainſcot, furniture, *&c.*

When I came down to my ſtudy on the firſt floor, I found a book thrown down from an upper ſhelf.

My family were awakened by the ſhock. The air was at that time, and for ſome hours after, extremely calm, and the wind weſterly.

This account was drawn up immediately before I had ſeen any other perſon, except my own family.

I have been ſince told by a gentleman, who reſided many years in the *Weſt Indies*, that this laſt ſhock was more violent than any he had felt there, except one at *Carthagena*, in which a city, about 200 leagues diſtant from thence, was ſwallowed up at that inſtant.

Another gentleman deſcribed to me the ſenſation, upon being awakened by the motion, to be like that of falling into a fit.

——*by Mr Henry Baker, F. R. S.* Ibid. p. 617. *Read* March 8. 1749-50.

3. About 40′ after 5 this morning I was waked out of a ſound ſleep by a great noiſe, hardly to be deſcribed, but what ſeemed then to me as if the roof of the houſe was tumbling in, or like the rumbling I have ſometimes heard thunder make before a very loud clap. It continued 2 or 3″ as nearly as I could gueſs, under the ſurprize of being awaked ſo ſuddenly; during which time the windows rattled, the tables, chairs, and other furniture in the room, ſhook greatly; and a braſs warming-pan, in particular, that ſtood upon a marble ſlab before the chimney, made a very remarkable ringing and jarring noiſe.

Two maids, in a room over mine, were alſo waked by it, and got up immediately, being frighted by the ſhaking of the bed on which they lay; but for my own part I felt little or no motion of my own bed. However, as moſt people who were in bed agree in deſcribing ſuch a motion, it was probably that which waked me, tho' the violence of it might be over, before I was ſufficiently awake to take due notice of it.

My ſon was likewiſe waked by the great rattling of the windows in his chamber, which he at firſt imagined to be ſhaken by a high wind,

they

they being apt to make ſome noiſe in windy weather, though never like what he then heard, which ſeemed like opening a caſement-window and ſhutting it again very ſuddenly and violently. A cat that lay upon his bed ſtarted up in much ſurprize.

Several people in the neighbourhood went out into the ſtreets, and others got upon the leads of their houſes, to ſee if any damage was done. Some milk-women felt it very ſtrongly as they were milking their cows near *Maribone*. It was violent in *Bond-ſtreet*, and all that quarter of the town; and I find, by people living in very different and diſtant parts of *London* and *Weſtminſter*, that it was felt univerſally through the whole, and was not longer in continuance, but that it's motion was more violent and ſhocking than the earthquake, which happened on *Feb.* 8.

A waterman that was lying in his boat, then at anchor near *Kingſton*, told a friend of mine, that he was frighted with ſomething like a violent blow ſtriking againſt the bottom of the boat; that the water was much agitated, and that the barges and other veſſels upon the river were toſſed about as in a tempeſt.

4. As I was walking this morning in my garden at *Kenſington Gravel-Pits*, about 40′ after 5 by my watch, I felt an exceeding great ſhock, a ſuccuſſion of the earth, attended with an exploſion that, I fancy, reſembled the blowing up of a mine, and with the ſame kind of noiſe. It was followed by a trembling, very briſk at firſt, which gradually abated; and in about 3′′, as near as I can judge, totally ceaſed. The tremor was attended with the noiſe of a diſtant thunder, which, with the motion, gradually died away.

——*by* Martin Clare, *Eſq*; *F. R. S.* Ibid. p. 620. *Read* March 8. 1749-50.

I am not certain the building near me moved; but I fanſied it did. My feet I am ſure felt great emotion; and a large watering-pot, of 9 inches baſe, that ſtood near me, was thrown all along, the moment the trembling ceaſed.

P. S. I have ſince diſcovered that my watering-pot was overſet by a brick that was thrown off the houſe by the ſhock.

5. At 40′ paſt 5 this morning, as I lay awake in a two pair of ſtairs room, I heard a noiſe, firſt as if the top of the houſe was cracking, and then as if it fell in. Inſtantaneouſly the houſe ſhook with great violence, rocking to and fro from N. E. to S. W. ſo as to make all the furniture rattle, the windows and bells ring. It alſo waked all the family, but the ſervants in the garrets felt the greater ſhock.

——*by* Daniel Peter Layard, *M. D.* & *F. R. S.* Ibid. p. 621. *Read* March 8. 1749-50.

It is reported, that two ſmall ſhocks preceded this, one at 12 laſt night, the other at 2 this morning; but I cannot ſay I felt either.

The milk-people in the fields were very ſenſible of this earthquake, and ſay, that it began by a report like that of a cannon fired near at hand. Thoſe who were milking in a barn thought it was coming down; but were not ſo ſenſible of the earth's motion as thoſe milking in the fields, who could ſcarcely ſtand. The cows were alſo frightened, and ran away from the people.

This earthquake was univerſally felt at *Hampſtead*, and much ſtronger than that on *Feb.* 8.

—*by the Rev Mr* Roger Pickering, *F. R. S.* Ibid p. 622. *Read* March 8. 1749-50.

6. The earthquake this morning, which happened at ſo early an hour as to ſurprize moſt people in their ſleep, I had all the opportunities imaginable of obſerving to as much exactneſs as *phænomena* of this nature can be; and therefore preſent you with this early intelligence.

About $\frac{1}{4}$ after 5 I was lying in bed awake, and under the compoſure which one generally feels upon recovering from a regular and refreſhing ſleep. To this the ſerenity of the morning greatly contributed, as well as the gradual increaſe of light, which, the ſhutters being open, could eaſily be perceived through the linnen furniture of the bed and window-curtains. I mention this particular, becauſe, every object in the chamber being thus rendered ſufficiently diſtinct, I had the opportunity of examining the event by the evidence of my eyes, as well as that of my ears. On a ſudden I heard a ſound like that of a blaſt of wind; ſuch a blaſt I mean, as thoſe we perceive in the dry windy days, ſo frequent with us about the equinoxes; one that, at different intervals, riſes gradually to its full ſtrength, and gradually dies off. The attention this occaſioned led me very ſenſibly to perceive myſelf raiſed in my bed (which ſtood N. and S.) and to obſerve, that the motion, as I lay upon my back, began on my right ſide, and from head to foot inclined me towards the left.

This was nearly inſtantaneous with the commencement of the blaſt, and I immediately concluded it to be an attack of the ſame kind with that on this day was a month. The ſenſation I felt at this time was rather ſolemn than terrifying; ſo that I patiently lay to obſerve the following circumſtances.

The firſt ſhock being given, the motion that followed was that of a very quick vibration; and looking at the curtains of the bed, I perceived their motion was ſimilar. The hinges of the drawers of a dreſſing-table on my right hand clapped, not only diſtinctly, but conſiderably loud; and a ſound, ſuitable to its materials, came from every moveable body in the room. The whole ſhock, to my judgment, laſted about 10 or 12″; when, the ruſhing noiſe above-mentioned having gradually died away, every thing was reſtored to that calmneſs which had preceded the event.

I then roſe, and found by my watch that it was half an hour paſt 5.

I am not able to judge of the comparative force of this ſhock with that on this day month; for I was then at my houſe on *Enfield-chace*, where it was not in the leaſt perceived, nor nearer to there (as I can learn) than *Edmonton*, which lies, in a S. E. direction, about 3½ miles nearer to town. But one of my chimnies in town has ſuffered by this ſhock; and I am informed that two others have been thrown down in *Monkwell-ſtreet* and *Jewin-ſtreet*, which lie in a N. W. direction, from *Aldermanbury*, at about $\frac{1}{8}$ a mile diſtance.

I think

I think I have ſomewhere found it obſerved by the ancient Naturaliſts, that earthquakes uſually, tho' not conſtantly, happen in the ſpring and autumn; and the principle they go upon gives ſome credit to their obſervation. However, this induced me to look over my minutes of ſuch earthquakes with us as have come to my knowledge ſince the middle of the laſt century. Theſe I found as follow; *viz.*

19 *Jan.* 1665-6.	at *Oxford*	about 6 even.
17 *Sept.* 1683.	at *ditto*	about 7 morn.
9 *Oct. ditto*	thro' the midland counties	about 11 night.
8 *Sept.* 1692.	In *London*, and parts adjacent, and in the *Engliſh* camp in *Flanders*	about 2 *p. m.*
28 *Dec.* 1703.	In the N. of *England*	about 5 even.
8 *February*, 1749-50.	In *London*, and about 7 miles round, and from private hands at *Calais*	about 12½ *p. m.*
8 *March*, *ditto*	In *London*, where elſe, at preſent unknown,	about 5½ morn.

Out of theſe 7 inſtances, you will perceive, that 3 have happened in the autumn, 2 in ſpring, and 2 in *Dec.* and *Jan.* ſo that the balance in favour of the ancient obſervations is as 5 to 2. I beg leave alſo to obſerve, that of theſe, the 2 in the ſpring are thoſe we have ſo lately felt, and thoſe, after the openeſt winter, and in the warmeſt ſpring, that our country, poſſibly, ever knew. How far this obſervation may be applicable to adjuſt the cauſe of the two late earthquakes, it would be impertinent in me to offer.

7. He was juſt come into the garden (at ½ an hour after 5), and was ſcarce arrived at the great tree, near the little door under *Harcourt's* buildings (which is about 10 or 12 yards perhaps diſtant from *Harcourt's* buildings), when he heard a GREAT NOISE, LOUDER BY MUCH than the noiſe of the exploſions upon the proof of the *great cannon* at *Woolwich*, when full-charged (at which proofs he has been often preſent); nay *louder*, *he thought*, *than* ANY *noiſe he ever heard*. This noiſe ſeemed to come from behind the buildings; and his face was then oppoſite to them.

At the ſame inſtant he ſaw the whole building *move upwards*, then *incline forwards* towards him (ſo that he thought it would fall upon him); then *recline backwards*, and then *ſettle*; at which time all the *windows rattled and clattered*, as upon an exploſion of a cannon; the ſound in the interim *rolling away* (ſeemingly from the water-ſide up towards *Temple-Bar*) juſt like the rolling ſound or echo which accompanies or ſucceeds the exploſion of thunder, or of ſhip-guns.

As to the continuance, he often expreſſed himſelf that it was *as a thought*; and, in point of time, he ſuppoſed it could not be 3″.

The ſound and the motion began both together; but the ſound was quite rolled off, rather *ſooner* than the buildings were ſettled.

— *as ſeen in the* Inner-Temple *garden, by* Robert Shaw (*a very ſenſible* Scotchman) *then at work there; communicated by* James Burrow, *Eſq; F. R. S.* Ibid. p. 626. *Read* March 15. 1749-50.

I aſked him how much he judged the declination of the building to have been *from the perpendicular?* he anſwered, "that it was impoſſi-"ble to be exact in relating that particular, both by reaſon of the ſur-"prize he was in, and alſo from the ſuddenneſs of the whole compaſs "of the appearance, which was but as a thought." However, in order to form ſome notion of it, I made a comparative inclination of the garden key, which I held perpendicular to my hand, and moved backwards and forwards, till he ſhould judge it to approach neareſt to what he could recollect of the heeling of the buildings (which indeed was but an inaccurate method of coming at the truth of a thing profeſſedly uncertain even to the relator): It ſeemed to me, by his deſcription in this rough compariſon (in which I might eaſily miſtake him) to be a declination of ſeveral degrees from the perpendicular; which is alſo agreeable to his apprehenſion, that the building would fall upon him. Poſſibly, his ſurprize might magnify the appearance far beyond the reality.

I then inquired what perception he had of his own *perſonal motion*, and that of the particular ſpot of *ground whereon he then ſtood?* he ſaid, his ſurprize was ſo great, on ſeeing ſo new and uncommon an appearance (for he was not in or near *London* when the laſt earthquake happened), that he either had *no perception* at all concerning himſelf and the ground under him; or at leaſt was ſo intirely engaged in obſerving what he ſaw and heard, that he had no attention to what he felt.

N. B. I made this memorandum immediately after converſing with this *Robert Shaw*; and therefore I ſuppoſe it contains a tolerably exact account of what he related to me.

——*by the Rev.* H Miles, *D. D. F R. S.* Ibid. p. 628. *Read* March 15. 1749.

8. At 5^h $40'$ nearly, equal time, I was ſtanding in my ſtudy, when I heard a noiſe, at the firſt moment, very like the exploſion I heard ſeveral years ago, when ſome drying and ſtorehouſes of gunpowder were blown up about 6 miles from us: this noiſe iſſued in a murmur in the air, like diſtant thunder, and ended like a ruſhing wind: there was no perceivable intermiſſion in the noiſe from firſt to laſt; it ſeemed to come from the W. and continued about $4''$ excluſive of the tremor, which laſted ſome time after the noiſe ſeemed to have ceaſed. I found no motion of the floor, which is over a cellar, upon a level with other adjoining ground-floors; but the doors of my book-preſſes, at the weſt end of the room, ſhook to and again, and the rings on the doors rattled exceedingly; but I was not ſenſible of any motion of the other doors on the ſides of the room, near to ſome of which I then ſtood. Such of the family as were in bed felt their beds to be raiſed up, and then ſhaken from ſide to ſide.

A ſenſible young gentleman, who called upon me that morning as he came from *London*, told me, that, conſidering the ſituation his bed was placed in, and the motion he felt from one ſide to another, he concluded the ſhock proceeded from the W. The ſame obſervation, he ſaid, others had made, with whom he had talked.

It

It was univerſally felt here *, and very much at the following places around us: at *Merton*, about a mile S. W. from us, and at *Mitcham*, about 2 miles S. at *Croydon*, 4 miles S ſome tiles fell from houſes; it greatly alarmed the inhabitants of *Streatham*, about a mile and $\frac{1}{2}$ E. of us; at *Clapham*, two miles and a half N. a chimney, and other parts of a building fell down; at *Wandſworth*, two miles N. W. the ſhock was very great; eſpecially near the ſide of the *Thames*. It was felt likewiſe at *Epſom*, about nine miles S. W. from us; but how much farther, my information does not ſay.

Upon inquiry made of ſeveral perſons, who were abroad at the time, going to their work, I cannot hear of any one, who ſaw any lightning, perceived any noiſe, or felt any thing of an earthquake. †

The ſtates of the Barometer and Thermometer were not remarkably different from what they had been for ſeveral preceding days; however I have added them underneath.

7 day at 4^h *a. m.* Bar. 30,07. Therm. 32 almoſt, Wind W. clear, and white froſt.

D° at 9^h *p. m.* Bar. 29,99. Therm. 40, clear evening.

8 day at 4^h *a. m.* Bar. 30,07. Therm. 40, cloudy morning. Wind W.

D° at 2^h *p. m.* Bar. 30,14. Therm. 58, cloudy evening. Wind W.

9. It was much ſtronger than that which happened in *Feb.* two great ſhocks being felt preſently after each other. Thoſe who were out of doors felt the ground ſhake under them very ſenſibly; whereas, in the former, few were ſenſible of the ſhock, except thoſe who were in houſes. I do not hear of any miſchief done by it in this neighbourhood; ‖ neither can I learn that it extended farther weſtward than the former. I am very well aſſured, that it was ſtrongly felt at *Fulham*, and at *Turnham-Green*. ——*by* John Martyn, *F. R. S.* Prof. Bot. Cantab. Ibid. p. 630. *Read* March 15. 1749.

Several were ſenſible of a ſmaller ſhock about 3 the ſame morning; and ſome talk of unuſual appearances in the air. But this laſt circumſtance ſeems not very well atteſted. For my own part, I looked out of my chamber-window to the S. W. at $\frac{1}{4}$ after 5, and only perceived it to be a grey morning, ſuch as uſually predicts a fine day.

10. As there happened ſome particular circumſtances in the laſt violent ſhock of an earthquake, that were not taken notice of at our laſt meeting, I preſume therefore to ſend you the obſervations I made thereon, as they appeared to me at my houſe in *Bloomſbury*. ——*by* Michael Ruſſel, *Eſq*; *F. R. S.* Ibid. p. 631. *Read* March 15. 1749.

On *Thurſday* morning laſt, about half an hour after 5 o'clock in the morning, I was awakened by a violent percuſſion of my bed; the ſhocks, I apprehend, laſted about 10 or 12″, becauſe my bed was rocked from one ſide to the other ſeveral times. The motion appeared to me to be in a horizontal direction. I heard no noiſe before the ſhake; but if any, it might perhaps occaſion my being ſo thoroughly awakened. My ſervants told me they heard a report like a great blaſt of wind with a

* Tooting *in* Surry. † See Art. 14. ‖ Chelſey.

clat-

clattering and ſnapping of the windows and doors of the houſe, as they expreſſed it.

In my dining-room there was an *India* cabinet, on which was placed ſome ornamental *China*, part of which was thrown down on the floor, and ſome broke.

But what was moſt remarkable, I had two *China* figures placed on the cabinet, with their faces fronting the W. which were, by the ſeveral ſhocks, turned about facing the N. E. which I took to be nearly $\frac{1}{3}$ of the circumference of a circle. In this ſituation I found them as ſoon as I aroſe in the morning; and I am aſſured nobody had been in the room before to diſplace them.

Since, I was told by ſome company, that were at my houſe, that a porter was going down *Chancery-Lane* to call a gentleman to go to the *Brentford* election, and, in his way, as he called it, was ſtruck with a blaſt, turned round on his heel, and fell down, and has not been well ſince. Alſo another perſon, that was ſet out on ſome buſineſs, was nearly turned round by the ſhock; which ſeems a little to confirm the moving the *China* figures in the direction before mentioned.

——*by* James Parſons, *M.D F. R. S.* Ibid. p. 633. *Read* March 15. 1749.

11. Between 5 and 6 in the morning I was awaked by a violent motion of the houſe, with a great noiſe reſembling the fall of ſome heavy body over-head: at waking I found my bed move very much; but in my ſurprize could not diſtinguiſh what kind of motion it might be, as to its direction; this being momentary, as well as my ſurprize, I ſaw and heard the *China* rattle upon a cabinet in my chamber, and perceived a trembling motion in the room for ſeveral ſeconds. My wife was alſo diſturbed; and aſking what was the matter, I ſaid I felt a ſhock, which I believed to be like what had happened that day month: ſhe anſwered, if this is one, I felt another about 2 this morning, while you ſlept. I immediately looked at my watch, and ſaw it was 36′ or 37′ after 5. I obſerved a light ſhine in at my windows more than was natural; and from the redneſs of the ſky, and clearneſs of the morning, concluded the ſun was riſing; but imagined it ſomething ſtrange it ſhould riſe ſo many minutes ſooner than was uſual upon the 8th of *March*: upon which I roſe, and went to my window, which looked eaſtward, and then ſaw it was a red light only, extended from about N. N. E. to S. E. which I take to be that mentioned by the Rev. Dr *Miles* laſt *Thurſday*.

Then I went into the ſquare, where ſeveral ſervants came out of their houſes much frighted. They all felt it the ſame way; and ſeveral labourers and market-people told me they were ſhaken very much as they walked, and that all *Newgate-Market* was in the greateſt confuſion imaginable, no one thinking himſelf ſafe. The greateſt part of thoſe I ſpoke to about it heard the ſame ſort of noiſe that occurred to me at the time.

Many complained all day of great ſickneſs at the ſtomach, and *head-aches*; I had the latter 3 or 4^h after it; and others of my acquaintance felt pains in the back the greateſt part of that day.

At

At *Mary'-bon* it was univerſally felt; and with the greateſt violence imaginable. At one houſe where I was at 12 that day, a maid-ſervant ſaid, ſhe was thrown firſt to one ſide, and then back to the other; and many compared it to the rocking of a cradle. I inquired particularly of ſuch as were thus rocked, about the ſituation of their beds, and obſerved, that, though all deſcribed the motion to be from ſide to ſide, their beds were in all directions.

As to the ſhock which was ſaid to be felt at two the ſame morning, there can be no doubt of it; for I went to ſeveral families in my neighbourhood, who poſitively aſſerted it to be true; but all ſaid it was not ſo ſtrong as that which happened afterwards; nor could any one ſay he was awaked by it: on the contrary, all thoſe, who felt it, ſaid they were awake before it happened. As for myſelf, I did not perceive it, being aſleep; and therefore I ſhould be inclined to think the motion to be inſufficient to diſturb a ſleeping perſon; for I am eaſily diſturbed.

There is a ſtrong report of a trembling motion being felt at two on *Friday* morning following, which laſted not long, and another at four the ſame morning which laſted ſeveral minutes, without any noiſe. I met counſellor *William Swinburn*, of *Devonſhire-ſtreet*, who told me, that Mr *Love*, an eminent Apothecary at *Weſtminſter*, declared to him, that he had occaſion to be up that night, and was very ſenſible of thoſe trembling motions both times; and I have been ſince informed, that one Mrs *Marſhal* of *Queen's-ſquare* felt the motion about two that morning, but was not ſenſible of the other.

I alſo made ſome inquiries concerning the extent of the ſhock of *Thurſday* morning, and was informed by *John Wolffe*, Eſq; a great ſtudier of nature, that he wrote to his gardener at his houſe at *Haſely-Court* near *Tedford* in *Oxfordſhire* about it, with orders to go to *Henley* upon *Thames*, and inquire whether it was felt there, and he wrote back word, that the market-people who came there from 20 miles round knew nothing of it. I was alſo informed by Mr *Sherwood* of this *Society*, that it was not felt at *Horn-Church*, a gentleman having ſent his ſervant on purpoſe to know; nor any further that way than *Ilford*. It was alſo felt at *Stanmore*, but not at *Watford*, five miles further.

I was yeſterday in the city, and a gentleman ſaid, the fiſh in his ponds were remarkably diſturbed by the ſhock, and that many of them leaped quite out of the water upon the bank; and while he was relating this, another came in, who ſaid, the ſame happened in a pond of his, and in that of another gentleman in his neighbourhood at the ſame time.

12. Mr *Joſiah Boyfield*, of *Gravel-Lane*, near *St George's Fields*, *Southwark*, told me, that at the time of the earthquake, he was not far from his canal, and that it was a clear ſtill morning; the air (as he expreſſed it) as clear as a bell, with not a breath of wind ſtirring. As clear as a bell, except that ſmall hazineſs which there almoſt always is in the morning-air.

——*by* James Burrow, *Eſq; F. R. S. of what Mr* Boyfield *told him concerning the ſame* Ibid. p. 637. *Read* March 22. 1749.

He

He heard, he ſaid, a great noiſe like thunder; which he at firſt took to be the report of ſhip-guns; and immediately after the ground ſhook and trembled under him, and he heard his men ſay, *Look at the fiſh.* The ſound came from the S. E. and moved to the W. or N. W. over *Weſtminſter*: It went off rattling like ten thouſand cannon; and he thought that *Weſtminſter-Abbey*, and all that part of the town was comeing down. The ſound preceded the motion, and was ended before the motion ceaſed.

He went, he ſays, nearer to the canal, and ſaw the water much diſturbed; but he did not himſelf actually ſee the fiſh leap out of the water: however, ſome of his men, whoſe account he can depend upon, ſaw ſeveral roach leap up, and ſaw the fiſh in general ſhoot away in all ſorts of directions, and ſeem to ſhift for themſelves, as if they were frighted and alarmed at what had happened.

——*by* C. Mortimer, *M. D. Secr. R. S.* Ibid. p. 638. *Read* March 22. 1750.

13. I awaked a little after 5; I opened my curtains, and obſerved the ſky hazy: I drank a draught of water, and, looking on my watch, found it to be but $\frac{1}{2}$ an hour paſt 5; and therefore was going to lie down again in my bed; when, leaning upon my right elbow, I firſt felt a ſhock, as if the whole houſe was violently puſhed from the N. W. to the S. E. and then, with equal force, puſhed back again from S. E. to N. W. between which ſhocks I heard a dull noiſe; as if ſomething heavy, but ſoft, rolled to and fro' in the garret over my head. I ſaw the cornice and ceiling of the room ſenſibly move, and then perceived a third and fourth ſhock, ſo ſlow as to give me time to reflect what might be the event; and then a fifth and ſixth, which I own began to frighten me; the ſtrokes ſeeming to be exceeding ſtrong; not at all like a quaking or tremulous motion, but like a forcible ſhoving backwards and forwards; much reſembling the rocking of a coach, when croſſing the rutts, or giving way to another carriage. My ſon, who lay in the next room called out in the midſt of it to know what was the matter? ſaying, the wainſcot partition between the two rooms ſeemed as if rending aſunder. The whole ſeemed to have laſted about 3″. I find that a poſt, which ſupports the above-mentioned partition cloſe to the door-caſe, has given way a little; the paint being freſh cracked; and a crack in a wall upon the ſtairs is grown ſomething wider. The door between the two rooms, which ſhuts with a fall-latch, that was unlatched by the firſt earthquake, and flapped to and fro' 3 or four times, was not unlatched by this laſt; which I think is an argument, that by the firſt the houſe was lifted up, and that it was not ſo by the laſt, but only rocked from ſide to ſide.

I am credibly informed of a pannel of wainſcot in the city being wrenched out of the groove, and not returning into it again.

——*by the Rev.* H. Miles, *D. D. F. R. S.* Ibid. p. 639. *Read* March 22. 1750.

14. It having been commonly reported, that there was much lightning on the morning, and about the time when the earthquake happened on the 8th of this inſtant, I was induced to get what information I could of the truth of it, in this neighbourhood.

* See Art. 8.

A per-

A perſon who went into an open cart at 4 that morning at *Mitcham*, for *London*, ſaw no appearance of lightning at all. Several other perſons, who were abroad, going to work, ſome time before the earthquake happened, declare that they ſaw none; nor were they ſenſible of any noiſe, or motion in the earth.

I got inquiry to be made of the watchmen, who belong to the callico-printers, and watch all night in the grounds, and received this anſwer from one, that he neither ſaw any lightning, nor heard any noiſe, and had no perception of an earthquake; and from another, who watched in other grounds, a mile or more diſtant from the firſt-mentioned, that he ſaw no lightning all that night; but ſays, that he heard an unuſual noiſe about the time in which the earthquake is ſaid to have happened; but this it ſeems was not ſo remarkable as to have cauſed him to take notice of it to any one, had he not afterwards been told there was an earthquake.

I have, beſides this, made inquiry of various perſons, and cannot meet with one, who did ſee any lightning, or who heard others ſay that they had ſeen it.

P. S. I am credibly informed, that a gentleman's ſervant at *Wandſworth* was watering his maſter's horſe by the river-ſide, at the time when the earthquake happened, who found the water ſo much agitated on a ſudden, that the horſe ſtarted back, and would not drink.

15. Whilſt I was in the country, I made as much inquiry into the progreſs and extent of the earthquake northward as I was able; but could collect very little worth notice.

It was felt very ſenſibly at *Northaw*, and at *Gubbins*, the ſeat of Sir *Jeremy Sambrooke* in that neighbourhood, whoſe houſe was ſhaken very much. At a farm $\frac{3}{4}$ of a mile N. E. of *Hatfield*, it was felt by the farmer and his family, but not perceived by the inhabitants of *Hatfield* itſelf.

In like manner at *Hertingfordbury*, a village a ſmall mile W. of *Hertford*, the ſhock was felt diſtinctly; but not obſerved by any of the inhabitants of *Hertford*.

At my own houſe at *Panſhanger*, 2 miles W. of the above-mentioned town, the noiſe was heard twice, at the interval of about $\frac{1}{2}$ a minute, reſembling the rumbling of a cart through the ſtreets; but no ſhock felt either within or without-doors.

This circumſtance of the noiſe being heard without any ſenſible tremor or heavings of the earth, makes me imagine, that the force of the vapour was ſpent before it reached that place; and thereabouts may be reckoned the northern limit of the earthquake; at leaſt, I have not heard of any places more to the N. that were affected by it. It is no wonder, in a ſhock ſo ſudden and alarming, that very few ſatisfactory obſervations are made, either as to the nature of the ſhock, the direc-

——*by the Rev.* Wm. Cooper, *D. D. Dean of* Durham. Ibid. p. 647. *Read* March 29. 1749.

tion of the tremor, the time it was felt, or it's duration; of none of which can I give you any particulars.

——*The Preſident's report of the account given him by* Tho. Burrat, *of* Kenſington. Ibid. p. 681. *Read* April 20. 1750.

16. On *Tueſday* the 24th of this preſent *April* 1750. Mr *Thomas Burrat*, of *Kenſington*, a huſbandman, and bailiff to the R. Hon. *Henry Fox*, Eſq; of *Holland-Houſe*, gave me the following account:

That, being early about his buſineſs in the grounds about ¼ of a mile from the ſaid houſe, on *Thurſday* the 8th day of *March* laſt, as he was going to tell his ſheep (which he does every morning, and which were then lying down on a dry ſloping piece of ground), he heard, as he thought, about ¼ after five a noiſe, much like thunder at a diſtance; which coming, to his apprehenſion, from the N. W. continued ſome ſmall time, growing louder as it came nearer him, and gave a crack (ſo he expreſſed himſelf) over his head; and then went off in the ſame manner it came on towards the S. E.

He ſaid, that the ſky was, to his thinking, quite clear, and without any cloud; and that he ſaw neither lightning, nor any appearance of fire; but that, immediately after the crack, he found the ground to ſhake under him; and that he even ſaw it move where he was (though as dry and ſound a ſpot as any he knows) like a quagmire or quickſand; inſomuch that he could not help being apprehenſive, that it would have opened, and taken him in.

He ſays, that the ſheep he was beginning to tell all ſtarted up at once, as frighted, and preſently began to run, as if purſued by ſomewhat they were apprehenſive of. He ſaid further, that he took notice, that ſeveral crows, which were at rooſt upon ſome trees not far off, all at the ſame inſtant flew away, making the ſame noiſe they conſtantly make when they are affrighted at the diſcovery of a bird of prey, or any other enemy; and that the trees themſelves very ſenſibly trembled and ſhook.

The noiſe he heard began a ſenſible time before the ſhake of the earth; and he judges that the whole matter laſted better than a minute.

He firſt expreſſed himſelf about the direction of the noiſe he heard in the manner juſt above related; and, being aſked again concerning that particular, he explained himſelf by ſaying, that, to his thinking, it came on from between *Hillington* and *Harrow on the Hill*, and went off over *Deptford*: which may be obſerved to be very agreeable to his other deſcription.

Mr *Bird*, the eminent mathematical inſtrument-maker in the *Strand*, told me alſo the ſame day, that he heard, at his houſe, a noiſe, like the diſcharge of a cannon at ſome diſtance, juſt before the earthquake; and that his bed, in which he then was, was very ſenſibly rocked from right to left twice: and that he is well aſſured the feet of the bed were actually lifted up from the floor, during the motion; as he was very ſenſible, by the noiſe they made when they came to the floor again, 4 times in all, twice to his right hand, and as often to his left hand.

——*an account of part*

17. At a pot-houſe belonging to Mr *Oad* in *Gravel-Lane*, a large part of the roof, containing near two ſquare, was intirely thrown down by the

the laſt earthquake, *March* 8. 1750. and ſeveral fiſhermen, then at work, imagined a porpoiſe, or ſome other large fiſh, had riſen under their boat.

by the ſame; communicated by Mr Wm. Jackſon, *Potter, to M. D. Sec. R. S.* Ibid. p. 700. *Read* June 21. 1750.

of a Roof *of a* Pot houſe *at* Lambeth *being flung down* C. Mortimer,

XXXI. 1. By a letter I had from Mr *Oakes* at *Portſmouth*, dated the 19th inſtant, he gives me an account of the inhabitants being alarmed with a ſevere ſhock of an earthquake on *Sunday* the 18th at 6 in the evening; and that it was felt ſtronger at the *Common*, which is about $\frac{3}{4}$ of a mile diſtant.

F. R. S Ibid. 646. *Read* March 22. 1749.

——*at* Portſmouth, March 18. 1749-50. *in an extract of a letter to Mr* J Ellicot,

2. Yeſterday, about $\frac{1}{4}$ before ſix in the evening, a ſlight ſhock of an earthquake was felt here. I don't find it was general, as many people felt nothing of it. It was felt ſenſibly at Mr *Carter*'s and Mr *Taylor*'s; ſo that it is ſomething more than fancy.

P. S. Since writing the above, I am told the ſhock was very violently felt in the *Iſle of Wight* yeſterday.

Read March

——*in an extract of a letter; communicated by* Daniel Wray, *Eſq*; *F. R. S.* Ibid. p. 647. 22. 1749.

3. The firſt part of the preceding week was fine weather. *Wedneſday* night it became damp and cold, and continued ſo all *Thurſday*, with miſts. *Friday* was a fine day; *Saturday* damp and cloudy; *Sunday* morning fine. Juſt before 6 in the evening we had ſome large drops of rain, and a thunder-cloud paſſed to the S. E. juſt as the earthquake happened.

——*in an extract of a letter from the Rev. Mr* Taylor, *vicar of* Portſmouth, *to Mr* Roderick. Ibid. p 649. *Read* March 29. 1750.

The firſt thing perceived was a ſhock, like the ſudden ſtop of a body in motion; a kind of jarring. This was ſucceeded immediately by a gentle motion, nearly in the direction between E. and W. which made 3 or 4 ſlow and deliberate vibrations. Thoſe who ſat facing the E. or W. were moved backward and forward; and thoſe who faced to the N. or S. were moved ſideways. The whole was attended with a noiſe like that of thunder at a very great diſtance. It laſted, as I judge by different accounts, about 4 or 5′′.

I do not hear of any exploſion. The ſaſhes and door in my chamber ſhook, as in a blaſt of wind a little ſtronger than ordinary. Several, who were on the battlements of the church, felt it more violent, and heard the bell-frames and floor ſhake and crack. Few on the ground, or in motion, were ſenſible of it.

It was felt at *Havant*, 7 or 8 miles to the E. and at *Fitchfield*, 7 miles to the W.

It paſſed to, or from, the *Iſle of Wight*, where it affected the ground-floor, as much as the chambers here. It ran along the coaſt between E. and W. but I have not heard that it was perceived at ſea, or went far inland.

I am informed it has been felt at *Guernſey* and *Jerſey*; ſo that if it moved horizontally, it muſt be a conſiderable depth under-ground; the

ſoundings from hence to thoſe iſlands being, in ſome places, 45 or 50 fathoms: ſo that I flatter myſelf, that the ſmall reſiſtance which it can meet with at ſea is ſo diſproportionate to what it muſt encounter under ſo many more fathoms of earth, that a more violent return will rather open and diſcharge itſelf there, than do us any conſiderable miſchief at land.

——*in an extract of a letter from Mr* Benj. Cooke, *F. R. S. to Mr* Peter Collinſon, *F. R. S.* Ibid. p. 651. *Read* March 29 1750.

4. Beſides the ſhock, which happened here * about 6 in the evening on the 18th inſtant, as has been mentioned in the public prints from many neighbouring places, there was another, which was felt by ſome betwixt 3 and 4 o'clock next morning: but whether this latter was as extenſive as the former, I cannot yet learn.

In the evening ſhock, a gentleman of my acquaintance was ſitting alone in his parlour by the fire with the doors ſhut; the ſpaniel-dog, which lay as it were aſleep before him, was ſo terrified at the unuſual motion, that he ran round the room in the greateſt fright and confuſion, as endeavouring to find a way of eſcape.

——*in an extract of a letter from a gentleman at* Southampton *to* Joſiah Colebrooke, *Apothecary, F. R. S.* Ibid. p. 652. *Read* March 29, 1750.

5. We have had nothing of an earthquake in theſe parts till laſt *Sunday* evening, when they had it all over the *Iſle of Wight*. My ſon wrote me the following account of it, which is dated at *Newport*, on *Monday* the 19th:

"Laſt night, juſt at 6 o'clock, as my aunt and I were ſitting to-
"gether, we felt an earthquake, and a terrible one I think. At firſt
"we heard a ſmall noiſe, which we ſuppoſed was a chariot; and, as
"the noiſe grew louder, the houſe began to ſhake; till at laſt the noiſe
"grew ſo loud, and the houſe ſhook ſo much, that we expected it
"would fall down. I believe it continued near a minute; and it was a
"great mercy we were not all conſumed. I confeſs I was dreadfully
"frightened. We had nothing fell down from the ſhelves in our
"houſe; but both our neighbours had things thrown down from their
"ſhelves. I heard a man ſay there was another ſhock this morning be-
"tween 3 and 4, but we felt nothing of that. I believe it was felt all
"over the iſland; for here is a man in town who felt it at *St Helen*'s"

Yeſterday I heard they had a little of it at *Portſmouth* and *Lymington*: and a ſervant-maid in this town ſays, ſhe felt her chair ſhake; and the windows ſhook, and the wainſcot cracked, juſt at the ſame time: but I neither felt it, nor can find any body beſides that did.

——*in a Letter from Mr* Peter Newcome, *F. R. S. to the* Preſ. *concerning the ſame ſhock being felt at* Hackney, *near* London, Ibid. p. 653. *Read* March 29. 1750.

6. I beg leave to give you ſome account of what was felt in our houſe at *Hackney*, on *Sunday* the 18th, a little after 6 in the evening; as we have received accounts of the ſhock of an earthquake being felt at *Bath*, *Portſmouth*, and ſome other places on that day.

My couſin *Peter Newcome* was ſitting in his chamber on the upper floor of the houſe, looking towards the fire, when he plainly perceived the hearth of his chimney to be moved; and immediately felt the chamber rock 3 or 4 times from W. to E. but heard no noiſe, as at the time of that ſhock felt on the 8th.

* Iſle of Wight.

Being

Being much ſurprized, he was running down-ſtairs, and there met with one of the maid-ſervants, who was running in a great fright out of another room on the ſame floor; and, before they could ſpeak, a young gentleman, of about 15 years old, came out of a cloſet juſt by, and they all at one moment aſked each other if what they had felt was not another earthquake.

In a chamber on the floor below, maſter *Hadley*, ſon to the late Mr *Hadley* of this *Society*, was in his bed, being ill of a cold; he felt the bed move upwards ſo ſenſibly, that he imagined ſomebody had got under it out of wantonneſs, and was lifting it up; and actually got out to look under the bed.

The ſame was felt by a ſervant, on the upper floor, at the other end of the houſe; and by another young gentleman underneath in that part alſo. The reſt of the family, being all together below-ſtairs, felt nothing of it.

7. On *Sunday* the 18th inſtant, at a very little after 6 in the afternoon, as miſs *Lethieullier* was ſitting in her dreſſing-room up two pair of ſtairs, fronting to the S. with a book before her, ſhe felt ſuch a ſhock of an earthquake, as ſhe apprehended, that ſhe immediately ran down-ſtairs, frighted; and finding Mr *Lethieullier* her father, and another perſon, ſitting together in the parlour, aſked them, "if they had not "felt another ſhock of an earthquake." But, finding that neither they, nor any one elſe, had perceived any thing like it, ſhe neither ſaid or thought any more of it; ſuſpecting it might be only a ſudden guſt of wind, or ſome other accidental cauſe.

On reading the accounts in the publick papers of a real ſhock of an earthquake being felt at *Portſmouth*, at the *Iſle of Wight*, and at other places, exactly at the ſame time, her father, and the gentlewoman who was in the parlour with him, began to doubt whether the young lady's apprehenſion was not founded upon ſomewhat more than mere fancy or imagination; and Mr *Newcome*'s account ſeems to render it probable that ſhe felt a real motion.

Whether it was, or was not, I don't pretend to determine; and ſhould ſcarce have mentioned it, if it had not ſo exactly coincided with what Mr *Newcome* has communicated. I have no doubt of the fact above rehearſed; having been aſſured of it by all the 3 perſons before ſpoken of, who firſt mentioned it in an accidental converſation upon the ſubject, and afterwards (on being particularly interrogated) poſitively and expreſsly atteſted it.

——*in a letter from* James Burrow, *Eſq; F. R. S. to* Peter Daval, *Eſq; Sec. R. S. concerning the ſame* Earthquake *being felt at* Eaſt-Sheen, *near* Richmond-*Park in* Surry. Ibid. p. 655. *Read* April 5, 1750.

XXXII. I have here incloſed a letter from my neighbour Mr *Bowman*, at *Moleſey*, near *Hampton-Court*; whoſe veracity and abilities to make the proper obſervations, I can depend on. I well remember the extraordinary redneſs, *&c.* in the ſky the evening before, which he mentions. The ſhock which he felt in a chair, was, as I gueſs, in *Italy*; he having travelled much abroad.

——*at* Eaſt Moleſey *in* Surry, March 14. 1749-50. *in a Letter from the Rev.* Stephen Hales, *D. D.*

Without

& *F. R. S. to the* Preſ. *ſerving to incloſe a letter to him from* Walter Bowman, *Eſq;* Ibid. p. 684. *Read* May 3. 1750.

Without any regular connexion with the moon, it happened about 7 days after the ſecond ſhock, on the 14th of *March*, I believe, before 4 in the morning; when full awake, I felt my houſe, for a ſecond or two, ſhake, like a ſpaniel juſt come of water. My bell on the ſtair-caſe rung only one twitch. I roſe, looked out, and ſaw the moon ſhine bright, without one cloud, or one breath of wind; and finding none of my ſervants diſturbed, I returned to ſound and quiet ſleep.

It was exactly of the ſame nature with the ſecond ſhock, a ſhudder of the houſe from top to bottom; ſo that I neither miſtook the one nor the other for an exploſion of Mr *Norman*'s horſe-powder-mills, wherein, you know, he never works above 40 pounds at a time. Here I felt nothing like an exploſion, but a concuſſion, which any man may conceive, from his hand ſhaking a bed upon caſters, if we may compare great things to ſmall. Nor can I deſcribe the ſecond ſhock, felt alſo in bed, compared with this third, otherwiſe than by the ſhudder of a horſe after ſwimming, more ſtrong than that of a dog; while the ſame bell ſounded all in confuſion, as if it had been packed and toſſed in a hamper.

March 13. in the evening, about ſun-ſet, the ſky was dreadfully charged with a deep purple mixed with red, which, from the W. tinged the clouds by the S. quite to the E. and was ſucceeded by a clear effulgent crimſon or pink-colour, luminous, as deep, as ever eye pierced into the azure blue.

I neither have met with, nor heard of, any perſon, who felt this ſecond ſhake which I have deſcribed. But, if any memorials are to be preſerved of theſe ſeveral ſhocks, all which I have felt moſt diſtinctly, I think this ought not to be forgot; becauſe I do not apprehend it to have been ſtrong enough to have waked any perſon, nor to alarm even any one awake in bed. And as for thoſe who were up, and on foot, I do not think they could have perceived it, if I may judge by ſuch a one, which I once felt by a ſingle ſtart of my chair, without knowing what it was, till I compared notes with my more experienced neighbours.

——*at* Bridport, March 18. 1749-50. *in the poſtſcript of a letter from Mr* Nath. Downe, *to the Hon. Mr* Greenville. Ibid. p. 688. *Read* May 10. 1750.

XXXIII. In the morning the ſun ſhone very bright; which, between 11 and 12, was with dark clouds ſo obſcured, as rendered it darker than common. Soon after, a violent clap of thunder, and a heavy ſhower of hail, ſucceeded: after which it grew again ſerene; and in the evening about 6, a ſhock of an earthquake was felt in this town, and the neighbeuring villages, with (thro' mercy) no other damage than a great ſurprize to all who felt it.

——Apr. 2. 1750. *in an*

XXXIV. 1. On *Monday* night laſt, about 10, we felt in this city a ſhock of an earthquake. It was ſenſibly felt by all or moſt of the inhabitants.

bitants. A few bricks were ſhaken off a chimney in *Foreſt ſtreet*: ſeveral houſe-bells were rung; the centinel at the caſtle was ſhaken off his ſeat in the centry-box; the houſes all over the town were ſhaken, and the people terribly frightened and alarmed. It has been felt for ſome miles round the town, particularly at *Barn-Hill*, where the houſes were greatly ſhaken.

extract of a letter from Cheſter, *communicated by* Robert Paul, *Eſqs F. R. S.* Ibid p. 683. *Read* May 3. 1750.

2. We were greatly alarmed with a violent ſhock of an earthquake between 10 and 11. I, who was in bed, was frequently moved up and down; and the bed, having caſters, was removed ſome ſmall ſpace from its proper ſituation.

During the ſhock, a great noiſe was heard in the air; and, ſome nights before, lights were ſeen in the ſky; ſuch as were previous to the earthquake in town.

Thanks to providence, no further miſchief has happened, than the terror this unuſual *phænomenon* occaſioned in our family.

This place is about 2 miles from the ſea.

——*in* Flintſhire, *in an abſtract of a letter from Mr* Pennant *to* Rich. Holford, *Eſq*; *Maſter in* Chancery. Ibid. p. 687. *Read* May 10. 1749.

3. The incloſed extract is the only written account of the late earthquake which I can obtain from any of the Naturaliſts in this country: and as I have converſed with ſeveral intelligent perſons who perceived it, the encloſed extract correſponds very exactly with their ſentiments and obſervations upon it. I have now in my cuſtody the original letter from Mr *Seddon* to Mr *Philpot*; and ſhall not part from it without your direction.

——*in* Cheſhire, *in a letter from Mr* Ph. Warburton *to the* Pref. *ſerving to incloſe an extract of* Jo. Philpot, *a Letter from the Rev. Mr* John Seddon, *of* Warrington *in* Lancaſhire, *to Mr in* Cheſter. Ibid. p. 695. *Read* June 14. 1750.

The late earthquake happened the 2d of *April*, at 10 at night, as nearly as can be determined; if any thing, rather after than before. I was at *Liverpool* at that time, where the ſhock was not ſo ſenſible as at ſome other places; and yet a perſon in company with us that had lived in *Jamaica* a great number of years, and well acquainted with motions of this kind, having felt it in one night, declared it to be the ſmarteſt he ever felt. The duration of the motion was every-where extremely ſhort, not exceeding, as nearly as I can gueſs, 2 or 3″.

Extract of a letter from the Rev. Mr J. Seddon, *of* Warrington *in* Lancaſhire, *to Mr* John Philpot *in* Cheſter, *dated* Warrington, May 10. 1750.

As to the nature of the motion, as far as I can judge of it myſelf, and from the obſervations of others, it ſeems to have been of the horizontal kind, proceeding in an undulating manner from N. W. to S. E. I was in a ſitting poſture, and the motion I felt was like that of a veſſel falling from the top of a wave, and riſing again upon the next. Mr *Breckell* of *Liverpool*, and others that I have converſed with upon the ſubject, repreſent it in the ſame way.

It is, I think, univerſally agreed, that an uncommon noiſe attended the ſhock, a noiſe that much reſembled diſtant thunder, or a hollow rumbling wind: ſome perſons alſo ſay, that they perceived a ſultry ſulphureous ſmell, much about the time of the ſhock; tho' that day and

evening

evening were remarkably cold; and whether this was real, or only imaginary, I cannot determine.

The ſhock was felt as far N. as *Lancaſter*, and as far to the S. as *Wrexham*, and the adjacent parts; in all, about 70 miles N. and S. It was felt as far as *Stockport* and *Altringham* to the E. and quite into *Flintſhire* on the W. that is, about 30 or 40′ E and W.

The ſhock was not ſo great, or of ſo long continuance, as to do any material damage. I think I heard of a large *China* jar falling from a chimney-piece in a gentleman's houſe, of a piece of marble reared againſt a wall falling and breaking, and two or three trifling inſtances of that kind. There is only one thing further that I would mention upon the occaſion: as ſoon as I felt the ſhock, I was immmediately apprehenſive what it was, and went out to ſee whether there was any thing remarkable in the atmoſphere. I then obſerved a very uncommon appearance; *viz.* an infinite number of rays, proceeding from all parts of the Heavens, converged to one point; no luminous body appeared at all. The rays were at firſt of a bright yellow; afterwards they became blood-red. This *phænomenon* was not far from our *zenith*. It continued about 20′, and then diſappeared.

The enſuing night was very ſtormy; a large quantity of hail fell about two in the morning; and the barometer was extremely low.

——*at* Winbourn *in* Dorſetſhire, May 4 1749 *and at* Taunton *in* Somerſetſhire, July 1. 1747. *by Mr* Henry Baker, *F. R. S. to the* Preſ. Ibid. p. 689 *Read* May 17. 1750.

XXXV. As the two remarkable ſhocks of an earthquake, lately felt at *London*, may probably excite a curioſity of knowing what of the ſame kind has happened in other parts of the kingdom within theſe few years, I wrote, about a week ago, to a ſiſter of my wife, who lives at *Winbourne* in *Dorſetſhire*, deſiring her to ſend me the beſt account ſhe could collect, of an earthquake, which, in one of her letters ſome months ago, ſhe had mentioned to have felt herſelf at that place, in *May* laſt year; and the ſubſtance of her anſwer is as follows:

She ſays, that, on the 4th of *May* 1749. about 10 in the morning, ſhe was ſtanding at one of the windows in her chamber, her ſon (a boy about 9 years old) was ſitting on a bed in the middle of the room, and her ſiſter was in another chamber two rooms from her, all on the ſame floor; when they heard a ſudden blow (ſo ſhe expreſſes it) that ſeemed to be very near, which ſhook the houſe ſo much, that the windows rattled, and the floor ſhook very much, and frighted her to ſuch a degree, that ſhe cried out, Lord, have mercy upon me, what is that? ſuppoſing it had been a burſt of thunder. Then looking out, the ſky was very clear, without any cloud near at hand; but there ſeemed to be a heavy cloud hovering at a diſtance, whence ſhe and her ſiſter imagined the ſhock came; for they had then no thought of an earthquake.

There was, ſhe ſays, but one blow, with a noiſe very loud, like the diſcharge of a cannon; which made her ſend to inquire if there were any powder-mills in that part of the country, but was aſſured there were none. Her huſband (Mr *Boſton*) was then at a place called *Caſhmoor*,

Caſhmoor, on the *London* road, 6 miles from *Blandford*, and about 8 from *Winbourne*, where he heard it in much the ſame manner. Their next neighbour was at the ſame time 12 miles diſtant, and heard it there; and every body ſaid it was an earthquake.

It was heard at *Shapeck*, about 4 miles from *Winbourne*; and at a place called *Eaſtbrook*, about half a mile from *Winbourne*, the people ſay it threw the pewter off the ſhelves. She ſays, I may depend upon it as a truth, that it was heard 20 miles round *Winbourne*; and adds, that people were very much frighted, but no harm was done.

I had the honour, two years ago, to lay before you an account of an earthquake felt at *Taunton* in *Somerſetſhire*, and for 40 miles in length as well as breadth, on the 1ſt of *July*, 1747. which was communicated to me by the Rev. Mr *John Forſter*, who happened at that time to be there by accident. It was likewiſe by mere accident I came to the knowledge of what I have juſt now been deſcribing: which induces me to imagine, that ſhocks of this kind may poſſibly happen more frequently than is commonly ſuppoſed, though we hear nothing of them: For, in country places, people are ſo little attentive to ſuch matters, that, unleſs ſome conſiderable miſchief be done, they mind them very little at the time, and, as ſoon as over, think no more about them.

XXXVI. Mr *Arderon* writes me word from *Norwich*, that, on *Thurſday* laſt, the 7th inſtant, as he and a friend were walking to take the air, a little to the W. of that city, they heard, about 7 in the evening, a kind of hollow noiſe, as loud as that of a large cannon. Which noiſe was once repeated nigh the ſame place as an echo, and then continued dying as it were away for about ½ a minute.

They ſaw no lightning, nor any clouds, except a few thin whitiſh ones in the weſtern horizon.

It was heard, he ſays, by great numbers of people in the city of *Norwich*, notwithſtanding the continual noiſe and hurry there: he likewiſe received accounts of it from *Swanthorpe*, 6 miles S. W. and from *Racka*, 4 miles N. E. of that city, agreeing with the above deſcription.

He has not heard that any perſon obſerved any tremor of the earth; and confeſſes his own ſurprize was ſo great, he does not know whether there was or not.

Mr *Wilſon*, a gentleman who was with him, thought the noiſe much reſembled the fall of a great building; and a gentleman at *Norwich* deſcribed it like a large weight falling down upon a chamber-floor over his head.

——*at* Norwich, June 7. 1750. *by Mr* H. Baker, *F. R. S. to the* Pref. *containing an extract of a letter from Mr* W. Arderon, *F. R. S.* Ibid. p. 698. *Read* June 15. 1750.

XXXVII. 1. The air mild and calm, no wind ſtirring, the ſun ſhining bright, at about 45′ paſt 6 in the morning, a ſhock of an earthquake was ſenſibly felt here, and hereabout, attended with a loud noiſe, and crack (as ſome call it): This was perceived both ſouthward, and

——Aug 23. 1750. *in an extract of a letter from*

Maurice Johnſon, *Eſq*; *to* Emanuel Mendez da Coſta, *F. R. S. dated* Spalding *in* Lincolnſhire, Aug. 25. 1750. Ibid. p. 725. *Read* Nov. 1. 1750.

northwardly, for ſome ſeconds. A gentleman from *Newark* in *Nottinghamſhire*, N. E. of us about 30 miles, ſays it was alſo felt there.

——*in a letter of Dr* John Green, *to the ſame.* Ibid.

2. The earthquake was ſenſibly felt through the whole county of *Lincolnſhire*, which is above 70 miles; but moſt ſtrongly on the coaſt. The weather had been for ſome days before mild and calm: an *Aurora Borealis* appeared vertically, ſhooting rays of all colours around, which turned to a very deep red colour.

——*at* Newton *in* Northamptonſhire, *on* Sunday, Sept. 30. 1750. *in a letter from* W. Folkes, *Eſq*; *F. R. S. to his brother the* Preſ. Ibid. p. 701. *Read* Oct. 25 1750.

XXXVIII. 1. On *Sunday* laſt, rather before one, whilſt we were at church, we had an earthquake here. The noiſe, to the beſt of my judgment, continued near a minute; but was not ſo loud as either of thoſe I heard at *London*. Several of the congregation perceived the ground to tremble; but I cannot ſay I did. It is ſaid to have been more violent in ſeveral places in the neighbourhood than here; but this I much queſtion. Alſo the day I went thro' *Stamford* to *Grantham*, in my way to *Yorkſhire*, an earthquake was felt in both thoſe places: ſo that I have been within the knowledge of no leſs than 4 of theſe ſhocks of nature in eight months time; but, thank God, none of them attended with any ill conſequences, any farther than furniſhing room for melancholy reflections upon ſuch a diſagreeable alteration in our climate, which had been generally thought before tolerably free from this calamity.

——*near* Bury St Edmund's *in* Suffolk, *and at* Narborough *in* Leiceſterſhire; *in a letter from* James Burrow, *Eſq*; *F. R. S. to the* Preſ. Ibid. p. 702 *Read* Oct 25. 1750.

2. This morning I have been making a viſit at Lord *Cornwallis*'s at *Culford*, about 4 miles from *Bury* in *Suffolk*. Lady *Cornwallis* (whoſe judgment and accuracy are ſuperior to all doubt or exception, and her veracity ſtill more ſo) aſſured me, that on *Sunday* laſt, about one, as ſhe was ſitting and reading in her dreſſing-room at *Culford*, ſhe ſuddenly felt and ſaw her chair and perſon move backwards and forwards; ſo that ſhe ſearched and examined whether any dog had got under her feet and chair, or any one entered her chamber unperceived; but found herſelf abſolutely alone in the room: whereupon ſhe tried, whether, by laying her hand or elbow upon the table, ſhe could repeat the ſame motion, or any thing like it; but could not. She added, that ſhe felt herſelf a good deal ſurprized at this extraordinary ſenſation, at the inſtant of perceiving it: but neither then, nor afterwards, had the leaſt imagination about an earthquake; till, upon coming down to dinner, ſhe was aſked by Miſs *Charlotte Cornwallis*, her ſecond daughter, a young lady grown up, "Whether ſhe had not felt the earthquake?" Miſs *Charlotte* agreed to the time; and was herſelf alſo ſitting and reading in her own dreſſing-room, which was one pair of ſtairs higher than her ladyſhip's, yet on the ſame ſide of the houſe. However, it was alſo felt by Miſs *Charlotte Cornwallis*'s maid-ſervant, whoſe chamber was in a different part of the houſe, and diſtant from either of the ladies apartments; and who was ſo alarmed at it, as to leave her room, and come into her young lady's, to ſee what was the matter. No one elſe in the

houſe

houſe perceived it. But Lady *Cornwallis* ſays, that, as far as ſhe can learn, they were all upon their feet; none being ſitting, except the three already mentioned.

The houſe ſtands alone in the park: and Lady *Cornwallis* had declined making any inquiry amongſt the inhabitants of the adjacent village; partly, for fear of alarming them with apprehenſions of danger, of which they would be very ſuſceptible from the name of an earthquake; and partly from the little hopes ſhe could have of procuring any tolerably accurate account of the fact from ſuch reporters.

P. S. On our return hither to Mr *Wollaſton*'s, we found a letter from a worthy friend of Mr *Wollaſton*'s and mine, Mr *Metcalfe*, a clergyman of reputation, ſenſe, and fortune; who reſides at *Leiceſter*, and has two livings near that place; one at *Narborough*, the other at *Tilton*: out of which I will tranſcribe a paragraph, which will ſerve to confirm Lady *Cornwallis*'s relation.

"Yeſterday [It is dated *Leiceſter*, *Oct.* 1. 1750.] about noon, we were all "greatly alarmed with a very great ſhock of an earthquake. I was "in the pulpit at *Narborough*; where the whole church ſhook with "ſuch violence, that the congregation expected that the roof was "falling in, and run out of the church immediately, leaving the "poor parſon to ſhift for himſelf. I ſtood my ground; and, by "calling to them, and aſſuring them there was no harm, prevailed "on them to return, and make an end of the duty: but it was with "fear and trembling. It was felt pretty much at *Leiceſter*; but how "much further, I have not heard."

Since the receipt of the above letter, I have read, in the public newspapers, an account of it's having been alſo felt at *Northampton* about the ſame time.

So that no doubt can remain of the ſhock which Lady *Cornwallis* perceived at *Culford*, having been a real earthquake.

3. I beg leave to ſend you ſome memoirs relating to the earthquake, which happened in theſe parts on *Sunday* ſe'nnight, *viz.* *September* 30. the one is a letter from Sir *Thomas Cave*, Baronet, of *Stanford* near *Lutterworth* in *Leiceſterſhire*, a gentleman of good ſenſe, and unqueſtionable veracity. The other is a paragraph taken from the *Northampton Mercury* of this day. As for my own part, being engaged at church in a very ſolemn part of our worſhip, I only remember to have heard a loud exploſion, like that of thunder; but my neighbours aſſured me, they perceived the windows to ſhake and jar. I believe it was more violent in other parts of our county, and the counties adjacent; but I am cautious of tranſmitting any accounts, but ſuch as I think may be depended upon.

——*in a letter from the Rev. Mr* John Nixon, *F. R. S. to Mr* John Ward, *F.R.S. and* Rhet. Prof. Greſh. *ſerving to accompany two letters; one from Sir* Tho. Cave, *Bart. and another from the Miniſter of* Weſton with Sutton *in* Northamptonſhire. Ibid. p. 705. *Read* Oct. 25. 1750.

——*by Sir* Tho. Cave.

We were amazed at ½ an hour after 12 on *Sunday* by a violent ſhock of an earthquake while we were at church; it laſted between 3 and 4′ and was attended with a prodigious rolling noiſe, louder than all the thunder I ever heard in my life, was it collected into one exploſion. Thank God, no damage accrued to any of us, beyond the confuſion it occaſioned.

Northampton, Oct. 8.

On *Sunday* the 30th of laſt month, about ¾ after 12, a ſhock of an earthquake was felt in this town, and in the country round us for many miles; but was not thought to be ſo violent as thoſe which happened at *London* at the beginning of the year. People who were ſitting in the churches, or in their houſes, were moſt ſenſible of it, but thoſe who were walking were not ſo much affected; and many conſidered it at firſt only as the noiſe of a ſudden guſt of wind, or the remote running of a coach or chair. We don't hear of any damage done thereby.

A letter from the Rev. Miniſter of Weſton with Sutton, *in the county of* Northampton, *dated* Oct. 2. 1750.

On *Sunday Sept.* 30. at *Aſhley*, in this neighbourhood, about ¼ before one, whilſt they were ſinging after ſermon, the whole congregation was flung into the utmoſt conſternation, by a very terrible ſhock of an earthquake; the ſingers could ſcarce perſuade themſelves to finiſh their anthem. The reading-deſk ſtands juſt by the ſinging-pew; and I really thought that part of the church betwixt the chancel and the pillar next to it would have ſunk into the earth, with a loud and dreadful noiſe from a ſort of ſubterraneous exploſion, or whatever the learned and curious will term it. After that awful noiſe, and ſomething far exceeding a common tremor, it kept rolling on ſeemingly from N. to S. with an hollow rumbling, like thunder at a diſtance. This uncommon ſhock, I find, upon inquiry, was felt in all the neighbouring towns in *Leiceſterſhire*, as well as in this county; and very likely we ſhall hear that many parts of the iſland were affected by it.

——*further particulars; in a letter from the Rev. Mr* Nixon, *to the* Pr. R.S. Ibid. p. 710. *Read* Oct. 25. 1750.

4. As to the extent of this *phænomenon*, with reſpect to the S. and S.W. (of which alone I am at preſent capable of giving you any information) it ſeems not to have reached much further than *Towceſter:* for it was not perceived at *Stony-Stratford*, 8 miles ſouth of that place on the *London*-road; nor at *Newport-pagnel* in *Buckinghamſhire*, ſomewhat more to the E. of that town.

I ſpent the week before laſt at Mr *Blencow*'s, at *Marſton St Laurence* in the S. W. angle of our county; and found that it had not been perceived there, nor in the other towns on the borders of *Oxfordſhire*.

We went from thence to Mr *Holbeche*'s of *Farnborough*, 8 miles weſtward of *Marſton*, on the confines of *Warwickſhire*, where we could hear nothing of it; tho' it was felt at *Stockton* and *Leamington*, villages lying more to the N. in the ſame county, about 6 miles from *Warwick*.

It did not reach *Warwick*, but paſſed on to *Rugby*, and from thence entered *Leiceſterſhire*. I lately ſent Mr *Ward* a letter, which I received from Sir *Thomas Cave*, Baronet, at *Stanford*, on the borders of that county;

county; whereby it appears, that the ſhock and exploſion were felt in a very ſurprizing manner there.

Wm. Hanbury, Eſq; of *Kelmarſh*, in the road from *Northampton* to *Market-Harborough*, told me laſt week, that it was felt ſo violently there, that the miniſter and the congregation went out of the church; the roof of which ſeemed to be disjointed, and ready to fall: and his Lady, who was at home, leaning forwards to read, was ſhaken out of her chair upon the floor. I hear that ſeveral repeated vibrations of the ſhock were perceived at *Peterborough*; the particulars of which I expect ſoon from a gentleman who lives in that place.

In anſwer to your ſecond query, I find different accounts given by different perſons. Sir *Thomas Samwell*, Baronet, at *Braddon*, 3 miles W. of *Towceſter*, being in his garden with two of his ſervants, heard nothing of the exploſion. Some of my neighbours tell me they perceived it. Mr *Brookes*, our clerk of the peace, informs me, that he was walking abroad, at a conſiderable diſtance from any buildings, at a place called *Oakly*, 3 miles from *Kettering*, and there heard the noiſe as of a riſing ruſtling wind, during the time that he walked 20 or 30 yards.

The difference of theſe accounts may, in my opinion, be reconciled, by ſuppoſing, that the exploſion might have been heard abroad in ſuch places where it was more violent; and not in others where it was leſs ſo.

As for any thing (I preſume you meant lambent flame, vapour, *&c.*) being perceived on the ſurface of the ground, before or during the earthquake, nothing of this kind has as yet been mentioned to me from any quarter.

I find there has been a report of a meteor, like a ball of fire, appearing in the morning before the ſhock was felt; but it is, by the judicious part of the world, ranked among the other *mirabilia* uſually invented upon theſe occaſions to amuſe the vulgar.

5. The ſhock of an earthquake lately felt here, which has been ſo much talked of, and in ſome public papers magnified far beyond the truth, happened on *Sunday*, *Sept.* 30. about 20′ after 12. Our *Mercury* ſtrangely fixed it at $\frac{1}{4}$ before one; which is ſo palpable a miſtake, and contrary to the certain knowledge of ſo many hundreds of people, that I could not but be ſurprized to ſee it.

The effects here were by no means ſo conſiderable as were repreſented, eſpecially at *London*. I was at that time in company with a pretty large number of friends, juſt returned from divine worſhip, and hardly ſet down in the parlour; but no one of us felt any thing of it; and if we heard any noiſe, did not diſtinguiſh it from a coach: but ſome gentlemen, who were retired into their ſtudies up two pair of ſtairs, plainly felt it; yet they were not the twelfth part of the perſons then in my houſe, who all, whether on the ground, or firſt floor, were quite inſenſible of it. However, it is certain that a great number of perſons in different

— *in a letter from the Rev.* P. Doddridge, *D. D. to Mr* Henry Baker, *F.R.S.* Ibid. p. 712. *Read* Oct. 25. 1750.

different parts of the town, perceived themſelves lifted up by it, as they were in their houſes, though hardly any in the ſtreets took notice of it. Thoſe that accurately obſerved it, deſcribe it as ſomething horizontal rather than perpendicular. A Lady of my acquaintance ſtanding with her face to the S. W. plainly felt her heels lifted up, and was thrown ſo much on her toes, that ſhe was in danger of falling,: and it was obſerved, that ſome caſements were moved outward, as if an attempt had been made to force them open, and the clattering of ſaſhes was as when a ſtrong wind blows againſt them.

In the long ſtreet that runs from S. to N. it was obſerved, that the ſhock was felt more on the eaſtern than the weſtern ſide of the way; and I think the whole eaſtern part of the town was moſt affected. Dr *Stonehouſe*, who lives in that part of it, felt it with great violence, as if a loaded waggon had run ſtrongly againſt the gable end of his houſe: and tho' the walls are remarkably thick, he was greatly alarmed with an apprehenſion that they would have fallen.

What further confirms this remark of the horizontal, or at leaſt oblique direction of the impulſe is, that a cradle was rocked by it. In the houſe of Mr *Yeoman*, where our little philoſophical ſociety meets, it threw down a board from the teſter of a bed; yet Mr *Yeoman* himſelf did not feel it.

There was a report, that in *Abington-ſtreet* ſome chimnies were thrown down; and this brought numbers of people from different parts of the town, to ſurvey the ſuppoſed ruins; but it only ſerved to illuſtrate the uncertainty of rumour. However, it was true that a few bricks were thrown down from a chimney in *College-Lane*.

It is very certain, that all who felt the ſhock heard a hollow ruſhing noiſe; which, ſo far as I can learn, ſeemed to come in a direction from the S. W. to the N. E. In rooms where ſeveral perſons were together, ſome were ſtrongly ſenſible of it, while others felt nothing at all: and (*cæteris paribus*) I think it was felt more ſenſibly by thoſe above than thoſe below, and by ſuch as were ſitting, ſtanding, or leaning, rather than walking.

A lumbering kind of noiſe was heard by ſome in lower apartments, as if ſome one over their heads had fallen down on a ſudden, with a dead weight; and ſome thought they heard ſuch a noiſe in the floor beneath; ſome thought the quivering of the ground continued longer than others apprehended; but I have met with none who in this reſpect were ſo accurate in their obſervations, as my ingenious friend Mr *Shippley*, who aſſures me that he felt four diſtinct concuſſions (the ſecond and third of which were much more violent than the firſt and laſt) all within three or at moſt 4″.

As far as I can learn from the moſt diligent inquiry I can make, the tremulation of the ground extended itſelf at leaſt 60 miles in length from S. to N. and from W. to E. about 25, or at moſt 30.

It

It did not affect either *Newport-pagnel*, or *Towceſter*, to the ſouth; but was felt very near the latter, and at leaſt 6 miles S. of this place, and all the way between that and *Nottingham*, and a little beyond it eaſtward; it ſcarce reached *Higham-Ferrers*, and was not felt at *Coventry*, and but very feeble within 5 or 6 miles to the E. of it: but its greateſt violence ſeemed to have been ſpent on the villages of *Creaton*, *Cotteſbrook*, *Kilmarſh*, *Maidwell*, *Eveſton*, and ſome other ſmall towns within 4 or 5 miles of *Market-Harborough*, moſtly between us and that place.

At *Creaton*, a friend of mine was ſo moved, as he ſat at dinner, that his elbow ſtruck againſt the wall, tho' he ſat at ſome diſtance from it; the roof of the houſe gave a great crack; and in a neighbouring houſe a braſs kettle was thrown down, as in another a plate of pewter was.

At *Cotteſbrook*, *Kilmarſh*, and *Weſton*, the congregations, which were not yet come out of their reſpective churches, were all exceedingly terrified; ſome ſhrieked out, others quitted the place; and the worthy clergyman at *Weſton with Sutton* near *Harborough*, ſays, in a letter publiſhed in our *Mercury*, dated the 2d inſtant, "that, as they were ſing-"ing after ſermon (he adds, a quarter before one) the whole congre-"gation were thrown into the utmoſt conſternation, ſo that the ſingers "could hardly prevail on themſelves to finiſh the anthem." He adds, "that he thought that part of the church betwixt the chancel and the "pillar next to it, would have ſunk into the earth; and that it was "attended with a loud and dreadful noiſe, from a ſort of ſubterraneous "exploſion."

At *Maidwell*, Mr *Scawen*, leaning upon a large marble chimney-piece, was violently ſhaken; and in the neighbouring pariſh of *Kilmarſh*, Mrs *Hanbury*, who was then reading by her fire-ſide, her chair being tilted forward, was thrown down on her hands and knees; and the whole pariſh at church were ſo alarmed, that they broke up the aſſembly, and ran out into the church-yard; but the Miniſter perſuaded ſome of them to return, and diſmiſſed them (as I am told) with an *extempore* prayer proper to the occaſion.

Some ſtrange ſtories have been told of much more violent effects produced elſewhere; particularly that a chaſm was opened at a garden at *Daventry*; but I can find no real foundation for them.

No building, that I can learn, has any-where been thrown down; but I am very credibly informed, that a beam in the new toll-houſe near *Harborough* was ſplit by the ſhock: and one tragical effect is certain; I mean, that Mrs *Alicock*, wife to the chief gentleman in *Loddington*, who had been delivered of her firſt child a few days before, and was in a very fine way, was ſo alarmed with the accident, that ſhe expired within a few hours, to the great grief of all that knew her.

I was ſurprized to ſee how little the inhabitants of *Northampton* were impreſſed with this awful (though by no means ſupernatural) event: the ſound of ſuch a ſhock was, in a manner, grown familiar to their ears, by

by what they had heard from *London*, and other places. Many did not themſelves perceive it; others found it very gentle; and in a very few hours it ſeemed to have affected them no more than a ſhower of rain.

I obſerve, that moſt accounts from the northern parts date the ſhock later than we felt it here; but that may perhaps be accounted for by the difference of clocks; but where they were moſt exactly adjuſted, all agree pretty well as to the time. I find alſo, that the degree of the ſhock was very different in nearly contiguous places. Thus at *Eaſton-Maudit* it was hardly felt at all; but at *Caſtle-Aſhby* was very violent. It was ſenſibly perceived at Lord *Pomfret*'s, and not at all at *Towceſter*, tho' within $\frac{1}{2}$ a mile of it; and, generally ſpeaking, the higher places were moſt affected, tho' in ſome it was quite otherwiſe.

It had been calm cloudy weather for ſeveral days before; and what little wind there was to be perceived was generally N. W. The height of the Barometer was as follows, on and about this remarkable day; *viz.*

Thurſday, Sept. 27.	29—95
Friday, Sept. 28.	29— 9
Saturday, Sept. 29.	29—88
Sunday, Sept. 30.	29—79
Monday, Oct. 1.	29—68
Tueſday, Oct. 2.	29—80
Wedneſday, Oct. 3.	29—83

The morning on which this *phænomenon* happened was remarkably calm; but quickly after the ſhock the wind roſe, and clouds which had covered the Heavens for ſeveral days, were pretty much diſperſed. On *Monday*, and on *Tueſday* and *Wedneſday*, the ſun ſhone clearly all day long. There was a report, that, on the morning of that *Sunday*, about 4 o'clock, a ball of fire was ſeen; but I could not trace it to any certainty. On *Monday* night the ſky in the eaſt was as red as blood; and, on *Tueſday* night, we had abſolutely the fineſt *Aurora Borealis* that I remember to have ſeen; of which I ſhall add a ſhort account, by way of poſtſcript, when I have added a miſcellaneous circumſtance or two to thoſe I have mentioned concerning the earthquake.

I am told, that, in ſome places, two ſhocks were felt, nearly at the ſame time, and within a few ſeconds of each other. This is ſaid to have been the caſe at *Noſely* in *Leiceſterſhire*, which ſtands very high; and at *Yelvertoft* in this county. But I have received the moſt certain account of this from *Hill-Morton*. Mr *Pool*, who keeps the turnpike there, and is remarkably curious, for a man in his ſphere of life, informs me, that he felt himſelf moved, as he ſat in his chair, in ſuch a manner, that he thought ſomebody had been at the door; or (if I underſtand him right) as if ſomething had fallen againſt it: and when he came to the door, about 2 or 3″ after, he felt a motion that he certainly knew to be an earthquake.

I do

I do not hear of any thing ſeen in or upon the ground; unleſs the caſe of a good woman at *Welden* may be accounted an exception; who ſays, that, while ſhe was ſhaken by it, ſhe ſaw the ground move around her.

Mr *Scawen* is very confident, that he heard that ruſhing noiſe (ſo generally ſpoken of by all who obſerved any thing extraordinary) not only before, but after the ſhock; and that he could by both trace the direction mentioned above.

I ſhall only add, that a very worthy lady of this town (niece, as I remember, to Sir *Hans Sloane*) obſerved, that, juſt before the ſhock, her birds drooped remarkably, and hid their heads under their wings: a circumſtance which is often obſerved in *Italy*, and other places where theſe *phænomena* are frequent.

Theſe are the moſt material circumſtances I have yet had an opportunity of collecting; and if any thing elſe, which ſeems at all worthy of notice, ſhould occur, I ſhall be very ready to impart it.

P. S. The principal circumſtances attending the *Aurora Borealis* mentioned above, were as follows:

On *Tueſday*, the ſecond of this month, as I was walking home from a neighbouring village, between 6 and 7 in the evening, the ſky being remarkably clear, and the moon then ſhining with delightful luſtre, I happened to turn, and obſerve in the N. E. a pretty large cloud, nearly in the form of a globe; which ſeemed firſt of a whitiſh, then of a very luminous appearance. It ſeemed connected with a cloud, which was dark, and ſomewhat bluiſh, and ſpread itſelf (almoſt like a vaſt beam of a building) from the N. E. to the N. W. At each end it was a little bent towards the horizon; but by far the greater part was in a horizontal poſition, and ſeemed to occupy the northern part of the Heaven, from the altitude of 10 to about 15°. The cloud I firſt mentioned ſoon appeared like a globe of lucid fire, much brighter than the moon; and ſhot coruſcations, ſometimes in a perpendicular direction, but much more towards the dark horizontal beam, if I may be allowed ſo to deſcribe it (thro' which, by the way, I could not then diſcern any ſtars). Thro' this the light darted from N. E. to N. W. ſo that at length it ſeemed all in a blaze; and from it there ſhot up ſeveral luminous pillars, perpendicular to the horizon, and directed towards the zenith. They were of very unequal lengths, and ſome of them appeared in a conical rather than a cylindrical form. As thoſe to the W. brightened, thoſe firſt raiſed diſappeared; till at length all the horizontal track of light vaniſhed, and ſome long truncated pillars, often varying their length, remained in the weſt; often riſing almoſt to the zenith, but generally ſeeming to hang between 30 and 70°, ſo far as I could conjecture. When this beautiful appearance ceaſed, the ſky appeared reddiſh in the eaſt; what before conſtituted the lucid globe, ſeemed reſolved into light clouds, of various

forms; and that part of the horizon looked much as it does in a ſummer's morning, when the ſun is within a few minutes of its riſing, and tinges the clouds of a light red. But all this ſeemed to diſperſe in a few minutes, about 7; and I neither ſaw nor heard of any thing remarkable.

——*by Mr* ——*ſteward to the earl of* Cardigan. *Dated* Deene, Oct. 2. 1750. *Read* Oct. 25. 1750.

6. In the morning, $\frac{1}{4}$ before one, there was a gentle weſterly wind, ſomething cool; but for ſome time before the earthquake happened, it was quite calm and cloſe, and much warmer. The air was very dry, and filled with clouds that had no motion, but prevented the ſun's appearing (I think) all day. The noiſe that preceded the earthquake was, for a few ſeconds, like the rumbling of a coach upon a bridge, or thunder at a diſtance, when there were two conſiderable exploſions very near one another, which gave the great ſhock; and after that the noiſe continued as before for about half a minute, the earth trembling all the while; but I don't find any body perceived any ſulphureous ſmell.

The direction of the earthquake was from W. to E. as was very eaſily diſcerned by every body that was out of door, as I was, and took notice of the noiſe.

It is probable it began in *Derbyſhire*, or ſome of the counties to the W. of that (for I am informed it was felt as much at *Derby* as here, and at all places between); and paſſed off the iſland thro' *Lincolnſhire*, and part of *Cambridgeſhire*.

The breadth from N. to S. I imagine to be 40 or 50 miles; of which much the greateſt part lay N. of this place.

The force of the ſhock was chiefly, if not intirely, lateral; and ſo conſiderable, as that ſeveral people, who were ſitting in chairs, catched at the walls, tables, and ſuch things as ſtood next them, expecting they ſhould be thrown down: buildings of all kinds were ſhaken greatly; and the beds, chairs, and ſuch things as ſtood above-ſtairs were diſplaced, and rocked about very much: windows were ſhaken as if they would have been broken; and in ſeveral places pewter upon ſhelves in kitchens thrown upon the floor.

At ſeveral churches, where divine ſervice was not finiſhed, both in this county, *Rutland*, and *Leiceſterſhire*, the people were ſo alarmed, that they ran out, fearing the churches would fall on their heads; and ſome were ſo terrified, they ſwooned away.

At *Stonton*, ſome of the plaiſtering of the church was ſhaken down; which moſt terribly frightened the people that were in it, and obliged them to run out. ——I have not heard of any damage being done by it more than ſome chimnies thrown down, but nobody hurt by them.

P. S. Deene ſtands cloſe to the road which leads from *Northampton* to *Stamford*; it is about 25 miles from the former, and 10 from the latter.

7. The

7. The firſt ſhock appeared to us at about half an hour paſt 12, at our houſe, as if a large ſtack of chimnies had fallen through the roof upon the chamber-floor over our heads (which at firſt we took to be the caſe, but in a very few ſeconds recollected what it was). This was ſucceeded by a prodigious rolling noiſe, as if the whole houſe was falling upon us, which we expected it would do, before we could get out of it. When we were out, we could not help looking behind us, to ſee if any thing was fallen; but found all ſafe. We could perceive the floor, *&c.* to ſhake very ſenſibly; and a walking-ſtick, that ſtood in one corner of the parlour, was thrown down: it alſo ſhook down ſeveral large ſtones from off an heap that lay in the yard.

A gentleman, who was walking from his own houſe to dine with us, happened to lay his hand upon a gate, to open it, juſt after the firſt noiſe, and found the gate-poſts, *&c.* to ſhake and totter about as if they were falling. In a great many places ſervice was not ended at church; and in ſome the whole congregation ran out, and happy they that could get out firſt. A gentleman of fortune near *Leiceſter*, narrowly eſcaped being killed by one of his own chimnies; which fell ſo near him, that ſome of the bricks grazed upon his ſhoulder.

By what I can hear, it reached 30 or 40 miles from us each way; and I fanſy we were not far from the center of it.

As near as I can judge, it laſted about 20″ at leaſt; but it's duration and appearance was different to people in different places.

—— *in a letter from Mr* Hen. Green *to Mr* James Ayſcough, *Optician, in* Ludgate-Street. Ibid. p. 723. *Dated* Rolleſton *in* Leiceſterſhire, Oct. 22. 1750. *Read* Oct. 25. 1750.

8. The Rev. Mr *Daniel Goodrich*, at *Oundle* in *Northamptonſhire*, has ſent me an account of the earthquake felt in that and the neighbouring counties, *Sept.* 30. at 12 ½ at noon.

He informs me, that he was then at *Uppingham* in *Rutlandſhire*, ſitting in a room intent on writing and thinking, when he was ſurprized with a ſound very ſtrong and awful; which, at the firſt moment, made him think of the rattling of a coach upon the pavement: but this apprehenſion was immediately corrected by ſomewhat very different in the ſound, and raiſed in him an idea of the craſhing of a falling houſe. He felt himſelf in a very ſhaking way: the table at which he ſat ſhivered, and the windows of the room jarred: but he did not think of an earthquake, till the people of the houſe came into the room, and told him what had happened. Some ſlates were ſhaken off the houſes, and in one houſe the hammer of a clock ſtruck the bell: ſome chimnies were thrown down; many talked of a ſenſible heaving of the ground: but, he ſays, he is credibly informed, that, in one houſe in that county, the mud floor was cracked not a little in 3 or 4 places. In one houſe, in the town of *Uppingham*, where two men and a woman were ſitting, upon the approach of the ſound (tho' they had no thought of an earthquake), the men could hardly draw their breath in the houſe; but were immediately obliged to go out for freſh air; but the woman felt no diſorder. My friend adds, that attentive obſervers apprehended the ſound to have moved from the N. to the S. or from N. W. to S. E. and

—— *in a letter from the Rev.* Hen. Miles, *D. D. F. R. S. to Mr* H. Baker, *F. R. S.* Ibid. p. 726. *Read* Nov. 1. 1750.

that himſelf had the ſame apprehenſion; and that, according to his preſent intelligence, the whole ſhock was felt in the counties of *Northampton*, *Leiceſter*, *Nottingham*, *Rutland*, and *Lincoln*, affecting a track of the county of about 60 miles in length, and as much in breadth. I have given you the account in his own words, with very ſmall variation.

——*a letter from the Rev. Mr* John Nixon, *F R.S. to the* Preſ. *ſerving to accompany a letter from Mr* William Smith *to Mr* Nixon, *giving a very particular account of the* Earthquake *felt on* Sept. 30. 1750 Ibid p. 727. *Dated* Higham, November 8. *Read* Nov. 15. 1750.

9. This waits upon you with a letter I have juſt received from *Peterborough*. I hope the veracity of my correſpondent, who is Regiſter of the place above-mentioned, and a gentleman of good credit, will atone for ſome defects in the form of his account.

Since I had the honour to write to you laſt, I have met with two gentlemen in my neighbourhood, one of them a clergyman, who aſſured me, that, about 6 or 7 on the morning before the late earthquake, they both ſaw a ball of fire in the air, reſembling the meteor commonly called a falling ſtar; only with this difference, that this phænomenon, after running ſome ſpace, burſt into ſeveral ſtreaming rays, conſiſting of ſparks of fire, in the manner of a ſky-rocket.

Reverend Sir,

I Received your favour of the 16th current; and the following account of the late earthquake, as it was variouſly felt here, is an anſwer thereto; which (as I could gather it from others, and collect of myſelf) pleaſe to take as follows:

Some little time after morning ſervice at the cathedral church was over, as near as I can gueſs about 25 or 30 minutes paſt noon, as I was in my garden with two friends, it being exceedingly calm, the ſky ſomewhat covered with light mottled clouds, I took notice of a hollow odd noiſe, and at firſt judged it to be a coach under the monaſtery walls; but ſoon found it was otherwiſe; and all of us then judged it to be thunder at a diſtance, but pretty loud, and of a hollow report. I was then 40 yards at leaſt from my own houſe, and about half that diſtance from Mr Archdeacon *Browne*'s, and not any buildings to the S. of us. I heard the noiſe of the exploſion at leaſt 2′ after I took notice of it. It's courſe, as I could plainly diſtinguiſh, was from N. E. to S. W. or thereabouts. We felt no ſort of ſhake, nor did my wife or family in the houſe. My next neighbour to the S. of me, and his family, being at dinner, felt the ſhock after the noiſe had partly paſſed, and were ſenſible what it was; but not very much, and but for a ſhort ſpace of time.

A perſon of very good reputation, who lives in the *Minſter-Cloſe*, was then in a chamber, heard the noiſe, ſuſpected the cauſe (it being *Sunday*, and no carr about), immediately threw up the ſaſh, and obſerved, as the noiſe of the exploſion decreaſed, the ſhock came on: the tables, chairs, *&c.* in the chamber ſhook; the windows clattered: he very ſenſibly felt the ſhock, which he affirms laſted a minute at leaſt; only,

only, when it firſt ſhook, it was with the greater violence, and dwindled away by little and little, till 'twas gone, in the ſame manner as went the noiſe. The people below were all affected by it; but not ſo much, or ſo long. One other perſon in a narrow ſtreet in the town, ſitting in a low room, with a brick floor, heard the noiſe, but judged it to be an odd lumber above ſtairs, or ſome carriages coming on (as moſt people firſt thought who were within doors); and preſently the floor under his chair heaved, and continued in a ſurprizing agitation for 1′. He was very much alarmed, and ran into the ſtreet, where a great number of people inſtantly appeared; ſome to ſee if any coaches or carriages were coming, others to get away, expecting their houſes were tumbling; and others finding ſomewhat extraordinary had happened, but at that inſtant did not know what, and came to ſee, *&c.* The perſon who felt the ground heave was ſo frighted, that he became ſick thereupon. At *Longthorpe*, a mile W. of us, the clergyman who did duty there was juſt then ſat down to dinner with Sir *Francis St John*, Bart. and his daughter, when a rumbling noiſe was heard, particularly in the chimney. Sir *Francis* expected the ſame was on fire, got up to ſee; which ſuddenly ceaſed, and immediately was ſucceeded by a concuſſion of the ground. I had this from the clergyman, who tells me further, that he felt it ſo ſenſibly, that he was obliged to relinquiſh his chair, and, when upon his legs, to lay his hands on the table to ſupport himſelf. Miſs *St John* was in like manner affected. Sir *Francis* only heard the noiſe, which he compared (and, for what I know, very properly) to the exploſion of a cannon at a diſtance, not being in the leaſt ſenſible of any motion under foot, altho' the diſtance between the parties was very inconſiderable. The ſideboard, with the clattering of glaſſes, *&c.* they thought was tumbling: they were ſo ſurprized, that he doth not remember how long the trembling continued. A perſon in the ſame village affirms, that, being ſitting, he was ſuddenly lifted twice or thrice on the ground, as with a ſpring, and dropped again. Many people felt it there in various ſhapes. At *Caſtor*, a mile and half ſtill farther W. one Mr *Serjeant* ſays, that, looking out of a window a conſiderable height, he found the houſe reel more than once, and then come into it's place again with a jolt. Many very odd inſtances we have of it. Some heard the noiſe, and felt not the ſhock; others felt it, and did not hear the noiſe. I am informed it was felt at *Boſton*, which lies about 30′ near N. of us; and it was felt a few miles to the S. So that its extent here, from N. W. to S. E. or thereabouts, ſeems to be about 40 miles.——Upon the whole, I find, the higher one was, as farther from the centre, the more the ſhock was felt; that it was local; the ſound of the exploſion was heard as well abroad as in the houſes, though people differently ſituated judged differently what the ſound was; that not any ſmoke, vapour, or flame, appeared on the ſurface, as I have heard.

XXXIX.

Part of a letter from M. de Reaumur, *F. R. S. to the* Pref. *concerning an* Earthquake *felt in* France, Oct 11. 1749. Ibid. p 691. *Dated* Paris, April 23. 1750. *Read* May 17. 1750.

XXXIX. On *Saturday*, *Oct.* 11. 1749. about 7 in the evening, there was an earthquake in *France*, which, according to my own inquiries, reached one way above 60 leagues in extent, from our coaſt of *Poiƈtou* beyond *Luçon*, as far as the neighbourhood of *Blois*. I was then at my own houſe at *Reaumur*, and ſitting in a cloſet on the ground-floor, where I had only notice of it by a noiſe like to the rattling of a coach over a rough uneven pavement, which ſeemed to grow ſtronger, as at different fits, for about a minute and a half. I went out of my cloſet to inquire what others might have felt, as I had not been at all ſhocked myſelf, in other parts of the houſe; and I met with ſome ladies juſt come in a fright from the apartments up one pair of ſtairs; and a learned Father of the *Oratoire* that was with me, and was juſt then run down, upon it's ſhaking, from a turret, on the top of the houſe, the motion of which had very much ſurprized him.

In the village ſeveral people alſo felt the ſhake; but others, that happened to be employed, were not ſenſible of it. I was informed, that, in ſome other villages and country-towns, it was more conſiderable than where I was; but I did not hear that it did any-where any miſchief worth ſpeaking of.

Extract of a letter from Dr Mackenzie, *to Dr* Mead, *F.R.S. concerning* Earthquakes *at* Smyrna. Ibid. p. 700 *Dated* Conſtantinople, May 23. 1750. *Read* July 5. 1750.

XL. I obſerve, in Mr *Touchit*'s Journal, that a certain ingenious gentleman would not allow the laſt ſhock of an earthquake in *London* to be an earthquake, becauſe it was not central; but rather calls it an airquake, becauſe it was lateral. I have felt many ſhocks, ſince I have been in this country; particularly in *Smyrna*, 1739. when, after the great ſhock on *March* 24. there were ſome ſhocks every day for a month; ſo that few or none went into houſes before the latter end of *April:* when I had time enough to obſerve their motion, which was regularly periodic, or about the ſame hour every day. But I muſt differ from this unknown gentleman's opinion ſo far as to ſay, that every one of thoſe ſhocks was lateral, moving from the S. to the N. and, as near as I could judge, the motions formed acute angles, ſomewhat reſembling the motion of lightning.

On the cauſes of Earthquakes, *by the Rev.* W. Stukely, *M.D. & F. R. S. to the* Pref. Ibid. p. 641. *Read* March 22. 1749.

XLI. 1. When ſo great and unuſual a *phænomenon* as an *earthquake*, and *that* repeated, happens among us, it will naturally excite a ſerious reflection in every one that is capable of thinking; and we cannot help conſidering it in a philoſophical as well as religious view. Any mind will take the alarm, when we perceive a motion that affects the earth, that bears the whole city of *London*, and ſome miles round; and at the ſame time, whilſt it gives us ſo ſenſible a ſhake, ſo gently ſets us down again, without damage to any buildings, and without a life loſt.

In the works of Nature and Providence there are no degrees of *Great* and *Little:* compariſons are incompatible; nevertheleſs we ourſelves are more affected with what ſeems great, in our own apprehenſions; but an OMNIPOTENT POWER admits of no diſtinctions; and whilſt pro-

prodigious effects are produced from cauſes imperceptible, it rightly claims our ſerious attention, as well as wonder; nor need we loſe ſight of the theological purpoſe of theſe amazing alarms, whilſt we endeavour to find out the philoſophy of them.

Permit me, then, to throw in my thoughts on the cauſe of earthquakes. I did not enter into the common notion of ſtruggles between ſubterraneous winds, or fires, vapours, or waters, that heaved up the ground, like animal convulſions; but I always thought it was an electrical ſhock, exactly of the ſame nature as thoſe, now become very familiar, in electrical experiments.

When we reflect on the unuſual winter now paſt, beyond what occurs to any one's memory, that it has been dry and warm to an extraordinary degree, the wind generally S. and S. W. and *that* without rain, we may, with much reaſon imagine, that the earth has been in a ſtate of Electricity, ready for that particular vibration wherein Electricity conſiſts.

And that it has been ſo, we may further conclude from the extraordinary forwardneſs of vegetation, from the frequency of the northern lights, and eſpecially of that called *Aurora auſtralis*, which are with us infrequent, and twice repeated, juſt before the earthquakes (being of ſuch colours as we had never ſeen before), and removed ſouthward, quite contrary to thoſe common with us.

Add to this, that ſome foreigners among us, from *Italy*, and thoſe parts, where earthquakes are frequent, obſerving theſe lights, and the particular temper of the air, did actually foreſee the event of an earthquake. All theſe matters concur, in ſhewing, that the earth was in a ſtate of Electricity, beyond what has ever been in our memory.

Admitting this, there is nothing wanting, to produce the wonderful effect of an earthquake, but the touch of any non-electric body; and that muſt neceſſarily be had *ab extra*, from the region of the air, or atmoſphere.

We had lately a very pretty diſcourſe read here, from Mr *Franklyn* of *Philadelphia* *, concerning thunderguſts, lights, and like meteors. He well ſolves them by the touch of clouds, raiſed from the ſea (which are non-electrics), and of clouds raiſed from exhalations of the land (which are electrified): that little ſnap, which we hear, in our electrical experiments, when produced by a thouſand miles compaſs of clouds, and *that* re-echoed from cloud to cloud, the extent of the firmament, makes that thunder, which affrightens us.

From the ſame principle I infer, that, if a non-electric cloud diſcharges its contents, upon any part of the earth, when in a high-electrified ſtate, an earthquake muſt neceſſarily enſue. As a ſhock of the electric tube in the human body, ſo the ſhock of many miles compaſs

* Read *Nov.* 16, 1749. publiſhed with other tracts on Electricity; by Mr *Peter Collinſon, F. R. S. London* 1750. *8vo.*

of

of ſolid earth, muſt needs be an earthquake; and that ſnap, from the contact, be the horrible uncouth noiſe thereof.

I have been informed, by thoſe who were up, and abroad, the preceding night, and early in the morning, that coruſcations in the air were extremely frequent (which confirms us in the notion of the earth's being then in an electrified ſtate); and that, a little before the earthquake, a large and black cloud ſuddenly covered the hemiſphere; which probably occaſioned the ſhock, by diſcharge of a ſhower.

It may be ſaid, that, if this were the caſe, earthquakes would happen much oftener than we find them. It may be anſwered, that they probably do, much oftener than obſerved: but *ſlight* ones; becauſe of the earth's being *ſlightly* electrified. And ſuch a winter as this has not been known before; to which we attribute the preſent earthquake.

The reaſon is obvious, why earthquakes are not ſo frequent with us, and the northern regions in general, as in *Italy*, and more ſouthern climes; and a due conſideration of it confirms our reaſoning. All Electricity requires great *dryneſs* and *warmth*; and I doubt not but earthquakes, of a ſmall degree, have and do frequently happen. And many people now recollect, that they have been ſhaken in their beds; though they took no notice of it then, having had no experience of an earthquake.

All that we have ſaid upon the ſubject receives great ſtrength from *this* particular, that water ſtrengthens and conveys the force of Electricity. From whence we may account for that obſervation, that the moſt dreadful effects of earthquakes are always felt in maritime towns; as *Port-Royal* in *Jamaica*, *Lima* in *Peru*, *Meſſina* in *Sicily*, *&c.* And *here*, we find plainly, that the ſhock went along the river, both upwards and downwards, farther than by land; like the bottle of water held in the hand, in electrical experiments.

We argue the ſame from the ſickneſſes, pains in the joints and back, rheumatic, hyſteric, nervous caſes, head-ach, colics, and the like; which many people, eſpecially of the weaker conſtitutions, felt, for more than one day after: juſt as after electrification.

But from hence it is highly worthy of remark, that the finger of PROVIDENCE is notoriouſly diſcernible herein;

——— *of* HIM,
Who guides the Thunder, *and directs the* Storm.

Tho' it operates by natural cauſes, yet it is *that* which gives them their deſtination. For, though the coaſts of the ſea are moſt liable to, and ſuſceptible of, this mighty ſhock, which we call an earthquake; yet the chaſtening rod is directed to *towns* and *cities*, where are inhabitants, the objects of it's monition; not to *bare cliffs*, and an *uninhabited beach*. And there cannot be a more direct proof, that earthquakes are divine judgments, than *this* obſervation: for, in all antient hiſtory, earthquakes are ever found in great cities. *A. D.* 17, no leſs than 12 flouriſhing

cities

cities in *Aſia minor* were deſtroyed in one night. In *A. D.* 1456. at *Naples*, 40,000 people periſhed by an earthquake. In 1531. in the city of *Liſbon*, 1400 houſes were thrown down.

We ſee and admire the effects of Electricity, and its ſtupendous properties, every day; which ſeems as it were an animating ſoul to matter. The Antients had a notion that the earth was a great animal; probably from ſome obſervations of Electricity; but certainly, when in our days we feel theſe unuſual and extraordinary convulſions of nature, it is a leſſon to us, to do our duty toward that Great Being, who, by a drop of water can produce effects ſo prodigious.

2. Among all the appearances of nature, which are the ſubjects of the inquiries of the *Royal Society*, none would more ſenſibly affect the minds of contemplative perſons than that of an earthquake; eſpecially to us in this country, where they ſo ſeldom happen.

——*by the ſame.* Ibid. p. 657. *Read* April 5. 1750.

When I laid the preceding paper before the *Society*, I found that ſome worthy members had not fully entered into my way of reaſoning; nor with that ſeriouſneſs ſo awful a ſubject required: therefore I judged it neceſſary to treat upon it in a more diffuſive manner; and with ſome further conſiderations relating to that argument.

Among the numerous accounts received here, and obſervations upon the manner of it, I judged it became the *Society* to inquire into the cauſe of ſo extraordinary a motion, of which we could not have formed a proper idea, had we not repeatedly both ſeen and felt it.

The notions of the Antients are ſufficiently known; nor have the moderns any-way improved upon them, any further than by ſome chymical mixtures. The vulgar opinion goes no deeper than ſome caverns, not far below the ſurface of the earth; wherein are ingendered vapours, exploſions, fermentations, and fires from inflammable minerals, that cauſe theſe convulſions of the ſurface.

I ſhall not pretend to deny, that there may be ſuch vapours, fermentations, rarefactions, and inflammable ſubſtances, and actual fires, in the bowels of the earth, and that there may be ſome caverns underground, as well as we find ſome few above-ground, *Pool's Hole*, *Okey Hole*, and the like, in mountainous countries. We know there are hot ſprings running continually, and *vulcano's* frequently belching out flames and ſmoke; and to theſe moſt probably, ſome ſmaller earthquakes are owing.

But theſe matters are very rare, much rarer than earthquakes, both as to time and place. *Veſuvius* in *Italy*, and in that part of it abounding with mines of ſulphur; *Ætna* in *Sicily*; ſome on the great *Andes* mountains in *America*, and the like. The ſcarcity of them, in my opinion is ſo far from being a proof of the general cavernous ſtate of the earth, that it ſtrongly proves the contrary.

How many thouſand acres of coal-mines, ſtone-pits, and the like, do they daily work in *England*, and have done for ages? I have been myſelf 100 yards deep in a ſalt-rock. I have walked half a mile length-

wiſe, directly into the earth, and under the bed of the ocean: but we never hear, from the many hundreds of thouſands of workmen in this kind, of the cavernous ſtate of the earth.

On the contrary, by their hard labour they confeſs it far otherwiſe. Nor have we any reaſon, in *England*, to believe there are great mines of ſulphur, and inflammable minerals: nor, if there were, could they burn, and cauſe convulſions of the earth, unleſs there were proper cavities, and conveyances of air; as in coal-pits, when ſet on fire.

But even from theſe coal pits, when fired, do we ever find any thing like an earthquake produced? Nor did we obſerve, in theſe two laſt earthquakes, any fire, vapour, ſmoke, or ſmell; or any kind of eruption in the leaſt, in ſo great a ſtruggle of the ſuperfice, as affected a circle of 30 miles in diameter.

Indeed this conſideration alone, of the extent of this ſurface, is ſufficient to overthrow any ſuppoſition of earthquakes being *chiefly* owing to ſubterraneous vapours. For it cannot poſſibly be imagined, that ſuch can have ſo immenſe a force, as to act upon that compaſs inſtantaneouſly, all at once, and never break ground, ſo as to be diſcoverable to ſight or ſmell. Many accounts we have of a little fire-ball burſting in the air, at a great diſtance, and inſtantly propagating a ſulphureous ſmell all around it for miles.

If the motion of a ſuperficies of 30 miles diameter was owing to fumes and vapours, we ought reaſonably to find ſome great diſcharge of them, like a coal-pit fired: the operation of it ought to be hours and days in continuance, not inſtantaneous: and the evaporation of ſuch a quantity of inflammable matter requires a long time to evacuate itſelf.

There is another argument, which, in my opinion, utterly overthrows theſe ſuppoſitions; and that is, a due conſideration of ſprings. If we would form any tolerable idea of the nature of ſprings and fountains perpetually flowing, and *that* (generally ſpeaking) from the creation of the world, we muſt needs conceive, that GOD ALMIGHTY has laid their pipes and canals in the earth, like as he has planted the veins, arteries, and glands, in an animal body; and that likewiſe they are more and more ramified, as they nearer approach the outward ſhell of the earth.

The workmen in coal-mines, and the like, never fail to meet with the veins of ſprings every-where: they that dig for wells ſeldom fail of finding water every-where. The colliers are obliged to drain, at very great and continual expence. A circumſtance not very favourable to ſubterraneous fires.

Now it is apparent enough, that the hypotheſis of vapours, and ſubterraneous fermentations, exploſions, and eruptions, being the cauſe of earthquakes, muſt abſolutely ruin the whole ſyſtem of ſprings and fountains, where-ever it has once been. But this is quite contrary to fact; even

even where an earthquake has been repeatedly; for inſtance, from home.

On *Wedneſday*, *April* 6. 1580. about 6 in the evening, juſt ſuch another earthquake was felt in *London*, and around it, as theſe two we have ſeen. Another, exactly ſimilar, in 1692. In all theſe 4 no houſes thrown down, nor any ſprings diſturbed thereby: no ſenſible eruptions or ſmells.

Theſe conſiderations I apply only to this little inconſiderable ſpace of a circle of 30 miles diameter. But what is that to the appearance of ſome earthquakes we read of in hiſtory? In the year of our Lord 17. no leſs than 13 great and noble cities of *Aſia minor* were deſtroyed in one night. The fact is ſo notorious, that ſome perſons here preſent have ſeen a vaſt block of white marble, now ſtanding near *Naples*, being the pedeſtal of a coloſs ſtatue of *Tiberius* the emperor, having carved on it, in *baſſo relievo*, the *genius's* of all thoſe cities, with their names; which were rebuilt by that Emperor.

Without going ſo far, we may ſee another evidence of it, a coin of that Emperor ſtruck upon it, with this inſcription,

CIVITATIBUS ASIAE RESTITVTIS.

I have one of them in large braſs, which was found at *Colcheſter*.

The compaſs of this earthquake may be reckoned to take up 300 miles in diameter. How can we poſſibly conceive the action of any ſubterraneous vapours to produce ſuch an effect, as inſtantaneouſly to demoliſh all theſe cities? And that ſuch an accident ſhould never happen after? How comes it to paſs, that the whole country of *Aſia minor* was not at the ſame time deſtroyed, it's mountains renverſed, it's fountains and ſprings broken up, and ruined for ever, and it's rivers diſannulled? Inſtead whereof, we find nothing ſuffered, but thoſe cities; no kind of alteration in the ſurface of the country, which remains the ſame to this day.

From theſe conſiderations therefore, I cannot perſuade myſelf eaſily to enter into the opinion of earthquakes *generally* ariſing from pent-up vapours and eruptions. I know there are many ſtrange relations of effects of ſubterraneous fires, told by authors that deal pretty much in the marvellous, and whoſe minds were prepoſſeſſed with thoſe vulgar notions. My conceptions of the matter are derived from the more general appearances, and what we have ſeen and felt ourſelves.

After we have treated this argument in a ſuperficial view, we muſt go a little deeper. If we would conſider things like Philoſophers, let us propoſe to ourſelves this problem;

Where is the power to be placed, that is required to move a ſurface of earth 30 miles in diameter?

To anſwer this, conſult the engineers, and thoſe that make mines in the ſieges of towns. They will acquaint us, that the effect of mines is

produced in form of an inverted cone: and that a diameter of 30 miles in baſe, will require an axis of 15 or 20 miles to operate upon that baſe, ſo as to ſhake it, at leaſt. So that the vapours, and whatever power we propoſe to operate upon that baſe, according to the foregoing hypotheſis, in order to form the appearance of an earthquake, muſt be 15 or 20 miles deep in the earth.

But what mind can conceive, that any natural power is able to move an inverted cone of ſolid earth, whoſe baſe is 30 miles in diameter, and axis 20? or, was it poſſible, would not the whole texture of that body be quite diſturbed and ſhattered; eſpecially in regard to it's ſprings and fountains? but nothing like this is ever found to be the conſequence of an earthquake, tho' fatal to cities.

Apply this reaſoning to the earthquake of *Aſia minor*; and this vigorous principle muſt lie at leaſt 200 miles deep in the ground. Enough to ſhew the abſurdity of it. A cone of 300 miles diameter at baſe, and 200 miles axis; I dare be bold to ſay, that all the gunpowder made ſince it's invention, put together, would not be able to move it. How much leſs would pent-up vapours?

And, could it be admitted as a thing poſſible, will any one be perſuaded, that ſuch a ſubterraneous tumult, of ſo vaſt an extent, will be no-ways injurious to the internal ſyſtem of ſprings and fountains? We may as well imagine, that we may ſtab a man 100 times, and never touch a vein or artery.

In an age when Electricity has been ſo much our entertainment, and our amazement; when we are become ſo well acquainted with it's ſtupendous powers and properties, it's velocity, and inſtantaneous operation, through any given diſtances; when we ſee, upon a touch, or an approach, between an Electric and a Non-electric, what a wonderful vibration is produced, what a ſnap it gives, how a lambent flame breaks forth, how violent a ſhock; is it to be wondered at, that hither we turn our thoughts, for a ſolution of the prodigious appearance of an earthquake?

It is every body's obſervation, that there never was a winter like the paſt, for warmth and drineſs, thunder and lightning very uncommon then; for coruſcations in the air, juſtly thought to be electrical; eſpecially for that called *Aurora auſtralis*; the wind continually S. and S. W. and that without rain, which is unuſual. This ſtate of the atmoſphere had continued 5 or 6 months, before the firſt earthquake: is it not hence reaſonable to conclude, that the earth muſt, eſpecially in our region, be brought into an unuſual ſtate of Electricity; and, conſequently, wanted nought, but the approach of a non-electric body, to produce the ſnap, and the ſhock of Electricity?

That the earth was in that vibratory and electric ſtate, we have further reaſon to conclude, from the very extraordinary forwardneſs of all the vegetable world with us. Every one knows, that, at the end of *February*, all ſorts of garden-ſtuff, fruits, flowers, trees, were as forward

as,

as, in other years, in the middle of *April*. Conformable to which, experiments abundantly ſhew us, that electrifying of plants quickens their growth; for the ſame reaſon as in animals it quickens the pulſe.

Any ſolid matter is capable of being put into a ſtate of Electricity; ſuch as iron guns; and the more ſo, by reaſon of their ſolidity: and in proportion to it is the greatneſs of the ſnap, and of the ſhock; and a kind of lambent flame iſſues out of the point of contact; and likewiſe ſomewhat of the ſulphureous ſmell. So that if both flame and ſmell were diſcernible in an earthquake, it is to be found, without going to the bowels of the earth.

As to the immediate cauſe of this wonderful appearance of an earthquake, I hinted that it was owing to a non-electric body coming near or touching the earth, when in it's electrified ſtate; which may be a ſhower of rain: and the learned Dr *Childrey* obſerves, that earthquakes always ſucceed rain: a ſudden tempeſt of rain, in the time of a great drought.

At the ſame time that the force of Electricity in ſolids is as the quantity of matter, we ſee moſt evidently, that water is equally forcible in ſtrengthening and conducting it, and that in proportion to it's quantity: which very much juſtifies my obſervation, that moſt frequent earthquakes have fallen upon maritime places. And I find the ſame obſervation is made before me by *Acoſta* and *Dolittle*, who wrote on that in 1692. and others. In the dreadful cataſtrophe of *Port-royal*, it is notorious, that it's violence was chiefly near the ſea: and even in thoſe ſo lately felt by us, they were ſenſibly more violent toward the river, than further from it. And in that earthquake in *England*, in 1692. (which was very much like that we are treating of) there were no houſes thrown down, nor perſons killed; but it reached more particularly *Sandwich*, *Deal*, *Dover*, *Sheerneſs*, *Portſmouth*, and the maritime parts of *Holland*, *Flanders*, and *Normandy*.

In this that happened laſt *Sunday* at 6 in the evening at *Bath*, it was felt particularly at *Portſmouth*, the whole *Iſle of Wight*, and *Jerſey*.

If we look into antient hiſtory, we find, in the 197th year before Chriſt, an earthquake ſhook terribly the iſle of *Rhodes*, damaged many cities, and ſome were ſwallowed up.

17 years before Chriſt, many cities in the iſle of *Cyprus* were deſtroyed.

6 years before Chriſt, the iſle of *Coos* was moſt vehemently afflicted.

During the *Peloponneſian* war among the *Greeks*, the iſle of *Delos* was afflicted, and the moſt beautiful temple of *Apollo* thrown down.

Soon after, the city of *Lacedæmon* was totally deſtroyed.

A. D. 79. three cities in *Cyprus* were overthrown.

A. D. 182. the city of *Smyrna* was ruined.

Conſtantinople has often ſuffered; particularly in 1509. 13000 people overwhelmed.

A. D.

A. D. 1456. in the city of *Naples* 40000 people were deſtroyed.

In 1531. at *Liſbon*, 1400 houſes were thrown down, and near as many ſhattered.

In the time of *Valens* the Emperor, a terrible earthquake happened in *Crete*, whereby 100 cities were deſtroyed.

But inſtances enough, to ſhew what I aimed at, that maritime places are moſt ſubject: which is no contemptible argument in favour of Electricity; when both the ſolid of the earth, and the quantity of the water, concur to make the ſhock exactly, as in common electrical experiments.

The gardener in the *Temple* garden obſerved the ſound to roll from the water-ſide toward *Temple-Bar*, before the ceaſing of the nodding of the houſes; juſt as the electrical ſnap precedes the ſhock. Others, that write upon earthquakes, commonly obſerve, that the noiſe precedes the ſhock. But it is obvious it muſt be quite the contrary, did the concuſſion depend on a ſubterraneous eruption.

We may well enough expect, that burſting vapours, and ſubterraneous exploſions, ſhould diſperſe every thing that happened in their way into the air: but, in my apprehenſion, it is not poſſible for us to imagine any thing to produce ſuch a vibration as we felt, but Electricity.

Several people felt pains in their joints, rheumatiſm, ſickneſs, headach, pain in their back, colic, hyſteric and nervous diſorders, for the whole day after, and longer (eſpecially weak conſtitutions), exactly as upon Electrification; and to ſome it has proved fatal.

Upon this principle alone can we account for the fiſhes leaping out of the ponds; or a ſort of thump felt at the bottom of a boat. Nay, we are told of earthquakes felt at ſea, far diſtant from land: which are eaſily ſolved by an electrical ſhock impreſſed upon the water: but we cannot eaſily ſee vapours and fires reſiding at the bottom of the ocean.

From electrical vibration alone can we account for that obſervation of ſprings and fountains being no-ways damaged after an earthquake. I doubt not but they run more plentifully at that time; juſt as the blood circulates quicker upon Electrification.

From Electrification only we can account for this particular. The walls of *Weſtminſter-Hall* are of no mean thickneſs; yet thoſe that ſat with their backs to it, during the ſhock, all relate that it ſeemed to puſh toward them with great force. So in that of 1692. at *Deal*, the wall of the caſtle, which is of an extraordinary thickneſs and ſtrength, ſhook ſo much, that the people living in it expected it would have fallen on their heads.

For thus the force of the electrical ſhock is proportionate to the quantity of the ſolid. And were fumes and lambent flames ſeen to iſſue out of the gaping ground on theſe diſaſters, as relations tell us, we juſtly may pronounce them to be purely the effect of Electricity.

It muſt be accounted no inconſiderable argument in favour of our hypotheſis, that the northern regions of the world are little ſubject to earthquakes, in compariſon of the ſouthern; where the warmth and drineſs of the air, ſo neceſſary in Electricity, is common. Notwithſtanding that we have a vulcano in *Iceland*, yet we hear not of earthquakes frequent in that latitude of the globe.

But whether our conjectures upon this important affair be well founded or no, it certainly becomes a Chriſtian Philoſopher, whilſt he is inveſtigating material cauſes, to look up to the moral uſe of them; for, in reality, every thing in the whole world was ultimately made for that purpoſe. And of all the great and public calamities which affect us mortals, earthquakes claim the firſt title to the name of warnings and judgments; none ſo proper to threaten, or to execute vengeance: nor has any other thoſe annexed terrors, ſo much of the unuſual, the unavoidable, and the horrible apprehenſion of being cruſhed to death, or buried alive.

I cannot but inſiſt upon my former obſervation to be juſt, that earthquakes proclaim themſelves to mankind in this light; becauſe peculiarly directed to great cities and maritime; abounding with wealth and luxury. It would be childiſh to make a long recital of particulars from hiſtory; for had we no other ſort of notices of earthquakes? look upon thoſe two ſhocks we have felt. We own, that *Hampſtead-Heath* and *Finchley-Common*, and *Kennington-Common*, may have been affected with it; yet it is notorious, that *London* was the centre; the place to which the finger of GOD was pointed.

3. As the late earthquakes in *London*, and ſome other parts of *England*, have rouſed the attention of mankind, to conſider the cauſes of them, both in a religious and natural view: and as in a religious view they have been conſidered by the Biſhop of *London*, in his excellent letter to the clergy and people of *London*, which has been received with general approbation: ſo I ſhall here give a ſhort account of what ſeems to me to be a probable natural cauſe of them.

——*by the Rev.* Stephen Hales, *D. D. & F. R. S.* Ibid. p. 669. *Read* April 5. 1750.

But I muſt firſt obviate an objection of ſome ſerious well-meaning people, who are apt to be offended at any attempts to give a natural account of earthquakes; which, but rarely happening in theſe more northern parts, are apt to be looked upon as the more miraculous. But it ought to be conſidered, that the ordinary courſe of nature is as much carried on by the divine agency, as the extraordinary and miraculous events. God ſometimes changes the order of nature, with deſign to chaſtiſe man for his diſobedience and follies; natural evils being graciouſly deſigned by him as moral goods: all events are under his direction, and fulfil his will.

On the other hand, there are ſome who make light of earthquakes, becauſe they are capable of being acounted for by natural cauſes. But the hand of God is not to be overlooked in theſe things, under whoſe government all natural agents act; eſpecially ſuch rare and unuſual

events

events as earthquakes. God uſes all creatures to be the inſtruments of his will: natural and moral agents are all under his direction. When he inflicts a famine on a nation, it is not the leſs the hand of God, becauſe we know the natural cauſes of it, *viz.* great drought, and unkindly ſeaſons: *fire and hail, ſnow and vapour, and ſtormy wind, fulfil his word*, Pſal. cxlviii. 8. Infectious air, peſtilential diſeaſes, and earthquakes, however occaſioned by natural cauſes, are under the divine influence. He not only orders and directs the operations of nature, but alſo influences the actions of moral agents, turning, as he pleaſes, the hearts of the governors of the nations, ſo as frequently to chaſtize mankind by that ſevere ſcourge, and great diſgrace of human nature, war. Earthquakes are not therefore ſlightly to be regarded, becauſe we think we can give a probable natural account of them; neither ought we, on that account, to encourage ourſelves to go careleſly on in wicked courſes. If national judgments do not overtake us, yet it cannot be long before we ſhall come into the puniſhment of our future ſtate: *And tho' ſentence againſt an evil work is not ſpeedily executed tho' a ſinner do evil an hundred times, and his days be prolonged; yet ſurely I know it ſhall not be well with the wicked.* Eccleſ. viii. 11, 12, 13.

It may not be improper, on this occaſion, to mention another conſtant and uninterrupted plague, in which of late years, we have been, and are like to continue ſufferers, in common with many other nations. A plague, of all others the greateſt that ever befel unhappy man; it being by far the moſt deſtructive, not only of the lives, but alſo of the morals, of mankind; both a natural and a moral evil; I mean fermented diſtilled ſpirituous liquors of all denominations. Did God Almighty deſtroy as many by earthquakes as are yearly deſtroyed by diſtilled ſpirituous liquors, which is probably about a million of perſons in a year all over the world; how great a terror and conſternation would it cauſe every where! But, alas! with what unconcernedneſs, with what calmneſs, and even complacency, is this enormous both natural and moral evil received, and even foſtered, among us; inſomuch that it is now become, by a juſt judgment, the curſe and the puniſhment of the world, even the greateſt that ever befel unhappy man! notwithſtanding which this inchanting *Siren* ſo bewitches and infatuates the nations, that it ſpreads it's baleful influence far and wide, making yearly farther and farther devaſtations, both of the lives and morals of mankind, and even debaſing the breed of man.

As to the affair of earthquakes, particularly that which happened at *London March* 8 laſt, about 20′ before 6 in the morning; I being then awake in bed, on a ground-floor, near the church of *St Martin's in the Fields*, very ſenſibly felt the bed heave, and conſequently the earth muſt heave too. There was a hollow, obſcure, ruſhing noiſe in the houſe, which ended in a loud exploſion up in the air, like that of a ſmall cannon: the whole duration, from the beginning to the end of the earthquake, ſeemed to be about 4″. The ſoldiers who were upon duty

duty in *St James's Park*, and others who were then up, ſaw a blackiſh cloud, with conſiderable lightning, juſt before the earthquake began; it was alſo very calm weather.

In the hiſtory of earthquakes it is obſerved, that they generally begin in calm weather, with a black cloud. And when the air is clear, juſt before an earthquake, yet there are then often ſigns of plenty of inflammable ſulphureous matter in the air; ſuch as *Ignes Fatui* or *Jack-a-Lanterns*, and the meteors called falling ſtars.

Now, I have ſhewn many years ſince, in the Appendix to my *Statical Eſſays*, Exp. 3. Page 280. the effect that the mixture of a pure and ſulphureous air have on each other; *viz.* by turning the mouth downwards, into a pan of water, of a glaſs veſſel of a capacity ſufficient to hold about two quarts, with a neck about twenty inches long, and two inches wide. Then, by putting under it, in a proper glaſs veſſel, with a long narrow neck, a mixture of *aqua fortis*, and powdered pyrites, *viz* the ſtone with which vitriol is made, there will be a briſk ferment, which will fill the glaſs with rediſh ſulphureous fumes; which, by generating more air than they deſtroy, will cauſe the water, with which the whole neck of the glaſs veſſel was filled, to ſubſide conſiderably. When the rediſh ſulphureous air in the upper part of the glaſs is clear, by ſtanding 2 or 3 hours, if then the mouth of the inverted glaſs is lifted out of the water, ſo as to let the water in the neck of the glaſs fall out; which, ſuppoſing it to be a pint, then an equal quantity of freſh air will ruſh in at the mouth of the neck of the veſſel, which muſt immediately be immerſed in the water: and upon the mixture of the freſh air with the then clear ſulphureous air, there will inſtantly ariſe a violent agitation between the two airs, and they will become, from tranſparent and clear, a rediſh turbid fume, of the colour of thoſe vapours which were ſeen ſeveral evenings before the late earthquakes: during which efferveſcence, a quantity of air, nearly equal to what freſh air was let in, will be deſtroyed; which is evident by the riſing up of the water in the neck of the glaſs, almoſt as high as before. And if, after the efferveſcence of the mixed airs is over, and become clear again, freſh air be admitted, as before, they will again grow rediſh and turbid, and deſtroy the new admitted air as before; and this after ſeveral repeated admiſſions of freſh air: but after every readmiſſion of freſh air the quantity deſtroyed will be leſs and leſs, till no more will be deſtroyed. And it is the ſame after ſtanding ſeveral weeks, provided, in the mean time, too much freſh air had not been admitted. Now, I found the ſum total of the freſh air thus deſtroyed to be nearly equal to the firſt quantity of ſulphureous air in the inverted glaſs.

Since we have in this experiment a full proof of the briſk agitation and efferveſcence which ariſes from the mixture of freſh air with air that is impregnated with ſulphureous vapours, which ariſe from ſeveral mineral ſubſtances, eſpecially from the *pyrites*, which abounds in many parts of the earth; may we not with good reaſon conclude, that the

irkſome heat, which we feel in what is called a cloſe ſultry temperature of the air, is occaſioned by the inteſtine motion between the air and the ſulphureous vapours, which are exhaled from the earth? which effervescence ceaſes, as ſoon as the vapours are equably and uniformly mixed in the air; as happens alſo in the effervescences and ferments of other liquors. The common obſervation therefore, that lightning cools the air, ſeems to be founded on good reaſon; that being the utmoſt and laſt effort of this effervescence.

May we not hence alſo, with good probability, conclude, that the firſt kindling of lightning is effected by the ſudden mixture of the pure ſerene air above the clouds, with the ſulphureous vapours, which are ſometimes raiſed in plenty, immediately below the clouds? The moſt dreadful thunders being uſually when the air is very black with clouds; it rarely thundering without clouds: clouds ſerving, in this caſe, like the above-mentioned inverted glaſſes, as a partition between the pure and ſulphureous airs: which muſt therefore, upon their ſudden admixture through the interſtices of the clouds, make (like the two airs in the glaſs) a more violent effervescence, than if thoſe airs had, without the intervention of the clouds, more gradually intermixed, by the conſtant more gradual aſcent of the warmer ſulphureous vapours from the earth, and deſcent of the cold ſerene air from above. And tho' there was no luminous flaſh of light in the glaſs, yet, when ſuch ſudden effervescence ariſes, among a vaſt quantity of ſuch vapours in the open expanſe of air, it may, not improbably, acquire ſo rapid a velocity, as to kindle the ſulphureous vapours, and thereby become luminous.

And ſince, from the effects that lightning is obſerved to have on the lungs of animals, which it often kills, by deſtroying the air's elaſticity in them, as alſo from it's burſting windows outwards, by deſtroying the air's elaſticity on the outſide of thoſe windows: Since, I ſay, it is hence probable, that the ſulphureous fumes do deſtroy a great quantity of elaſtic air; it ſhould therefore cauſe great commotions and concuſſions in the air, when the air ruſhes into thoſe evacuated places; which it muſt neceſſarily do with great velocity.

Dr *Papin* has calculated the velocity with which air ruſhes into an exhauſted receiver, when driven by the whole preſſure of the atmoſphere, to be at the rate of 1305 feet in a ſecond of time; which is at the rate of 889 miles in an hour: which is near 18 times a greater velocity than that of the ſtrongeſt ſtorms; which is eſtimated to be at the rate of 50 miles in an hour *.

Hence, we ſee that an outrageous hurricane may be cauſed, by deſtroying a ſmall proportion of the elaſticity of the air of any place, in reſpect to the whole. No wonder then that ſuch violent commotions of the air ſhould produce hurricanes and thunder-ſhowers; eſpecially in the warmer climates; where both the ſulphureous and watry vapours, being raiſed much higher, and in greater plenty, cauſe more violent effects. M. *de*

* See Vol. I. p. 586.

M *de Buffon*, in his Natural Hiſtory and Theory of the Earth, mentions black dark clouds in the air near the tempeſtuous *Cape of Good Hope*, and alſo in the ocean of *Guiney*, which are called by the ſailors the *Ox's Eye*; which are often the forerunners of terrible ſtorms and hurricanes. Whence it is to be ſuſpected, that they are large collections of ſulphureous vapours; which, by deſtroying ſuddenly a great quantity of the elaſtic air, cauſe the ambient air to ruſh with great violence into that vacuity, thereby producing tempeſts and hurricanes. And off the coaſt of *Guiney* they have ſometimes 3 or 4 of theſe hurricanes in a day; the forerunners of which are theſe black ſulphureous clouds, with a ſerene clear air, and calm ſea; which on a ſudden turns tempeſtuous, on the exploſion of theſe ſulphureous clouds. And in *Jamaica* they never have an earthquake when there is a wind to diſperſe the ſulphureous vapours.

In the like manner we find, in the late earthquakes at *London*, and in the accounts of many other earthquakes, that, before they happen, there is uſually a calm air, with a black ſulphureous cloud: which cloud would probably be diſperſed like a fog, were there a wind: which diſperſion would prevent the earthquake; which is probably cauſed by the exploſive lightning of this ſulphureous cloud; being both nearer the earth than common lightnings; and alſo at a time when ſulphureous vapours are riſing from the earth in greater quantity than uſual; which is often occaſioned by a long ſeries of hot and dry weather. In which combined circumſtances, the aſcending ſulphureous vapours in the earth may probably take fire, and thereby cauſe an earth-lightning; which is at firſt kindled at the ſurface, and not at great depths, as has been thought: and the exploſion of this lightning is the immediate cauſe of an earthquake.

It is in the like manner that thoſe meteors, which are called falling ſtars, are ſuppoſed to be kindled into a flame at the upper part of a ſulphureous train, which is kindled downwards into a flame, in the ſame manner as a freſh-blown out candle is inſtantly lighted from another candle held over it at a diſtance, in the ſulphureous inflammable ſmoke of it.

I am ſenſible that it may ſeem improbable, that the aſcending ſulphureous vapours in the earth ſhould thus be kindled; but, ſince they are continually aſcending through the pores of the earth, more or leſs, for many good and uſeful purpoſes, it is plain there is room for them to paſs. Beſides, as M. *de Buffon* remarks, Naturaliſts have obſerved perpendicular and oblique clefts, in all kinds of layers of earth, not only among rocks, but alſo among all kinds of earth, that have not been removed, as is obſervable wherever the earth is open to any depth. Now theſe clefts are cauſed by the drying of the ſeveral horizontal layers of the earth; and will alſo be conſiderably the wider in long dry hot ſeaſons, which are uſually the preparatory forerunners of earthquakes,

and the exploſion of the ſulphureous vapours may probably widen them more.

It is very obſervable, in the opinion of *Borelli*, and other Naturaliſts, that *volcano's* begin firſt to kindle near the ſurface or top of the mountains, and not in the caverns in the lower parts of the mountains. M. *de Buffon* ſays, that earthquakes are moſt frequent where there are *volcano's*; ſulphureous matter abounding moſt there: but that, tho' they continue burning long, yet they are not very extenſive. But that the other ſort of earthquakes, which are not cauſed by a *volcano*, extend often to a great diſtance. Theſe are much longer E. and W. than broad N. and S; and ſhake a zone of earth with different degrees of force in different parts of their courſe; *viz.* in proportion to the different quantities of exploſive ſulphureous matter in different places. Theſe kind of earthquakes are obſerved to be progreſſive, and to take time to extend to the great diſtances ſometimes of ſome thouſands of miles. They are an inſtantaneous exploſion in every place, near the ſurface of the earth; and therefore do not produce mountains and iſlands, as *volcano's* ſometimes do.

The earthquake in *London*, *March* 8. was thought to move from E. to W. M. *Buffon* mentions an earthquake at *Smyrna*, in 1688. which moved from W. to E. *viz.* becauſe the firſt kindling probably began on the weſtern ſide; and in the earthquake at *London* on the eaſtern ſide. And accordingly it was obſerved, that the rediſh bows in the air, which appeared ſeveral days before that earthquake, aroſe in the eaſt, and proceeded weſtward. It was obſerved, after the earthquake at *Smyrna*, that the caſtle-walls, which run from E. to W. were thrown down; but thoſe from N. to S. ſtood; and that the houſes on rocks ſtood better than thoſe on the earth.

M. *de Buffon* relates, that the vibrations of the earth, in earthquakes, have commonly been from N. to S.; as appears by the motion of the lamps in churches: which makes it probable, that, tho' the progreſs of the earthquake at *Smyrna* was from W. to E. yet the vibrations of the earth might be from N. to S; and thereby occaſion the falling of the caſtle-walls, which run from E. to W. but not thoſe which run from N. to S. A probable argument, that, as the freeſt paſſage, ſo the greateſt exploſions were made in the clefts of the earth which run E. and W. which would make the vibrations N. and S.

It was obſerved, that the waters turned foul the day before an earthquake at *Bologna* in *Italy*: and I was informed, that the water of ſome wells in *London* turned foul at the time of the earthquakes. Which was probably occaſioned by the aſcent of great plenty of ſulphureous vapours through the earth.

As to the hollow rumbling noiſe, which is uſually heard in earthquakes, it ſeems not improbable, that it may be occaſioned by the great agitation that the electrical æthereal fluid is put into by ſo great a ſhock of a large maſs of earth. For, if the like motion of a ſmall revolving glaſs globe

globe can excite it to the velocity of lightning, and that with a force ſufficient to kill animals, how much greater agitation may it probably be excited to, by the exploſive force of an earthquake!

The exploſion of a cannon in *St James's Park* is obſerved to electrify the glaſs of the windows of the *Treaſury* And what makes it ſtill more probable, is, the analogy that there is between them in other reſpects. For, as the electrical flaſh ruſhes, with the velocity of lightning, along the moſt ſolid bodies, as iron, *&c.* and as I have ſeen it run only on the irregular gilding of leather; ſo ſuch ſolid bodies are obſerved to be the conductors of aëreal lightning, which rends oaks in pieces, and has been known to run along and melt an iron bell-wire on two ſides of a room, *&c.* And accordingly it was obſerved, in the great earthquake in *Jamaica*, that the moſt tremendous roaring was in the rocky mountains. And in the late earthquake of *March* 8. in *London*, the loudeſt exploſions were thought to be heard near ſuch large ſtone buildings as churches, with lofty ſteeples and ſpires.

I, who lay in *Duke's Court*, near *St Martin*'s church, and was awake all the time of the earthquake, plainly heard a loud exploſion up in the air, like that of a ſmall cannon: which made me conjecture, that the noiſe was owing to the ruſhing off, and ſudden expanſion, of the electrical fluid, at the top of *St Martin*'s ſpire; where all the electrical *effluvia*, which aſcended up along the larger body of the tower, being by attraction ſtrongly condenſed, and accelerated at the point of the weather-cock, as they ruſhed off, made ſo much the louder expanſive exploſion.

XLII. Since I had the honour to lay before the ſociety, in the ſpring, my thoughts upon earthquakes, we have had many opportunities of reflecting on that moſt awful, and hitherto unuſual, appearance. The year 1750. may rather be called the year of earthquakes, than of *Jubilee*. For, ſince they began with us at *London*, as far as I can learn, they have appeared in many parts of *Europe*, *Aſia*, *Africa*, and *America*, and have likewiſe reviſited many counties in our iſland: at length, on 30th of laſt *Sept.* taken their leave (as we hope) with much the moſt extenſive ſhock we have ſeen in our days.

The Philoſophy *of* Earthquakes; *by the Rev.* Will. Stukeley, *M. D. F. R. S.* &c. *in a letter to* M. Folkes, *Eſq*; *LL. D.* Pref. R. Soc. *&c.* Ibid. p. 731. *Read* Dec. 6. 1750.

It may well be expected, that theſe frequent viſits, in themſelves ſo very extraordinary, to us ſo rare, and that in one year, ſhould keep up our attention; and, as to my own part, induce one to reflect on what I before offered concerning them, and be a ſufficient apology for the preſent paper.

We have been acquainted, by thoſe that remember it, that in the earthquake of *November* 1703. which happened in *Lincolnſhire*, the weather was calm, cloſe, gloomy, warm, and dry, in a degree highly unuſual at that ſeaſon: and thus it has been with us all the year: and from the numerous accounts we have received at the *Royal Society*, in the beginning and end of the year, where any mention is made of the weather,

weather, they agree in the like particular: which is conſentaneous to what I remarked as the conſtant forerunner of earthquakes, and what prepares the earth's ſurface to receive the electrical ſtroke.

In my laſt we had a paper read at the *Royal Society*, concerning the firſt earthquake felt by us at *London*, *Feb.* 8. A ſhepherd belonging to Mr Secretary *Fox* at *Kenſington*, the ſky being perfectly ſerene and clear, was much ſurprized with a very extraordinary noiſe in the air, rolling over his head, as of cannon cloſe by: he likewiſe thought that it came from the N. W. and went to the S. E. a motion quite contrary to what muſt have been the caſe, if it were really of cannon. This noiſe paſſed ruſhing by him; and inſtantly he ſaw the ground, a dry and ſolid ſpot, wave under him, like the face of the river. The tall trees of the avenue, where he was, nodded their tops very ſenſibly, and quavered. The flock of ſheep immediately took fright, and ran away all together, as if the dogs had purſued them. A great rookery in the place were equally alarmed; and, after an univerſal clangor, flew away, as if chaſed by hawks.

I was likewiſe informed, that, in the ſame earthquake, a great parcel of hens and chickens, kept at that time in *Gray's-Inn Lane*, upon the ſhock, ran to the rooſt affrighted: and the like was obſerved of pigeons. And in our account of the laſt earthquake from *Northampton*, it is remarked, that the birds in cages put their heads under their wings, as to hide themſelves.

June 21. at the *R. Soc.* Mr *Jackſon*, Potter at *Lambeth*, gave an account of ſome boats and loiters, in the river at that time; the people in them ſeemed to feel as if a porpoiſe, or ſome great fiſh, had heaved and thumped at the bottom of the loiters. This is ſometimes the caſe of ſhips at ſea; which ſeems evidently owing to an electrical impreſſion on the water.

In the *Evening-Poſt*, *June* 23. we had a paragraph from *Venice*, that a terrible earthquake had lately been felt in the iſle of *Cerigo*; a little rocky iſle. It threw down a great number of houſes, and above 2000 inhabitants were buried in the ruins.

Another earthquake about that time happened in *Switzerland*, which ſplit a vaſt rocky mountain, and an old caſtle-wall, of an immenſe thickneſs.

But, ſince then, theſe wonderful movements have ſtalked round the globe; and again been lately felt in our own iſland, to the terror only of many thouſand people; beſides thoſe that appeared in the weſtern parts, in the more early time of the year.

I received a letter from *Maurice Johnſon*, Eſq; the founder and ſecretary of the *Literary Society* of *Spalding*, which has now ſubſiſted theſe 40 years. He acquaints me, that, on *Thurſday*, *Aug.* 23. laſt, an earthquake was very ſenſibly felt there, about 7 in the morning, throughout the whole town and neighbourhood, and many miles round; but chiefly ſpread northward and ſouthward. He ſays, that, for a fort-

night

night before, the weather had been ſerene, mild, and calm; and one evening there was a deep-red *Aurora auſtralis*, covering the cope of Heaven, very terrible to behold. This ſame ſhock was felt at *Grantham*, *Stamford*, and *Milton* by *Peterborough*; and generally at all the intermediate places.

Since then, I had a letter from Mr Alderman *Taylor*, of *Stamford*, giving an account of another earthquake, that happened there on *Sunday*, *Sept.* 30. at 36′ after noon. He deſcribes it thus: "They were "ſuddenly ſurprized with an uncommon noiſe in the air, like the roll- "ing of large carriages in the ſtreet, for about 20 ſeconds. At the "ſame inſtant they felt a great ſhake, or ſnap (as he calls it); inſo- "much that it ſenſibly ſhook a punch-bowl, which was in his parlour, "and made it ring. He ſays, it was perceived of moſt of the people "of *Stamford*, who generally ran out of their houſes. At *Okeham*, the "chief town in *Rutland*, the congregation ran out of the church. All "the towns round *Stamford* were ſenſible of it, and at *Peterborough*, "down to *Wiſbich*."

Thus far the Alderman. But we have had many advices from all hands, at the firſt and ſecond meetings of the *R. S.* for the winter-ſeaſon; with further particulars relating to this great concuſſion: that it was felt at the ſame time at *Rugby* in *Warwickſhire*, and reached to *Warwick*; at *Lutterworth* in *Leiceſterſhire*; at *Leiceſter*, and round about. They deſcribe it, that the houſes tottered, and ſeemed to heave up and down, tho' it laſted but a few ſeconds. It was attended with a ruſhing noiſe, as if the houſes were falling; and people were univerſally ſo affrighted as to run out; imagining that their own, or neighbours houſes, were tumbling on their heads. In the villages around, the people, being at divine ſervice, were much alarmed, both with the noiſe, which exceeded all the thunder they had ever heard, beyond compare; and with the great ſhock accompanying, which was like ſomewhat that ruſhed againſt the church-walls and roof; ſome thinking the pillars cracked; many, that the beams of the roof were disjointed; and all, that the whole was falling; and happy were they that could get out firſt. A few ſlates, tiles, and parts of chimnies, fell from ſome houſes; pewter, glaſſes, and braſs, fell from ſhelves; a clock-bell ſometimes ſtruck; windows univerſally rattled; and the like circumſtances of tremor.

The ſame extended itſelf to *Coventry*, *Derby*, *Nottingham*, *Newark*; then came eaſtward to *Harborough*, *Towceſter*, *Northampton*, *Rowell*, *Kettering*, *Wellingborough*, *Oundle* in *Northamptonſhire*, *Uppingham*, *Okeham* in *Rutland*, *Stamford*, *Bourn*, *Grantham*, *Spalding*, *Boſton*, and to *Lincoln*, in *Lincolnſhire*; *Holbech*, and all *Holland*, in that county; *Peterborough*, *Wiſbich* in the iſle of *Ely*, together with all the intermediate and adjacent places. Then it paſſed over the whole breadth of *Ely-Fen*, and reached to *Bury* in *Suffolk*, and the country thereabouts; of which we had notice from Lady *Cornwallis*: an extent from *Warwick* to *Bury*

of

of about 100 miles in length; and, generally ſpeaking, 40 miles in breadth. And this vaſt ſpace was pervaded by this amazing motion, as far as we can get any ſatisfaction, in the ſame inſtant of time.

In regard to circumſtances, they were pretty ſimilar throughout. At *Northampton*, a Gentlewoman, ſitting in her chair, relates, that ſhe and her chair were twice ſenſibly lifted up, and ſet down again. A ſtack of chimnies were thrown down in *College-Lane*; a place retaining the memory of a ſort of univerſity once beginning at *Northampton*. The windows of houſes rattled throughout the whole town; but no miſchief done: in general, frightful, and innocuous.

They fanſied there the motion of it, as they expreſs it, to be eaſtward. In the ſtreets that run N. and S. the houſes on the E. ſide of the way were moſt affected: and Dr *Stonehouſe*'s dwelling, the ſtrongeſt in the town, was moſt ſenſibly ſhaken. So it was likewiſe obſerved, that churches were moſt ſubject to it's violence. They thought too that the motion ſeemed rather horizontal, or lateral, than upward. Some counted the pulſes diſtinctly, to the number of four: that the ſecond and third pulſe were ſtronger than the firſt and fourth.

From all theſe various accounts, there was no ſulphureous ſmell, or eruption; no fiſſures in the ground perceived: yet ſeveral people were ſick upon it; infinite numbers terribly affrighted; and as ſoon forgot the impreſſion of it, or talked of it in a merry ſtrain, as commonly with us at *London*. So little are the vulgar affected, without ſomething very ſenſible, and ſo ſoon is the ſenſe of it worn out!

It was more evidently perceived by people ſtanding; moſt, by thoſe that were ſitting; leaſt, by ſuch as were walking; and in upper ſtories of houſes more than in lower, or in cellars. Some, coming down ſtairs, were in danger of being thrown forwards: ſeveral ſitting in chairs, and hearing the hollow thundering noiſe, and thinking it was a coach paſſing by, when they attempted to get up, to ſee what it was, they were thrown back again into their chair. Some heard the wainſcot crackle. A lady, ſitting by the fire, with her chair leaning forwards, was thrown down on her hands and knees.

It was particularly remarked (as before obſerved), that birds in cages were ſenſibly affrighted, thruſting their heads under their wings. Mrs *Allicock*, of *Loddington*, a Lady in childbed, was ſo affected, that it cauſed her death. Some people felt ſuch a ſudden ſhortneſs of breath, that they were forced to go out into the open air, it ſo affected the pulmonary nerves. Many were taken with head-achs.

Theſe are, in general, the obſervations made at the time of theſe earthquakes; when we recollected ourſelves, after the ſuddenneſs and affright. Give me leave to make the following remarks.

1. As far as we can poſſibly learn, where no one can be prepared at different places, by time-keepers, this mighty concuſſion was felt preciſely at the ſame inſtant of time, being about half an hour after 12 at noon. This, I preſume, cannot be accounted for by any natural power, but

but that of an electrical vibration; which, we know, acts inſtantaneouſly.

2. Let us reflect on the vaſt extent of this trembling, 100 miles in length, 40 in breadth, which amounts to 4000 ſquare miles in ſurface. That this ſhould be put into ſuch an agitation in one moment, is ſuch a prodigy, as we ſhould never believe, or conceive, did we not know it to be fact, from our own ſenſes. But, if we ſeek for a ſolution of it, we cannot think any natural power is equal to it, but that of Electricity; which acknowledges no ſenſible tranſition of time, no bounds.

3. We obſerve, the vulgar ſolution of ſubterraneous eruptions receives no countenance from all that was ſeen or felt during theſe earthquakes: it would be very hard to imagine how any ſuch thing could ſo ſuddenly and inſtantaneouſly operate thro' this vaſt ſpace, and that in ſo ſimilar and tender a manner, over the whole, thro' ſo great a variety as well as extent of country, as to do no miſchief.

A philoſophical inquirer in *Northamptonſhire*, and who had his eye particularly on this point, takes notice there were not any fiſſures in the ground, any ſulphureous ſmells, or eruptions, any-where perceived, ſo as to favour internal convulſions of the earth; yet we learn, from a letter, at *Uppingham* in *Rutland*, that a plaſter floor became cracked thereby. Theſe kind of floors are frequent in this country; what we call *Stucco* in *London*; and it gives us a good notion of the undulatory vibration produced by an earthquake; which ſome have compared to that of a muſical ſtring; others, to that of a dog, or a horſe, ſhaking themſelves when they come out of the water.

4. The former earthquake, that happened at *Grantham*, *Spalding*, *Stamford* (which towns lie in a triangle) took up a ſpace which may in groſs be accounted a circle of 20 miles in diameter; the centre of which is that great moraſs called *Deeping-Fen*. This comprehends 14 miles of that 20 in diameter; and where, probably, the electrical impreſſion was firſt made. Much the major part of *Deeping-Fen* is under water in the winter; underneath is a perfect bog: now it is very obvious how little favourable ſuch ground is for ſubterraneous fires.

In the ſecond earthquake, not only this country was affected again, but likewiſe a much larger ſpace of the ſame ſort of fenny ground, rather worſe than the former: all *Donington-Fen*, *Deeping-Fen*, *Croyland-Fen*, *Thorney-Fen*, *Whitleſea Fen*, *Bedford-Level*, and the whole extent of *Ely-Fen*, under various denominations. This country, under the turf, abounds with ſubterraneous timber of all kinds; fir, oak, and bruſh-wood; ſtags horns: now-and-then they find a quantity of hazel-nuts, crouded together on a heap: I have ſome of them. This is a matter common to all boggy ground over the whole globe. They are the ruins of the antediluvian world, waſhed down from the high country, where they grew, here lodged, and by time overgrown with the preſent turf. They that ſeek for any other ſolution of this affair, than the univerſal *Noachian* deluge, want to account for a general effect by

a partial cauſe; and ſhut their eyes, both to the plain hiſtory of this matter, and to the infinite notorious demonſtrations of it from foſſil appearances.

5. All this country, tho' underneath it is a watry bog, yet, through this whole ſummer, and autumnal ſeaſon (as they can have no natural ſprings in ſuch a level) the drought has been ſo great on the ſuperficies, that the inhabitants were obliged every day to drive their cattle ſeveral miles, for watering. This ſhews how fit the dry ſurface was for an electrical vibration; and we learn from hence this important particular, that it reaches but very little below the earth's ſurface.

Mr *Johnſon*, in another letter which he wrote to me concerning the ſecond earthquake, obſerved at *Spalding*, ſays, upon this occaſion, he was obliged to ſcour his canal, and deepen it; that they came to a white quickſand, which afforded to all the neighbourhood excellent water in plenty.

In the gravelly ſoil of *London*, and where the two ſhocks were felt by us, in the beginning of the year, we know there is not an houſe in the whole extent of this vaſt city, and all around it, but a ſpring of water is ready, upon digging a well: whence we have much reaſon to believe, that the internal parts of the earth are like a ſponge ſoaked in water; ſo that the only dry part of it is the ſuperficies; which is the object, and the ſubject, of that electric vibration, wherein (according to my ſentiments) an earthquake conſiſts.

This ſhews the miſtake of the Antients; who, fancying that earthquakes proceeded from ſubterraneous eruptions, built their prodigious temple of *Diana* of *Epheſus* upon a boggy ground, to prevent ſuch a diſaſter.

6. Earthquakes are truly moſt violent in a rocky country; becauſe the ſhock is proportionate to the ſolidity of the matter electrified: ſo that rocks, old caſtle-walls, and ſtrong buildings, are moſt obnoxious to the concuſſion. The iſle of *Cerigo* was more liable, and more rudely handled by the late earthquake; both becauſe it was an iſle, and becauſe it was rocky. So we muſt ſay of the late earthquake in *Switzerland*, that ſplit the mountain and the old caſtle-wall. Whence Mr *Johnſon*, in his ſecond letter, ſays, it cracked a very ſtrong brick houſe in *Goſberton* by *Spalding*. Dr *Doddridge* obſerves, from *Northampton*, that Dr *Stonehouſe*'s dwelling, being a very ſtrong one, was moſt ſenſibly ſhaken. And, throughout the whole extent of this great earthquake, we find both the noiſe, the ſhock, and the terror, was greateſt at the churches, whoſe walls and bulk made more reſiſtance than houſes: and, generally ſpeaking, the churches throughout this whole extent have very fair and large towers, and very many remarkable ſpires of good ſtone.

This ſame vibration, impreſſed on the water, meeting with the ſolid of the bottom of ſhips and loiters, gives that thump felt thereon. Yet, of the millions of ordinary houſes, over which it paſſed, not one fell: a con-

a conſideration which ſufficiently points out to us what ſort of a motion this was not; what ſort of a motion it was; and whence derived: not a convulſion of the bowels of the earth, but an uniform vibration of it's ſurface, aptly thought like that of muſical ſtring; or what we put a drinking-glaſs into, by rubbing one's finger over the edge; which yet, brought to a certain pitch, breaks the glaſs; undoubtedly an electric repulſion of parts.

7. We find, from all accounts antient and modern, that the weather preceding theſe ſhocks was mild, warm, dry, ſerene, clear, froſty: what notoriouſly favours all our electrical experiments. We very well know, that, generally, all laſt winter, ſpring, ſummer, and autumn, have been remarkably of this kind of weather; more ſo than has been obſerved in our memory; and have had all thoſe requiſites, appearances, and preparations, that notoriouſly cauſe Electricity, that promote it, or that are the effects of it.

8. We find the blood-red *auſtralis aurora* preceding at *Spalding*, as with us at *London*. This year has been more remarkable than any for fire-balls, thunder, lightning, and coruſcations, almoſt throughout all *England*. Fire-balls more than one were ſeen in *Rutland* and *Lincolnſhire*, and particularly obſerved. All theſe kinds of meteors are rightly judged to proceed from a ſtate of Electricity in the earth and atmoſphere.

9. Mr *Johnſon*, in both his letters to me on the firſt and ſecond earthquakes at *Spalding*, remarks particularly of their effects being moſtly ſpread to the N. and S. and eſpecially felt on the ſea-coſt. We may obſerve that ſuch is the direction of *Spalding* river, which both conducts and ſtrengthens the electric vibration; conveying it along the ſea-ſhore, thence up *Boſton* chanel, and ſo up *Boſton* River to *Lincoln*; as we diſcern, by caſting our eye upon a map.

We obſerve further, that the main of this ſecond earthquake diſplayed it's effects along and between the two rivers *Welland* and *Avon*; and that from their very origins down to their fall into the ſea. It likewiſe reached the river *Witham*, which directed the electric ſtream that way too to *Lincoln*: for which reaſon, as there meeting the ſame coming from *Boſton*, the ſhock was moſt ſenſibly felt. It reached likewiſe to the *Trent* at *Nottingham*, which conveyed it to *Newark*.

The firſt electrical ſtroke ſeems to have been made on the high ground above *Daventry* in *Northamptonſhire*, where the *Roman* camps are, made by *P. Oſtorius* the Proprætor. From thence it deſcended chiefly eaſtward, and along the river *Welland*, from *Harborough* to *Stamford*, *Spalding*, and the ſea; and along the river *Avon*, or *Nen*, to *Northampton*, *Peterborough*, and *Wiſbich* to the ſea. It ſpread itſelf all over the vaſt level of the iſle of *Ely*, furthered by very many canals and rivers, natural and artificial, made for drainage. It was ſtill conducted eaſtward, up *Mildenhall* river in *Suffolk*, to *Bury*, and the parts adja-

cent. All this affair, duly conſidered, is a confirmation of the doctrine I advanced on this ſubject.

10. I apprehend it was not the noiſe in the air, as of many cannon let off at once, preceding the earthquake, that ſo much affrighted people, or affected the ſheep, the rookery at *Kenſington*. the hen and chickens in *Grays-Inn-Lane* and the pigeons: it could not be barely the ſuperficial movement of the earth that diſturbed them all at once: I judge it to be the effect of Electricity, ſomewhat like what cauſes ſea-ſickneſs; ſuch a ſort of motion as we are not accuſtomed to. So the earthquake affects all thoſe of weak nerves, or that have nervous complaints, obnoxious to hyſterics, colics, rheumatic pains in their joints. Several women were ſeized with violent head-achs, before both the ſhocks we felt in *London*. It was this that affected the people, with a ſhortneſs of breath. This made the dog run whining about the room, ſeeking to get out: this made the fiſhes leap up in the pond at *Southwark*; like as the experiment of electrifying the fiſhes; it makes them ſick: and this cauſes the birds in cages to hide their heads under their wings, becauſe they cannot fly away: which is commonly obſerved of them in *Italy*, and countries where earthquakes are more frequent.

11. I obſerve, the ſhepherd of *Kenſington* thought the motion of the earthquake, and the ſound, were from N. W. to S. E. On the contrary, Mr *Byfield*, the ſcarlet-dyer in *Southwark*, thought the noiſe came from the river below-bridge, and went toward *Weſtminſter*; where it rattled ſo, that he did not doubt but that the abbey-church was beaten down.

Dr *Parſons* took pains to find out the way of the motion of the earthquake, from the different poſition of the beds; but, from the contradictory anſwers given, he could obtain no ſatisfaction, as to that point. All this, and what was obſerved from *Northampton*, of the motion being thought by ſome to be upward and downward, by others, rather horizontal or lateral, the counting the pulſes, and the like, only points out to us the prodigious celerity, and the vibratory ſpecies of the motion of an earthquake; but far, very far, is this from being owing to the tumultuous ebullition, the irregular hurry of ſubterraneous exploſions.

12. How the Atmoſphere and earth are put into that electric and vibratory ſtate, which prepares them to give or receive the ſnap, and the ſhock, which we call an earthquake, what it is that immediately produces it, we cannot ſay; any more than we can define what is the cauſe of magnetiſm, or of gravitation, or how muſcular motion is performed, or a thouſand other ſecrets in nature.

We ſeem to know, that the AUTHOR of NATURE has diſſeminated ethereal fire thro' all matter; by which theſe great operations are brought about. This is the ſubtil fluid of Sir *I. Newton*, pervading all things; the occult fire diffuſed thro' the univerſe, according to *Marſilius Ficinus* the *Platonic* Philoſopher, in the *Timæus* of his maſter. And the *Platoniſts*

toniſts inſiſt on an occult fire paſſing thro' and agitating all ſubſtance by it's vigorous and expanſive motion.

Before them, *Hippocrates* writes in the ſame ſenſe, I. *de victus ratione*, that this fire moves all in all. This ethereal fire is one of the 4 elements of the Ancients: it lies latent, and diſperſed thro' all the other 3, and quieſcent; till collected in a quantity, that overbalances the circumjacent; like the air crouded into a tempeſt; or till it is excited by any proper motion.

This fire gives Elaſticity, and Elaſticity, or vibration, is the mother of Electricity. This fire is in water, and betrays itſelf to our ſenſes in ſalt water. Many a time, when I have paſſed the *Lincolnſhire* waſhes, in the night-time, the horſe has ſeem'd to tread in liquid flames. The ſame appearence oft at the keel of a ſhip.

The operation of the ethereal fire is various, nay infinite, according to it's quantity, and degree of incitement, progreſs, hindrance, or furtherance. One degree keeps water fluid, ſays the learned Biſhop of *Cloyne*: another turns it into elaſtic air: and air itſelf ſeems nothing elſe but vapours and exhalations rendered elaſtic, by this fire.

This ſame fire permeates and dwells in all bodies, even diamond, flint, and ſteel. It's particles attract with the greateſt force, when approximated. Again, when united, they fly aſunder with the greateſt celerity. All this according to the laws preſcribed by the ſovereign ARCHITECT. This is the life and ſoul of action, and reaction, in the Univerſe. Thus has the great AUTHOR provided againſt the native ſluggiſhneſs of matter! light, or fire, in animals, is what we call the animal ſpirits; and is the author of life and motion. But we know not the immediate mode of muſcular motion, any more than how, in inanimate matter, it cauſes the vibrations of an earthquake.

Of this fire *Manilius* thus writes, who lived in the time of *Auguſtus*, *Aſtronom.* I.

Sunt autem cunctis permiſti partibus ignes,
Qui gravidas habitant fabricantes fulmina nubes,
Et penetrant terras, Ætnamque imitantur Olympo,
Et calidas reddunt ipſis in fontibus undas,
Ac ſilice in duro, viridique in cortice, ſedem
Inveniunt; cum ſilva ſibi colliſa crematur.
Ignibus uſque adeo natura eſt omnis abundans!

Which may thus be engliſhed:

Fire, univerſal nature traverſes;
It makes the thunderbolt in tumid clouds,
In dire *Volcano's* penetrates the earth;
And ſends the boiling water from it's ſprings:
In hardeſt flint, and ſofteſt wood, it dwells;

Which,

Which, by colliſion, ſhews itſelf in flame.
With fire ſo pregnant is all nature found!

13. The great queſtion then with us, is, how the ſurface of the earth is put into that vibratory and electric ſtate by heat and drineſs? we muſt needs acquit the internal of the earth from the charge of theſe ſuperficial concuſſions. How is the ethereal fire crouded together, or excited, ſo as to cauſe them; ſeeing, in our ordinary electrical experiments, we make uſe of friction?

But that friction alone does not excite Electricity, we know, from the obvious experiment of flint and ſteel; where the ſuddenneſs of the ſtroke, and hardneſs of the matter does it. Another method of exciting it, is the letting off a number of great guns; which ſo crouds the ethereal fire together, as to electrify glaſs windows; obſerved by Dr *Hales*. The *Aurora borealis*, *auſtralis*, all kind of coruſcation, meteors lightning, thunder, fireballs, are the effects, and may reciprocally be the cauſe, of Electricity; but how, in particular, we know not.

Come we to the animal world, we muſt needs aſſert, that all motion, voluntary and involuntary, generation, even life itſelf, all the operations of the vegetable kingdom, and an infinity more of nature's works, are owing to the activity of this electric fire; the very ſoul of the material world. And, in my opinion, it is this alone that ſolves the famous queſtion, ſo much agitated with the writers in medicine, about the heat of the blood. How theſe, how earthquakes, are begun and propagated, we are yet to ſeek.

We may readily enough preſume, that the contact between the electric and the non-electric, which gives the ſnap, and the ſhock, muſt come from without, from the atmoſphere; perhaps by ſome meteor, that crouds the ethereal fire together, cauſes an accenſion in the air, in the point of contact, on the earth's ſurface; perhaps another time by a ſhower of rain. We may as readily conclude, that, tho' the original ſtroke comes from the atmoſphere, yet the atmoſphere has no further concern in it: no aëreal power, or change therein, can propagate itſelf ſo inſtantaneouſly over ſo vaſt a ſurface as 4000 miles ſquare: therefore the impetuous ruſhing noiſe in the air, accompanying the ſhock, is the effect, not the cauſe.

But ſurely there is not a heart of fleſh that is not affected with ſo ſtupendous a concuſſion. Let a man eſtimate his own power with that which cauſes an earthquake, and he will be perſuaded that ſomewhat more than ordinary is intended by ſo a rare and wonderful a motion.

Hippocrates makes the whole of the animal oeconomy to be adminiſtred by what we call nature; and nature alone, ſays he, ſuffices for all things to animals: ſhe *knows* herſelf, and what is neceſſary for them.

Can we deny then that he here means a conſcious and intelligent nature that preſides over, and directs all things; moves the ethereal ſpirit, or fire, that moves all things; a divine neceſſity, but a voluntrary agent, who gives the commanding nod to what we commonly call nature; the chief

the chief inſtrument in the moſt important operations of the vaſt machine, as well as in the ordinary ones? And this leads us,

14. Laſtly, in regard to the ſpritual uſe we ought to make of theſe extraordinary *phænomena*, or of our inquiries about them; I ſhall firſt obſerve, that we find abroad, ſeveral of theſe earthquakes this year have been very fatal. In the laſt we read of at *Philippoli* in *Thrace*, the whole city was deſtroyed, and above 4000 inhabitants killed. At home, where above half a ſcore ſeparate concuſſions have been felt, there has not been one houſe thrown down, one life loſt. This ought to inſpire us with a very ſerious reflection about them. 2. We may obſerve, that if we did but read the works of *Hipprocrates*, *Plato*, and his followers, of *Tully*, *Galen*, and the like ethic writers of antiquity, whilſt we ſtudy and try the affections of matter, we ſhould improve in Philoſophy, properly ſpeaking; we ſhould lift up our minds from theſe earthly wonders, and diſcern the celeſtial monitions they preſent to us.

The original meaning of the word Philoſophy was rightly applied to moral wiſdom: we, who have improved both, ſhould join them both together. By this means we gather the truth of the higheſt and moſt excellent Philoſophy, to be found in thoſe volumes of firſt antiquity, which we call ſacred; and we ſhould adore that divine light which they hold forth to us; eſpecially in a country where the principles of true religion are open and undiſguiſed; where the eſtabliſhed profeſſion of it is rational, noble, and lovely; worthy of the Moral Governor of the world.

XLIII. About 4 years ago, Mr *Charles Orme*, of *Aſhby de la Zouch* in *Leiceſterſhire*, acquainted me that, in drying his glaſs tubes for his diagonal Barometers (which for ſome years he has continued to make in much greater perfection than any other perſon that I know of in *England**), he had obſerved a rotatory motion about their *axes*, and at the ſame time a progreſſive one towards the fire. He was ſo obliging then as to promiſe at any time to ſhew me the experiment; but other buſineſs intervening, I ſtill deferred accepting his offer; having the leſs curioſity to ſee it, as I imagined the motions were occaſioned by the draught of air up the chimney, aſſiſted by the weight of the inclined tube. But a little above a year ago, making ſome ſtay at *Aſhby*, upon repeating his offer, I went to ſee the experiment, which anſwered fully to his deſcription: the tubes which were about 4 feet long, and $\frac{1}{2}$ an inch over, moving at 6 or 8 inches diſtance from the fire, not only progreſſively, and about their *axes* along the ſide-wall they leaned againſt, but along the front-wall of the chimney, which made an obtuſe angle with the other; ſo that they ſeemed to move up hill, and againſt their weight.

Surpriſed at this, I thought the caſe deſerved a little farther examination; and propoſed placing two tubes horizontally, parallel to each

Two letters from the Rev. Mr Granville Wheler *F. R. S. to the* Preſ. *concerning a* rotatory *motion of* glaſs tubes *about their* axes, *when placed in a certain manner before the fire.* N°. 476. p. 341. April &c. 1745. *Read* March 28. *and* April 4. 1745.

* One of which Mr *Wheler* made a preſent of to the *R. S.*

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other, and at right angles to the face of the fire, to be ſupporters to a third which was to be placed upon them parallel to the fire. We did ſo, and with pleaſure obſerved the ſupported tube turn about it's *axis*, and move on towards the fire in ſuch a manner, as made me ſtill leſs inclined to think either of the motions owing to the draught of the fire, and certainly not to the whole weight of the moving tube; a fine ſpirit-level informing us, that the ſupporting tubes leaned from the fire; ſo that the motio was a little up-hill.

This ſucceſs determined me, with Mr *Orme*'s leave, to go on farther; and, furniſhing myſelf, from him, with tubes of ſeveral lengths and thickneſſes, I made ſeveral trials; and found, that with a moderate fire the experiment ſucceeded beſt, when the ſupported tube was about 20 or 22 inches long, the diameter about $\frac{1}{10}$ of an inch, and had in each end a pretty ſtrong pin, fixed in cork, for an axe to roll with upon the ſupporting tubes; which, to leſſen the contact, had nearly the ſame diameter with the moving one. Under theſe circumſtances the tube would begin to move at 18 inches diſtance from the fire; and continue to do ſo, with little intervals, till it touched the bars, and moved much in the ſame manner, when a little ball of cork, an inch or more in diameter, was fixed in the middle of it. But what ſurprized me ſtill more, and ſeemed to take off the objection of the draught of the chimney, was, letting it once ſtay a little while againſt the bars, I found it ſtill continue it's motion about its *axis* in the ſame direction.

This put me upon making little rings of wire, to fix upon and move along the ſupporting tubes, ſo as to ſtop the moving tube at any diſtance from the fire I pleaſed. Stopped with theſe, the motion of the tube about its *axis* ſtill continued.

Deſirous to try what would be the effect in or near an upright poſture, I made the pin at one end of my tube reſt upon a *China* plate, that at the other turn in a ſilver ſocket (that carried my pencil) fixed in an horizontal arm of wood, but ſo as I could ſlip it up and down, to adapt it to the length of the tube. Here I found, that if the tube leaned to my right hand, (which was the caſe of Mr *Orme*'s tubes before his fire) the motion was from E. to W.; but if they leaned to my left, the motion was from W. to E.; and the nearer I could get to the perfectly upright poſture, the leſs the motion ſeemed to be either way.

I now proceeded to place my tube horizontally upon a glaſs plane (a large fragment of a coach-ſide window-glaſs). The tube, inſtead of moving towards the fire, moved from it, and about it's *axis*, in a contrary direction to what it had done before. Obſerving that this glaſs plane was broader at one end than the other, and that the rotation backwards was more ſenſible when the narrower end was towards the fire, I placed a triangular piece of the ſame glaſs with it's *vertex* towards the fire nearly horizontal, but rather riſing from the fire; ſo that it's baſe was a little higher than it's *vertex*; and upon it a tube of glaſs, about 22 inches long, and $\frac{1}{8}$ of an inch diameter, near the *vertex* and the fire.

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This tube receded from the fire, moving about it's *axis* till it came to the diſtance of eight inches; which is four inches more than it receded the day before upon the ſame piece of coach-glaſs, before it was broken into this triangular form.

I was naturally led now to make uſe of two ſupporting tubes, inſtead of the triangular glaſs plane. Theſe were about 18 inches long each, and $\frac{1}{10}$ of an inch in diameter, and placed parallel to one another at the diſtance of about two inches, ſo as to ſupport the moving tube near the middle of it. When very nearly horizontal by the level, the ſupported tube moved from the fire about it's *axis* to the diſtance of 13 inches: when the ſupporters were a little raiſed at their remote ends, ſo as manifeſtly by the level to deſcend towards the fire, it receded to the diſtance of 10 inches, moving as before about it's *axis*; but in this latter caſe the fire had declined a good deal, otherwiſe, probably, the tube would have receded farther, tho' up-hill.

The next day, the ſame tube, when the ſame ſupporting tubes were $8\frac{1}{2}$ inches diſtant from each other, receded nearly as before: when $12\frac{1}{2}$ inches from each other, it ſtood ſtill; and when removed to the diſtance of $16\frac{1}{2}$ inches, the ſupported tube very manifeſtly changed it's motion, and went towards the fire; as it did afterwards, when the inclination of the ſupporting tubes was altered, ſo as to aſcend towards the fire.

I made ſeveral other experiments, with regard to the ſituation of the tubes to the fire, with regard to the quantity of fire ſuffered to come at the tubes, and with regard to attraction and repulſion, which I will not trouble you with at preſent: only obſerve, that, when the tube had 4 others under it, all ſupporting, one near each extremity, and one on each ſide of it's centre, no motion at all was perceived; and when two of them on the ſame ſide of the centre were taken away, the ſupported tube moved into an oblique ſituation with regard to the fire, the unſupported half receding from the fire.

Upon the whole, it appears ſufficiently plain, that the ſtream of air up the chimney is not the cauſe of the rotation: another may be aſſigned, ſimple and eaſy; but as I have already ſaid too much, it will be better to make it the ſubject of another paper.

I ſuſpended two fragments of ſmall tubes, 8 inches long, and about $\frac{1}{10}$ of an inch in diameter, near the fire, from two pins, by blue ſilk lines, which had each a loop at one end, were tied at the other to the top of the tubes, and hindered from ſlipping off by a little ſealing-wax. The tubes came together at the upper end, and receded manifeſtly from each other at the lower, appearing to be in a ſtate of attraction above, and a ſtate of repulſion below: but, ſuſpecting this to be owing to the ſealing-wax, which ſoon began to melt, I ſcraped it off both, leaving only as little as was poſſible, to hinder the ſilks from ſlipping. The conſequence then was, they came together at the lower ends, and very near ſo at the upper; and, when ſuſpended from one pin, ſo that the *The ſecond Letter.*

loops of the ſilks touched each other, the tubes ſeemed equally cloſe all the way down, without any appearance either of attraction or repulſion. But, imagining ſtill that a repulſive power in the heated ſupporting tubes, when placed near together, might poſſibly be the occaſion of the receding of the upper tube at contact with them. To put the matter out of all doubt, I wet the 3 tubes all over; yet the regreſſive and rotatory motion was ſtill manifeſt, with very little, if any difference; not more than might be well accounted for, from the increaſe of reſiſtance by wetting.

Theſe two experiments fully convinced me, that neither attraction nor repulſion would be of any aſſiſtance in ſolving our rotation Upon conſidering therefore the matter farther, I found nothing was wanting, but that the moving tube ſhould ſwell towards the fire; and indeed I thought I could perceive ſuch a ſwelling in Mr *Orme*'s long tube of four feet and an half, which I ſaw firſt placed near a good fire in the manner deſcribed in my laſt. For, allowing ſuch a ſwelling, gravity muſt pull the tube down, when ſupported near it's extremities horizontally; and a freſh part being expoſed to the fire, and ſwelling out again, muſt fall down again, and ſo on ſucceſſively; which is, in other words, a rotatory motion towards the fire.

When the ſupporting tubes are brought near to each other, as well as near to the centre of the ſupported tube, then the parts hanging over on each ſide, being larger than the part which lies between the ſupporters, will, by their weight, pull downwards, and conſequently force the middle part, reſting upon it's two *fulcra*, upwards; and being leſs advanced towards the fire, as being leſs heated, will, by their oblique ſituation, pull the middle part backward alſo from the fire: which effects, being ſucceſſive, will exhibit a rotatory regreſſive motion, quite contrary to what the tube had when ſupported near it's extremities: and when a ſingle tube lies inclining oppoſite to the fire, either to the right hand or the left, out of a plane perpendicular to the ſurface of the fire, gravity will not permit the curved part to reſt, but pull it down till it coincides with a plane perpendicular to the horizon; and, conſequently, as new curves are generated, new motions will be ſo too; that is, the tube will be made to move about it's *axis*; but with this difference, when the tube inclines to the right hand, the motion about the *axis* will be from E. to W. when to the left hand, from W. to E. The juſtneſs of this reaſoning is made manifeſt with a very little trouble; only bending a wire, and ſupporting it firſt near it's extremities, then near it's centre on each ſide, afterwards inclining it to the right, and then to the left; the bending in every caſe repreſenting the curved part of the tube next the fire. And that this ſolution is the true one, ſeems farther probable from hence, that when 4 ſupporters were made uſe of, one at each extremity, and 2 near the middle, there was no motion at all either backward or forward: nor is it of any ſervice to object here, that the increaſe of contact hinders the motion; becauſe,

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upon the plane of glaſs, mentioned in my former paper, ſo large as to have a much greater contact with the tube, both a rotatory and regreſſive motion was manifeſt.

XLIV. The ingenious author does not treat expreſly of thoſe productions of the chemical art, which we uſually call *phoſphori*, but principally of ſuch ſubſtances, whether natural or artificial, which imbibe the rays of light in ſuch quantities, and in ſuch a manner, as to appear luminous for a time, even in abſolute darkneſs.

An account of a book intituled, De quamplurimis Phoſphoris nunc primum detectis Commentarius. *Auctore* Jac. Barthol. Beccario. *Printed in* 4to *at* Bolognia, 1744. *Extracted and tranſlated from the* Latin *by* W. Watſon *F. R. S.* N°478. p 81. Jan. *and* Feb. 1746. *Read* Feb. 27. 1745-6.

Before I enter upon the ſubject matter of this treatiſe, I muſt take notice of the *apparatus* made uſe of in theſe inquiries. He cauſed a wooden box to be made, large enough either to ſit in, or ſtand in upright: yet not ſo large but he might with eaſe be carried to any place the moſt convenient for his obſervations. In this box was a little window, in which a cylindrical tube was fitted ſo exactly, that no light could be admitted but through the tube, in which there was an *apparatus* ſo adapted, that the perſon within could place any object proper for obſervation in ſuch a manner, as to receive as much light as it could contain, and then to turn it inſtantly towards his eye, without admitting the leaſt ray of light, beſides that brought in by the object. The inſide of this box muſt be abſolutely dark, without which caution many of the attempts would be unſucceſsful; becauſe the light in many of the ſubjects was neither very lively nor very laſting. Therefore it was neceſſary alſo, that the eyes of the obſerver ſhould be as free as poſſible from the impreſſions of former viſion: ſo that it were proper to make theſe obſervations immediately after ſleep, or to keep the eye you intend to make uſe of ſhut at leaſt $\frac{1}{2}$ an hour before you enter the box. A proof to know whether your eye is properly diſpoſed, will be expoſing a piece of white paper to the light in the tube, and then being able to perceive it's form and colour when turned towards you in the box.

He divides the *phoſphori* into ſeveral kinds; ſome of which ſhine of themſelves naturally, as the glow-worm and dates; or adventitiouſly, as the fleſh of animals, which moſt probably ariſes from a degree of putrefaction, ſometimes too ſlight to be obvious to our ſenſes. Other bodies become luminous by attrition, heat, the free acceſs of air, and, laſtly, by imbibing and retaining the rays of light. Thoſe bodies that are luminous by attrition, are amongſt others, ſome diamonds, and the hairs of animals; by heat, ſeveral ſort of gems, and mountain cryſtals; from the free acceſs of air, the *phoſphori* of *Kraft* and *Homberg*; from the aſpect of light, the *Bolognian* luminous ſtone, the preparation by *Chriſtian Adolphus Baldwin* of chalk diſſolved in ſpirit of nitre, as well as ſeveral others diſcovered by the late M. *Du Fay*, who found, that whatever ſubſtances would, by calcination, be converted into a *calx*, or whoſe concrete, from a ſolution in the acid of nitre, would bear fire enough to become red-hot, theſe bodies were adapted to imbibe and retain light.

The greateſt number of *phoſphori* are of the laſt-mentioned kind, and theſe are principally the ſubjects of this treatiſe. Some of theſe are natural, others artificial; but of theſe laſt the preparation is ſo ſlight, as not to change the nature of their conſtituent parts. The natural *phoſphori* are either foſſil, vegetable, or animal. The foſſil are, though very different in degree, ſome ſorts of earths, white ſand, lime-ſtones, *ſtalactites*, and ſeveral other figured ſtones, iſland cryſtals, flints, ſome ſpecies of agates, white arſenic; but no ſort of metals, metallic or ſulphureous bodies, as jet, amber, except the before-mentioned arſenic. On the other hand, ſalts imbibe light, provided they are diveſted of every metallic principle; otherwiſe not, though pellucid as poſſible. For this reaſon, none of the vitriols will imbibe light; but other ſalts will, though with a conſiderable difference as to quantity; for *ſal. gem.* and rock-ſalt imbibe very little; ſea-ſalt, if dry, and in cryſtals, much more; and, in like manner, *ſal ammoniac.*; more yet, *ſal. catharticum* and nitre; weak in the *natron* of the Ancients, and alūm; but brighteſt of all in *borax*.

In the vegetable kingdom we find very few *phoſphori*; that of dry rotten wood is weak, and not laſting; it appears chiefly upon the edges and inequalities of the ſurface. But this is moſt remarkable in the rotten wood of the fir-tree, and ſome others, where in the dark you ſee ſhining ſpots as big as tares; whereas, in full light, the whole ſurface appears alike. Some few barks are luminous, but not conſiderably ſo; but no fruits, ſeeds, or their meals. Cotton appears very bright, and the cryſtals of tartar; but fine loaf ſugar appears the moſt luminous of all, both without and within. Gums and reſins retain no light.

There is a vaſt variety of *phoſphori* in the animal kingdom, ſuch as the bones and teeth; to theſe may be added the ſhells of fiſh, egg-ſhells, the human *calculus*, bezoar, and in whatever parts of animals the terreſtrial principle is very predominant. But where there is a conſiderable quantity of oily matter, as in the hoofs, horns, and feathers, no light is manifeſt.

Having gone through the natural *phoſphori*, he propoſes ſome queries concerning them; of which the firſt is, In what and how great a light the object ought to be placed? He tried different *phoſphori* in different degrees of light, and found them imbibe moſt light from the ſun itſelf; next in quantity, when the ſky was clear; and the leaſt in foggy weather. Theſe experiments ſhould be made in the open air, and not in a houſe with the glaſs windows ſhut; becauſe many bodies appear luminous, when the light has come directly to them, which will not have that appearance when the light has paſſed through the glaſs. He laſtly tried what light they would imbibe from very bright flame, and found, that alabaſter itſelf, which is ſaturated more than any ſubſtance by the ſun's rays, imbibed exceedingly little. The next query is, How long theſe bodies ſhould remain in the light to be ſufficiently ſaturated? 4 or 5″ were found the utmoſt length of time required for that purpoſe.

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The other query is, How long the received light will continue in theſe *phoſphori?* It does not laſt the ſame time in all; but continues more or leſs, from 2 to 8′′, in proportion to the ſtrength of the *phoſphorus*, and the quantity of light received.

We paſs now to thoſe *phoſphori*, which are produced by art; and, firſt, to them which are made by the maceration of plants alone, and without any fire; ſuch as thread, linen cloth, but, above all, paper. The luminous appearance of this laſt is greatly increaſed by heat. This is confirmed by two experiments: the firſt is, by expoſing the paper, ſpread upon an iron grate, to the naked fire, yet not near enough to ſcorch it, and then laying a warm brick thereon to retain the heat; by which means it was obſerved, that where the paper was not ſkreened by the iron grate, it was moſt luminous; ſo that, by the lights and ſhades, you might diſtinguiſh in the dark the image of the iron grate a conſiderable time. The other experiment is the application of the paper to a plate of warm braſs; from which, when in the dark, you might very eaſily, by it's being leſs luminous, diſtinguiſh the margin of the paper, that had not been warmed by the braſs.

He proceeds to take notice of thoſe *phoſphori* which become ſo by the aſſiſtance of fire. But the fire here ſpoken of is not great enough to diſſolve their conſtituent parts, but only ſuch as may affect the external parts of their texture, and that but gently; ſo that the proceſs here mentioned is only drying or roaſting. For it is not the watery or the ſaline part in bodies which is torrefied; but the oleaginous, wherewith many vegetables, and moſt animals, abound.

The white fleſh of animals, ſuch as that of chickens, becomes a *phoſphorus* by roaſting, as well as the tendons, and whatever parts of animals become glutinous by boiling, ſuch as carpenter's glue, iſing-glaſs, to theſe may be added cheeſe. Bones, though they imbibe light without any preparation, have that property in a much greater degree when burnt, and their luminous appearance is much more lively. But roaſting has not this effect upon feathers, hoofs, horns, or whites of eggs. The ſame operation, which produces ſeveral *phoſphori* from the animal kingdom, gives alſo ſeveral from the vegetable. Thus, by gently toaſting, gums, as myrrh, gum tragacanth, and others, appear luminous, tho' different in degrees; and this light is clear, in proportion to the gentle evaporation of their aqueous parts. By this treatment, nuts of every kind, pulſe, corn, coffee-berries, meal, bread, and wafers, alſo become *phoſphori*. Turpentine, amber, and ſome reſins, require more fire before they imbibe light; ſo that you muſt diveſt them of their acid, and their light ethereal oil, to make them appear luminous. But here great care muſt be taken that they boil no longer than from being white they turn yellow; for if you proceed longer, your labour is loſt.

It is neceſſary that you ſhould be acquainted, that thoſe *phoſphori*, which are produced by torrefaction, ſoon loſe their power, which, perhaps,

haps, neither time, nor a thorough diſſolution of their parts, can deprive the natural ones of. In general, as long as the *phoſphori*, gained by torrefaction, preſerve their power, their light is more ſharp and ſtriking, but the natural, more weak. But thoſe that are gained by calcination, and *Baldwin*'s *phoſphorus*, ſeem to poſſeſs both the ſtriking light of thoſe gained by torrefaction, and the weaker light of the natural *phoſphori*: the laſt they preſerve a long time, but the former is loſt by degrees much ſooner. The well calcined aſhes of plants, or rather their terreſtrial parts remaining after the ſolution of their fixed ſalts by waſhing, and neutral ſalts, continue *phoſphori* after many years. So that, as far as we can judge, the luminating power which is gained by calcination, tho' not ſo intenſe, continues perpetual; whereas that gained by torrefaction always decreaſes, and in a very little while is no longer viſible. Some even, by this method, continue to imbibe light much longer than others. Gum *Arabic*, which continues longeſt, laſts 6 days; bread, not one; and coffee, only a few minutes. However, at any time, by a freſh torrefaction, you may recover theſe languid *phoſphori*; in which property they have great likeneſs to the *Bolognian* ſtone, and other *phoſphori* prepared by art. The *phoſphori* gained by torrefaction, as well as that of *Bolognia*, will not imbibe light, while they are warm; and this laſt does not appear ſo luminous when firſt prepared, as when it has been ſo ſome time.

The natural *phoſphori* do not differ only in the before-mentioned particulars, but alſo in the colour of the light itſelf. The light of the natural generally appears either perfectly bright, or ſomewhat inclining to yellow: the artificial produces a red, and ſometimes a brown light; but there are ſome exceptions to both theſe rules. From theſe different appearances, the author conjectures, that there are two ſorts of fire ariſing from different principles; *viz.* that in torrefied ſubſtances, from a ſulphureous, and that of the natural, from a terreſtrial principle.

In obſerving a piece of *lapis tutiæ*, which was rough and unequal on it's convex ſide, ſmooth and ſomewhat poliſhed on the concave; he found, to his ſurprize, that the rough ſide was luminous, and the ſmooth one not. He was very deſirous of inveſtigating the cauſe of this appearance. He remembered that ſome poliſhed marbles did not imbibe light, or very little, and that at their edges; but, having loſt their poliſh, they did admit and retain it. He therefore conjectures, that bodies, according to the diſpoſition of their ſurfaces for the reflection of the light, either ſuffer or prevent it's entrance into them. If this poſition holds good in the reflection, why ſhould it not with regard to the refraction? He produces 2 experiments, which he apprehends not foreign to the preſent purpoſe; but is yet making others, for his further ſatisfaction. He expoſed a glaſs bottle full of well-water to the light, and, as ſoon as poſſible, obſerved it in the dark. As he expected, it imbibed no light. Upon pouring into it ſome oil of tartar, it became turbid and whitiſh, from the well-water being uſually impregnated with

calcarious

calcarious matter. Upon obſerving it then in the dark, after having been expoſed as before, it retained enough of a pale light to diſtinguiſh the ſhape of the bottle. In a bottle of rain-water he diſſolved ſome talc; which ſtone, by rubbing, will diſſolve in water as ſalts do, without rendering it opaque; to this ſolution he added oil of tartar, and this mixture was luminous as the preceding. He therefore concludes, that ſo long as earthy corpuſcles are very ſmall, ſeparate, and agreeing in their ſurfaces with the water in which they float, they readily tranſmit the light they receive; for which reaſon it is impoſſible they ſhould retain light enough to appear luminous in the dark. But, by the affuſion of the ſaline principle, the earthy corpuſcles unite with the water and ſalt; and from the union of theſe principles the mixture grows thick, whereby the ready tranſmiſſion of light is prevented; ſo that, if this mixture is without colour, or any thing metallic, the light will be ſtopped long enough to be viſible in the dark. But if, inſtead of oil of tartar, you add ſugar of lead, the mixture will be turbid, but retain no light. In theſe two experiments the water becomes a *phoſphorus*.

Gems, cryſtals, and glaſs, whether whole, or powdered ever ſo fine, retain no light; ſo that neither their tranſparency nor whiteneſs contribute to their becoming luminous in the dark. Of ſeveral diamonds, in all appearance perfectly the ſame, ſome were very luminous, others not at all. Of many opaque ſubſtances, whether rough, poliſhed, or finely powdered, ſome were luminous, others not. So that it appears, that not only the external, but the internal texture of bodies alſo, may conduce ſometimes to their being luminous.

From the preceding experiments, the author is led to make ſome inquiries into the cauſe of this luminous appearance; and takes notice, that almoſt all bodies, by a proper treatment, have that power of ſhineing in the dark, which, at firſt, was ſuppoſed to be the property of one, and afterwards only of a few. How this is brought about, is not very eaſy to ſolve. If we ſuppoſe with ſome (from which our author, in ſeveral paſſages of this work, ſeems not averſe), that the light from a luminous body enters and abides in the *phoſphori*, we ſhall find ſomewhat new to admire in light itſelf. It is no new opinion, that this fluid conſiſts of very fine particles, which are continually darted forth from a luminous body, in all directions, with a very great velocity: but it has by nobody been laid down hitherto, that theſe particles are not diſſolved by the violence of their agitation, not diſperſed, nor immediately ceaſe to exiſt; but ſubſiſt ſtill, and adhere to what bodies come in their way, as heat does, and are the cauſes of odours. If therefore the particles of light are not diſſolved as ſoon as they are emitted from a radiant body, but continue ſome time, what elſe is required, but that we allow it's atmoſphere to every lucid appearance? If the *phoſphori* ſhine with a borrowed light, but not with their own, and that only when put in motion, and fired by the rays of a ſhining body, which ſome experiments ſeem to confirm, then other new doctrines will ariſe.

There

There muſt be then a hidden, a ſecret principle in bodies, to be lighted up by this moſt ſubtle fire. There will be in the univerſe a certain perpetual fire from theſe *phoſphori*; the matter of which, tho' conſtantly diſſipated by burning, does not waſte enough to be obvious to our ſenſes.

I cannot conclude my extract of this author, without mentioning, that his work is the reſult of a great variety of very ingenious obſervations, and of experiments made with the utmoſt accuracy; to which I may add, that where-ever he makes any conjecture concerning their cauſes, he does it with all poſſible decency, and ſubmiſſion to the judgment of the learned.

The Lacrymæ Batavicæ, *or glaſs-drops, the* tempering *of* ſteel, *and* efferveſcence, *accounted for by the ſame principle. By* Claud, Nic. le Cat, *M. D. F. R. S.* &c. *Tranſlated from the* French, *by* T. S. *M. D.* N°. 492. p. 175. Apr. &c. 1749. *Read* June 15. 1749.

XLV. The glaſs-tear, or drop, commonly called *Lacryma Batavica*, or *Lacryma Boruſſica*, becauſe it was firſt made in theſe countries, is much celebrated among Natural Philoſophers, upon account of the ſingular phænomena which it exhibits, and which have for a long time exerciſed their ſagacity.

The make of this drop is as ſimple as its explanation is difficult. It is the work of the meaneſt workman in a glaſs-houſe. On the top of an iron-rod they take up a ſmall quantity of the matter of glaſs in fuſion: they let it drop into a pail of water: the drop makes that part of the water which it touches, to boil with a hiſſing noiſe, as a red-hot iron would do, which it reſembles in that inſtant; and when it does not break in this operation, as it moſt frequently does, it forms the little pyramidal maſs, which is known by the name of a glaſs-drop; the effects whereof I will firſt relate, and then endeavour to account for them.

Experiment 1. This drop is of ſuch hardneſs and reſiſtance, that it bears ſmart blows of a hammer, without breaking.

Exper. 2. Nevertheleſs, if you grind the ſurface of this drop which reſiſted the hammer, or if you only break the tip of the ſmall end or tail, the whole ſhatters into powder.

Exper. 3. This ſhattering of the drops is attended with a loud report; and the duſt or powder to which it is reduced, ſhoots out, and ſcatters all around.

Exper. 4. If the drop be ground with powder of emery, imbibed with oil, it often eſcapes breaking.

Exper. 5. If this experiment be made in the air-pump, the drop burſts with greater impetuoſity, ſo as ſometimes to break the receiver; and it's duſt is finer than when done in the open air; and if it be made in the dark, the drop in burſting produces a little light.

Exper. 6. If this drop be annealed in the fire, it loſes all theſe ſingularities; and being reduced to the ſtate of common glaſs, it eaſily breaks under the hammer; and does not burſt upon breaking the ſmall end.

Exper 7. The drops that are made by letting them cool in the air, produce no other effects than thoſe which have been annealed.

The

The firſt Natural Philoſophers who endeavoured to inveſtigate the cauſe of theſe phænomena, imagined that they found it in the air. Some of them ſuppoſed, that this air was ſhut up in the drop by the cruſt which the cold water forms on it's ſurface while it is yet red-hot; and attributed it's rupture to the violence with which this air iſſued through the too narrow paſſage made for it, in breaking the ſmall end of the drop. Others maintained on the contrary, that the drop, in this ſtate, contained no air at all, nor any thing but particles of fire, or ſubtile matter; or, in one word, a vacuum of air; and that the ſudden burſting of the drop was occaſioned by the impetuous entry of the air into this ſort of vacuum. In fine, the *Carteſians* have ſubſtituted their ſubtile matter in the room of this exterior air, and ſay, that the drop is burſted by the leſs ſubtile particles of this matter; which entering with force into the drop by the opening made therein, and finding large pores on the inſide, and ſmall ones on the outſide, burſt the ſides of the drop, by ruſhing from the centre to the circumference, wherewith it's paſſage is obſtructed.

Meſſ. *Mariotte* and *Homberg* came afterwards; being provided with an air-pump, they cauſed one of theſe drops to be broken *in vacuo*; and *Homberg* having obſerved, that it broke therein better and with a louder report than in the open air; they both inferred, that neither the impetuous entry of the outward air, nor that of a fluid ſomewhat leſs groſs, could be the cauſe of this ſhock; becauſe the receiver of the air-pump is void of theſe fluids; and even if a little ſhould remain therein, it is too much rarefied, and too thin to be capable of ſuch an effect.

Mr *Mariotte*, thro' ſome remains of attachment to an opinion, which he had held to that time, did not intirely exclude the exterior air from the cauſe of the phænomenon of the drop; but thought proper to add another to it; which he makes uſe of as a ſubſtitute in caſes like thoſe of the preceding experiment, where the inſufficiency of the air, or of a fluid nearly ſimilar to it, plainly appears.

Mr *Homberg* ſhews no indulgence to the exterior fluid; and aſcribes the whole to the new cauſe, which is, the quality of tempered glaſs, which the drop acquires, like ſteel, by being thrown red-hot into cold water. This tempering, according to theſe great Academicians, confers at the ſame time more ſpringineſs to the parts, and leſs connection with each other. When a ſteel ſword-blade is bent forcibly, it breaks more eaſily than one of iron; and the jarring which is occaſioned by it's ſpring, is capable of breaking the other parts of the blade: and thus we ſee, that it generally breaks into ſeveral pieces. This blade is the image of the *Lacryma Batavica*, or glaſs-drop.

This is the point to which I found things brought, when I began to ſtudy the phænomena of the glaſs-drop.

The air was partly baniſhed from the inſide of this maſs of glaſs: there is none in the liquid red-hot matter of a glaſs furnace. It was purely out of complaiſance for a generally received opinion, that Mr

Mariotte allowed the exterior fluid any ſhare in the phænomenon; and Mr *Homberg* put the finiſhing hand to it's excluſion. But the ſort of temper given to the drop by plunging it red-hot into cold water, and it's compariſon with tempered ſteel, is not ſo much a cauſe as a compariſon: and moreover, is this compariſon very juſt? Can there be any between a long, thin ſword-blade, which breaks into two or three pieces, and a thick inflexible maſs of glaſs, which flies into powder. The tail alone of the drop might ſeem to favour this parallel: but an experiment, which I made, entirely deſtroys this opinion, and proves, that it is not the ſpring, or the vibrations of the parts of the drop, that occaſion it's burſting.

I put about half the tail of a glaſs drop into a vice between two bits of deal-board of about a finger's breadth. I ſcrewed the vice, till I ſaw this ſmall cylinder or thread of glaſs make impreſſions in the wood on each ſide for its lodgment, in order to be ſure that it could not be ſuſceptible of vibrations. In this condition I broke the end of the tail, ſupporting it on my nail, to prevent forcing any part but the end which I intended to break; and in order to be the more certain of giving no ſhock to the part that was ſqueezed in the vice. My drop flew into powder as uſual; and the portion ſecured between the two bits of wood, perfectly retained it's figure in the impreſſions wherein it was lodged. But when I touched this little cylinder, it was reduced to powder, much in the ſame manner as is ſaid to have happened to ſome men who had been ſtruck with lightning. Now, it was not poſſible for this glaſs to receive, or convey to the body of the drop any vibrations; or if any, they muſt be infinitely ſmall; and yet the effect was preciſely the ſame as uſual. Therefore the ſyſtem of vibrations is not happier than thoſe invented before it.

It is among the glaſs-workers, and in their art, that the ſecret of the *Lacryma Batavica*, or glaſs-drop, is to be ſought; and there it is that I think I have diſcovered it.

All thoſe who have ſeen glaſs-houſes know, that when a piece fails in the hands of a workman, he throws it aſide; and this piece is not long expoſed to the air, before it breaks in pieces: and when the ſame workman has ſucceeded in making a piece, and is willing to preſerve it, he takes great care not to let it cool in the air; but carries it hot into another oven of a moderate heat, where he leaves it for a certain ſpace of time. And this laſt operation is called *annealing the glaſs*.

A Natural Philoſopher, who is witneſs to this management, ought to inquire into the reaſons and neceſſity of it.

How comes it that the glaſs, which cools in the air, breaks; and when it has been nealed, it does not break? This is the reaſon, if I am not miſtaken.

A bit of melted glaſs, red-hot and liquid at the ſame time, is in that ſtate, purely becauſe it's particles are divided by ſo great a quantity of particles of fire, or ſubtile matter ſo violently agitated, that theſe component

ponent parts of the glaſs do hardly touch one another: they ſwim, if I may be allowed the expreſſion, in a flood of this matter of fire; and for this reaſon it is, that melted glaſs affects the colours of flame.

When this ſubſtance is expoſed to the air, the coolneſs of this fluid, which touches the ſurface of the glaſs, cools that ſurface firſt; that is, brings the particles nearer together, braces their pores, and thus impriſons the particles of fire, which ſtill fill the inſide of this ſubſtance. While theſe fiery particles find pores enough on the ſurface, to move freely, the glaſs continues whole; but when the glaſs grows colder, that is, when the pores of it's ſurface begin to confine theſe fiery particles; then their whole action is exerted againſt the parts of the glaſs, which they break into a thouſand pieces. In order to avoid this *fracas*, nothing more is requiſite than to keep the pores on the ſurface of the glaſs wide enough, that the fiery particles contained therein may paſs through, and fly off inſenſibly. Now, this is what is done, by putting the hot piece of glaſs into an oven, the moderate heat of which keeps theſe pores open to a certain pitch, and yet allows the glaſs to acquire it's due conſiſtence in this ſtate of middling poroſity: wherein conſiſts the annealing of glaſs and other fuſed ſubſtances.

Hence it appears, that all unnealed glaſs carries within itſelf it's principle of deſtruction, which is the matter of fire impriſoned. But the *Lachryma Batavica*, or glaſs-drop, is in this reſpect, in a worſe caſe ſtill than unnealed glaſs: for beſides that it has not been expoſed to this ſecondary heat, which keeps it's pores open, till the glaſs has acquired it's due conſiſtence, for fear that the coolneſs of the air alone ſhould not cloſe it's pores ſoon enough, and impriſon a ſufficient quantity of the igneous matter, it is ſuddenly thrown into cold water, which by it's coldneſs and weight is fitter than the air to produce ſuch an effect ſpeedily and effectually. Wherefore the only ſurprizing circumſtance in theſe glaſs-drops is, that any of them remain without breaking, by the great quantity of igneous matter ſuddenly ſhut up in them by the cold water. And indeed this accident befals more than one half of them; and thoſe that eſcape, doubtleſs owe their preſervation to the ſpherical or cylindrical figure of the compact ſhell, which the coldneſs of the water forms on their ſurface: for it is well known that this figure produces an equality of reſiſtance on all ſides, which conſiderably increaſes the reſiſting force: and this is the firſt reaſon why, as ſoon as this æquilibrium is broken, either by rubbing away one ſide of this ſurface, or by making a hole in it, or, in fine, by breaking the ſmall end of the drop; the reſiſtance is inſtantly overcome, and the igneous matter, impriſoned within the glaſs, and conſtantly upon the ſtrain againſt it, burſts it into powder.

This deſtroyed æquilibrium is but one diſpoſition that favours the effect of the impriſoned igneous matter: but the communication which is opened for it with the ſubtile exterior fluids, rouſes this matter which is in a ſtate of inactivity, develops it's ſpring, kindles it ſomewhat in

the manner of the phoſphorus, which produces no effect while cloſe ſhut up, but takes fire, as ſoon as a free communication with the outward air is given it.

On the union of theſe cauſes depend the phænomena of the glaſs-drop. It is of a hardneſs that reſiſts the ſtrokes of a hammer, becauſe the violent condenſation, given to it's ſurface by the cold water, into which it was thrown when in a ſoft ſtate, rendered it's texture very cloſe, compact, and conſequently hard. It burſts with great noiſe; and in ſo doing it retains the character of all the effects produced by the exploſion of the igneous matter. It's duſt flies two or three feet all around, becauſe it is puſhed forward by the action of a fluid contained in it's centre; which would not happen, if it had been the effect of an exterior fluid. This ſame duſt of the glaſs-drop darts forward with greater force in the air-pump than in the air, becauſe the air is an obſtacle, of which it is freed in the receiver of the air-pump: wherefore it ſometimes breaks the receiver; and for the ſame reaſon it's duſt is finer, that is, more minutely broken, as being done by a ſtronger power, and leſs counter-balanced. This violent exploſion produces light, becauſe the property of ſhining lightning is always the effect of ſuch an exploſion of the matter of fire: wherefore this fact affords another proof, that this matter is the principle of the phænomenon of the drop.

If the ſurface of the drop be ground with fine powder of emery, imbibed with oil, it frequently happens, that it does not burſt; becauſe the ſort of oily maſtic that reſults from this mixture, ſtops the pores of the drop, and prevents the ſudden communication of the exterior fluids with the impriſoned igneous matter; and as glaſs cannot be ground with very fine emery and oil, but by long rubbing; ſuch rubbing heats the drop, and gradually opens the pores ſo as to grant an inſenſible paſſage to the igneous matter, whereby the drop becomes at laſt in the ſame caſe with nealed glaſs; and in the caſe in which itſelf is, when it is put into the oven to be nealed.

When a glaſs-drop is made, by ſuſpending it in the air only, it does not break ſooner than nealed glaſs: becauſe as this ſmall maſs of glaſs retains it's heat a long while in the air, the heat ſerves as a nealing-oven, and keeps it's pores dilated long enough for the igneous particles to find a free paſſage.

The principles, by which I have accounted for the effects of the glaſs-drop, are not confined to this phænomenon alone: they are more general than is commonly imagined. Some corollaries, which I ſhall deduce from them, will prove what I advance.

The tempering of ſteel.

Steel, like the glaſs-drop, acquires it's hardneſs by being plunged into water: and if Meſſ. *Mariotte* and *Homberg* had compared them together in this circumſtance alone, they had been in the right.

The moſt celebrated Natural Philoſophers, in order to account for the tempering of ſteel, have had recourſe to different arrangements of it's

it's parts produced by the fire, and fixed, by the cold of the water, in the new ſtate, in which the violent heat had put them.

The mechaniſm of the tempering of glaſs-drops, applied to that of ſteel, is the moſt ſimple of all the hypotheſes, and anſwers all it's properties, which are theſe: 1. Tempered ſteel has a coarſer grain. 2. It is increaſed in bulk. 3. It is more hard and brittle. 4. By annealing it becomes leſs brittle.

Steel made red-hot is filled and ſwollen, and it's pores dilated, by the igneous matter. In this ſtate, the cold water, into which it is thrown, compreſſes and cloſes the parts of the ſurface, while the impriſoned igneous matter dilates the pores within: thus the texture of ſteel becomes more compact by theſe two cauſes, while it's pores are dilated. *Explanation.*

Theſe large pores conſtitute the coarſe grain of tempered ſteel. It's dilatation by the igneous matter, which could not be thoroughly condenſed by the cold of the water, cauſes it's augmented bulk: the cloſe texture of the ſubſtance that ſurrounds the pores, and the impriſoned igneous matter, occaſion it's hardneſs and brittleneſs. It's recoction or annealing deprives it of this brittleneſs, and of a part of it's hardneſs: becauſe it opens this texture, which it relaxes at the expence of the neighbouring pores, and drives the igneous matter out of it.

The fermentation of acids and alkali's ſeem, to me to be another corollary of the ſame principle. *Fermentation.*

1. It is pretty univerſally allowed, that the acid particles have the figure of ſmall needles; and that alkali's are ſpheroidal or polyhedrous bodies with a vaſt number of pores proper to admit the acid needles.

2. Experience ſhews, that ſalts are alkaliſed by fire, and that our juices are alkaliſed by heat, *&c.* What can the repeated action of the fire produce on ſalts, in order to alkaliſe them? It calcines them, blunts their points, and hollows them with a vaſt number of pores; and we ſee with the naked eye, that calcination has this effect on all bodies. In a word, it converts an angular very ſolid body into a very porous and light ſpheroid; and this body is an alkali by the firſt ſuppoſition.

3. Calcination introduces, and generally leaves in the pores of the calcined body, after the operation, a great quantity of igneous matter. This matter is perceptible to the ſenſes in the *Lapis Bononienſis*, which becomes a phoſphorus by calcination; in lime-ſtone, which by calcination is furniſhed with ſo great a quantity of igneous matter, that in the efferveſcence, which is raiſed in it by throwing a little water on this ſtone, you may kindle ſulphur or a match by it. The alkaline, or alcalized ſalts alſo, that is, thoſe which are calcined, have their pores full of the igneous matter.

4. Such is the nature of the igneous matter, that it tears aſunder whatever oppoſes it's paſſage, and makes it fly off with a report. This principle is univerſally allowed: the effects of gun-powder, of volcano's and earthquakes, prove it: and to come nearer our ſubject,

unnealed

unnealed glaſs breaks in the air, and the *Lacryma Batavica* does as much upon breaking it's ſmall end.

Whereas an alkali is a ſpongious body filled with the igneous matter, and an acid are points proportioned to theſe pores; theſe ought to be regarded as ſo many pegs or pins, which enter into the holes on the ſurface of the alkali, and fill them up exactly; whereby the igneous matter is impriſoned; and by the preceding principle it burſts the alcaline globule with noiſe, and ſcatters around the acid pegs, in the ſame manner as it burſts the glaſs-drop.

A mixture of an alkaline and acid liquor being compoſed of an infinite number of ſuch particles that burſt and broke to pieces, the liquor muſt take up more room, or ſwell. The particles of air therein contained, being toſſed about by all thoſe little exploſions, together with the neutral liquors, which are a vehicle to the ſalts, form the ſcum or froth; and the igneous matter, which gets out of the alkali's, and is agitated by the ſhocks of all theſe exploſions, produces heat, drags with it the aqueous and other volatile particles, which form the ſteam.

Yet there are cold fermentations, becauſe then, either the motion of the particles of fire, and their *fracas*, is inconſiderable; or becauſe theſe particles fly off eaſily by a direct motion. Moreover, at this day that we have it in our power to be convinced, that the *bruſh* or ſtream of electric matter is very cold, nobody will be ſurprized, that a ſtream of the matter of fire may produce cold.

If all the alcalious corpuſcles burſted at once, the fermentation would laſt but an inſtant: but as the acid liquor requires a certain ſpace of time, to penetrate the whole alcaline liquor, and fill the pores of the alcalious corpuſcles, the fermentation is performed ſucceſſively in a certain number of corpuſcles at a time, until they are all broken: and this ſucceſſion conſtitutes the duration of the fermentation; which ceaſes when there are none of the alkali's left entire.

Theſe principles not only ſerve to explain the fermentation which reſults from the mixture of acids and alkali's, but alſo almoſt all the motions of this kind, which are occaſioned by the mixture or penetration of two or more ſubſtances.

For example; lime, which we have mentioned above as a body filled with the matter of fire, and which produces an effervеſcence capable of lighting ſulphur, if water be thrown on it; lime, I ſay, produces this effect, only becauſe the particles of water, which enter into it's pores, have a tendency to ſhut up the igneous particles more cloſely. It is by a mechaniſm entirely ſimilar, that *Homberg*'s phoſphorus kindles into flame, upon being expoſed to the air: 'tis upon this principle likewiſe, that a mixture of ſpirit of wine and water acquires a new degree of heat; and ſo of other phænomena of this nature.

CHAP.

CHAP. II.

HYDROLOGY.

I. THOUGH the ſpring called *La Fontaine du Salût* is at a good diſtance from the town of *Bagneres*, it is, nevertheleſs, as much frequented as any in that country; and, beſides it's admirable effects in curing a great number of diſtempers, it likewiſe offers, to the eyes of the lovers of Natural Hiſtory, a very remarkable ſingularity.

Of the Fontaine du Salût, *near* Bagneres *in* Gaſcony: *with other obſervations; communicated by M.* Secondat de Monteſquieu, *of the Acad. Sc. of* Bourdeaux; *in a letter to* M. Folkes, *Eſq*; *Pr. R. S.* N°. 472. p. 26. Jan. &c. 1744. *Read* Mar. 8. 1743-4.

In the firſt bath, through which the largeſt of the two branches of the ſpring flows, there are found, from time to time, ſmall ſtones, of the colour of iron-ruſt, and of a regular figure; being either parallelopipedes with oblique angles, of which the ſides are unequal; or ſmall ſolid bodies with 6 ſides, only differing from cubes or dice in this, that the ſurfaces are not perfectly perpendicular one to another, but a little inclined; as alſo commonly longer than they are broad, and broader than they are high.

The largeſt which I have ſeen were but 11 lines in length, 9½ in breadth, and 6 in height: they are moſtly a great deal ſmaller. I have one which is very odd, being a parcel of 100 in one lump. There are ſome on which one may obſerve ſhining *ſtriæ*, that ſeem to be of a metallic ſubſtance.

I have heard, that a great many ſtones like theſe are alſo found on the ſides of a brook in *Spain*; from whence, without doubt, they got the name they are commonly called by, of *Ferreles d'Eſpagne*.

About two months ago, happening to take a walk in the road newly made between *Bagneres* and the *Fontaine de Salût*, I perceived, that, in digging the ditch on the ſide of the road, the workmen had laid open a rock of a ſort of imperfect ſlate, but ſofter, and of a lighter colour, than ſlate commonly is. The rock itſelf is compoſed of layers or beds lying almoſt parallel one over the other: the ſubſtance of the ſlate ſeems to be a compoſition of fibres or ſtrings, placed on the ſides of each other, and equally inclined to their beds or layers; whence it comes, that, upon breaking them with a hammer, the pieces, ſometimes, are pretty like the figure of a regular parallelopepid with oblique angles.

Upon a narrower examination of this ſort of ſlate, I found a great number of parallelopiped ſtones, like thoſe before ſpoken of, only ſmaller: I have ſeen them of all ſizes, from thoſe in which the largeſt ſide is but of 2 or 3 lines. I obſerved alſo, after having broken to pieces ſeveral little bits of ſlate, certain black ſpots; which, by the help of a microſcope, I found to be real figured ſtones.

Beſides this, I took notice, that every one of theſe ſtones, as long as it remains in the rock, is always found between two bundles or cluſters of

of tranſparent fibres, of which, generally, one is placed on the one, and the other on the oppoſite ſide. Theſe bundles are larger in great ſtones: thoſe which ſeem, to the naked eye, to be but ſmall black ſpots, are, nevertheleſs, accompanied by their bundles.

I have ſome of theſe ſtones, where the tranſparent fibres, of which every bundle is compoſed, had left a vacancy in the middle of their *axes*: this ſort of conduit being coloured with a matter of a ruſty colour, one finds likewiſe, ſometimes, between the fibres a little of this ruſty-coloured matter; and now-and-then metallic and ſhining veins. One might ſay, that the uſe of theſe tranſparent fibres is the ſame as of ſtrainers; which (let the matter be of a metallic nature or not) ſuffer nothing to paſs thro' them to the little ſtones, but ſuch particles as are proper to advance their growth, and ſo to ſerve them as it were for roots. In bits of imperfect ſlate, though harder, and of a bluer colour, than the ſort I before mentioned, are found ſmall ſtones, of a like figure, but different in this, that they are of a fine and ſhining braſs colour. They are, as well as the others, accompanied by tranſparent lumps.

One meets likewiſe with large ſtones of the colour of iron-ruſt in ſeveral rocks thereabout. It is probable, that the *Fontaine de Salût* paſſes thro' one like that I have deſcribed; and, if it meets with any of theſe figured ſtones in the ſides of the conduits thro' which it paſſes, it eaſily looſens them, and carries them along with it. The bundles of tranſparent fibres ſtick pretty faſt to the ſlate or rock, but are ſlightly faſtened to the little ſtone, from which they are very eaſily ſeparated. Hence it comes, that all thoſe which fall into the bath, or *Fontaine de Salût*, are got thither without their bundles.

The formation of theſe ſtones deſerves to be examined: if it was carefully ſtudied, it might, perhaps, give light to the formation of other figured ſtones. It even ſeems, that, by beginning with this figure, which is the moſt ſimple that one can imagine, one may the rather hope for ſucceſs in the like inquiry.

This parallelopiped figure with oblique angles is common to many ſtones in the country of *Bagneres*, and the neighbouring mountains. Several cryſtallizations of the grotto of *Campan* break into fragments of this figure: thoſe which hang down from the top of the vault of that grotto, are, originally, ſmall hollow pipes, formed by the water which trickles down drop by drop; and whoſe outer ſurface, fixing themſelves by their ſmall baſes, forms, by degrees, a ſort of blunted pyramids, which, like ſo many rays from the axis, which is nothing but the hollow pipe, become ſolid at laſt. This axis ſeems compoſed of plates, almoſt cylindrical, laid one over another; but, if broken, the whole divides into fragments of a parallelopiped figure. The blunted pyramids, that are about the axis, divide themſelves at firſt into other blunted pyramids; but, afterwards, almoſt all theſe fragments divide of themſelves into other fragments of a parallelopiped figure.

The

The ſtone of the mountain of *Barege*, upon which the *aſbeſtos* grows, breaks alſo conſtantly into fragments of the ſame figure.

I have likewiſe preſerved a bit of rock half tranſparent, the fragments of which are like the others.

Having ſeen ſeveral productions of nature, in which one diſcovers, that the figure I have been ſpeaking of ſo remarkably prevails, I was nevertheleſs ſurprized, when I found the ſame figure in the ſediment of the water of the *Fontaine de Salût*.

I had let a conſiderable quantity of the water of the mineral ſpring evaporate; there remained a ſhining duſt, in which I could diſtinguiſh nothing. I then looked at it through a microſcope; and, among ſeveral cryſtals of a leſs regular figure, I found many which were quite regular and well-ſhaped, with ſix faces, and oblique angles. Several perſons, who have, at different times, been eye-witneſſes of this ſight, have been well ſatisfied with it.

The waters of this ſpring contain no iron, as it is commonly believed. When you put the tincture of galls in it, it grows neither black nor red: this mixture only turns it a little, and makes it look whitiſh, after having ſtood ſome hours.

When theſe waters are evaporated by a mild and equal heat, the ſmall cryſtals are found ſwimming on the ſurface; where they join, and form a film upon the water; ſome of which ſticks alſo to the ſides and the bottoms of the veſſel. Thoſe cryſtals which are formed firſt, are inſipid; but thoſe which are produced towards the end of the evaporation, are, indeed, of the ſame ſhape, but of a tart and ſaltiſh taſte. There remains yet a little of this matter, which cannot be reduced to very regular cryſtals: it is of a very ſharp and pungent ſavour, but has nothing of the prevailing character of acid or alkali; at leaſt, it makes no ſenſible impreſſion upon blue paper.

The waters of the ſpring *du Pied* have the ſame quality as thoſe of the *Salût*: they produce the ſame effect when mixed with galls; yield cryſtals exactly of the ſame figure, but in yet greater quantity.

I have not made the ſame experiments with the other ſprings at *Bagneres*; but, it is probable, they do not differ from the former, except as to more or leſs.

II. *Carlſbad* is a ſmall town not far from the frontiers of *Saxony*, ſituated in an hollow between two high mountains: a ſmall river called *Toeple* runs through it from S. E. to N. W. The principal fountain riſes on the N. E. ſide, about 20 paces from the river, and about 5 or 6 feet higher than the ſurface of the water. This ſpring riſes through a ſquare tube of wood, whoſe diameter is about 7 inches, with a conſiderable degree of violence: whence it is called the *Sproudle*, or *Furious Fountain*. It comes from the mountain on the other ſide, and paſſes underneath the river, where the petrifying quality of it's own water has formed for itſelf an aqueduct of *Tophus*, through which it is conducted

Of the hot ſprings at Carlſbad; *by* James Mounſey, *M. D. Phyſ. to the* Czarina's *army*. N°. 493. p. 219. Oct. &c. 1749. *Read* Nov. 23. 1749.

to this place. Sometimes this aqueduct is ſo filled and choaked up with the *tophus*, that it burſts into the river, and puts the inhabitants to a conſiderable expence for repairing it. But to prevent this, they bore and clean it every year near the fountain. It forms rocks of *tophus* along the river-ſide, compoſed of *ſtrata* of ſeveral colours, according as the water has been impregnated with different matter, or perhaps from the difference of heat or cold, or the impreſſions of the air at the times of forming the *lamellæ*. This *tophus* is hard, and receives a good poliſh, and of it they make ſnuff-boxes, heads of canes, and other toys. Some years ago, in digging to lay the foundation of a church, 40 or 50 paces higher up the hill, they found vaſt quantities of this *tophus*, which was in many places ſo decayed and rotten (reſembling very ſoft clay), that they were obliged to dig ſeveral fathoms deep, before they could find a ſolid foundation. Here they threw out great quantities of the *piſolithus*, of the kinds I ſend you, which are compoſed of the ſame matter as the *tophus*, though of a very different conſtruction: the *tophus* being made up of plains joined together, whereas the *piſolithi* are globular, and compoſed of ſeveral ſpherical ſhells. Some globules are found above an inch in diameter, but more commonly about the bigneſs of white peas, and decreaſing gradually in ſize till they become as ſmall as fine ſand, and at laſt common *tophus*.

The ſeveral ſhells which compoſe theſe globules differ in colour as the *lamellæ* of the other *tophus* do: but theſe ſhells ſeparate more eaſily than the *lamellæ*, and ſhew that the colour often conſiſts in one very thin ſhell between two thicker ones.

Such kind of *tophus*, with *piſolithi*, is found at other places; but I have never yet met with any body who could give a ſatisfactory account of it's formation. Some think the *piſolithi* are drops of water petrified, as they are found commonly near falls of water which is impregnated with the like ſtony matter: and as the ſmalleſt ſcattered drops of water fly fartheſt from the centre, they ſo account for the gradual diminution in magnitude of the *piſolithi*. Others affirm they grow from the vapour of this petrific water, though they cannot tell how.

Amidſt this uncertainty, I determined, whilſt I was on the ſpot, to ſpare no pains to ſearch after (and diſcover, if poſſible) the manner how theſe regular globous bodies are produced. I have already told you, that the waters of theſe hot ſprings at *Carlſbad* are ſo replete with tophaceous matter, that where-ever they run, maſſes of *tophus* are formed; and when theſe waters are cold, a ſcum (like the little ſcales of the ſame matter) riſes on the top, ſome of which I ſend you, and I believe you will think it, on examination, little or nothing different from the ſubſtance of the component matter of the *piſolithi*, or from that which forms the common *tophus*, which I ſuppoſe to differ from the *piſolithi* only in appearance.

Firſt of all, I obſerved in the chinks and hollows of the ordinary rock-ſtone very ſmall *moleculæ* looſely adhering; I found alſo cluſters

of

of *piſolithi* in the like places, and on breaking up a piece of the rock by the ſide of the river, where it had been burſt by the water, I diſcovered maſſes of *piſolithi* lying in the chinks, and many looſe ones twirling round and played about in the bubbling water. My ſuppoſition therefore is, that the ſtream deſcending from the body of the oppoſite mountain, paſſing beneath the river, and afterwards burſting out on the ſide of the hill with a conſiderable force, could not fail to form caverns in the ſide of the mountain, and to change it's current as the paſſages became choaked up with the tophaceous matter: and as I found vaſt numbers of *moleculæ* like grains of ſand in the chinks where water paſſed, theſe being waſhed off might ſerve for *nuclei* to the *piſolithi*, and being kept in continual motion by the *vortex* or whirling of the water, would acquire a globular figure, and by receiving new ſhelly coats, from time to time, would increaſe in bulk, ſo long as they were ſuſtained, and whirled about in the water. And as in this caſe ſome would be precipitated ſooner, and others later, a difference in ſize muſt conſequently happen, and their arrangement muſt be according to their proportion of ſurface and gravity, till the place becoming full of ſuch matter, the water was obliged to ſeek out a new paſſage. At the formation of this kind of *tophus* in the caverns, ſome intervening accidents from the motion of the medium, the influence of the air, and other concurring cauſes, have ſometimes ſo far prevented a compact and firm conjunction of the component particles, that in ſeveral places it ſeems in a decaying ſtate; and is even ſoft as clay. In the air indeed it grows again ſomewhat harder, but then it is porous and light: and they call it *Sproudle Sand*. The inhabitants of a houſe near the church have a hollow, out of which they take this, and ſell it for the ſcouring and poliſhing of ſilver, *&c.* In this hollow it is very warm and ſuffocating, eſpecially in rainy weather, and then there riſes from it a ſtrong vapour.

On the other ſide of the river, at the foot of the mountain, are a good many houſes, and a broad ſtreet; croſs under which the ſtream runs, and in the winter no ſnow lies on the place where it paſſes. Some rooms in a houſe built here are always warm like a bagnio, and in one of the cellars may be heard the noiſe of the water running under ground. Along this ſide of the river are ſeveral hot ſprings, which differ in quality from one another, as well as from the water of the *ſproudle*. The principal of theſe is called the mill-fountain (from it's being near a mill) which is much uſed, and reckoned milder than the *ſproudle*. It is not near ſo ſaturated with the limy matter, and forms ſcarce any *tophus*.

Theſe ſprings either have different origins, or elſe the great ſtream divides in the body of the mountain into ſeveral branches; which, according to the nature of the paſſages they run through, or from the different thickneſs of their columns, and the velocity they move in, are impregnated with different matter, and when cold precipitate more or

leſs calx; but their ſalts are the ſame, nor is there much difference in the quantity they yield. The *ſproudle* is ſo full of the ſtony matter, that any thing laid into it is covered over with a thick tophus in a few days. When the water is taken up, and let ſtand a little in the air, it incruſts the veſſels that contain it, and it's ſurface is covered with a ſcale, like lime-water, which is made uſe of as a dentifrice.

I don't propoſe to inform you of the medical virtues of theſe waters, nor to enter into phyſical accounts of their origin: I have only in view to ſatisfy you about the formation of the ſpecimens I ſend you.

Moſt of the rocks about *Carlſbad* are an aggregate of *ſpatum*, *mica*, *quartzum*, *rubrica*, *cum matrice lapidis calcarii*, and cleave into rhomboids. The ſoil on the ſide of the mountain is made by the diſſolution of ſuch rocks intermixed with ſome vegetable earth; and the whole ſurface is covered with the leaſt diſſolvable parts, often adhering together in maſſes by the intervention of a limy matter like incruſted ſpatum. And I found higher up the mountain ſome rocks moundering into ſuch ſoil.

The *Carlſbad* waters give a good deal of neutral ſalt by boiling and cryſtallizing. From 1080 ℔. of water xxii ℥ of pure ſalt. I ſend you ſome which I prepared myſelf, ſuſpecting the Apothecary might adulterate it to increaſe the quantity.

My thermometer being broke, I procured one of a friend: but not knowing of what conſtruction it was, I tried it in the following manner: in melting ice the mercury fell to 28 ½ of it's equal parts, and by the heat of my body it roſe to 66 of thoſe parts. This thermometer held into the *ſproudle* fountain roſe by it's head to 96, and in the mill-fountain to 67.

About twenty miles from *Carlſbad* to the S. W. near the town of *Eyra*, is a cold ſpring of mineral waters, much in uſe in theſe countries. This gives alſo a ſalt much of the ſame kind. To the ſouth from *Carlſbad* about twenty five *Engliſh* miles are likewiſe ſeveral cold ſprings: one of which is much richer in this ſame kind of ſalt than the former. It belongs to the monaſtery of *Toeple*. In the winter, when they boil this water, from x ℔. of water they get ſometimes above ℥i of ſalt. They prepare here a neutral ſalt, by adding a mineral acid, or perhaps ſome other neutral ſalt (but the preparation they keep a ſecret) which makes it ſhoot into beautiful cryſtals. It is called *Sal Medium Toeplicenſe*, and is ſold in many places of *Germany*. I ſend you ſpecimens of all theſe. On expoſing theſe ſalts ſome time to the air, they fall into a *magneſia*, but diſſolving and cryſtallizing them again recovers them; though the oftener they are diſſolved, the cryſtals ſhoot the ſmaller.

About 7 miles S. W. from *Carlſbad*, at *Altſettle*, are mines of black *ſchiſtus*, and formerly they made a great deal of alum and vitriol from it; but it is now neglected, as they find in the ſame mines plenty of *gleba pyriticoſa*, from which they diſtil ſulphur. Six hundred weight of this *pyrites* give one of ſulphur: and the oven makes from one to

two

two hundred weight per week. The reſiduum being thrown in great heaps in the open air, takes fire, and conſtantly ſmokes. This matter they throw into large reſervoirs of water, which afterwards they let run off into the boiling-houſe, and ſo make copperas.

About 9 *Engliſh* miles to the ſouth from *Carlſbad*, are the tin mines of *Schlachtenwald*. They reckon this mine has been wrought near 500 years. There are five entries, four whereof are provided with machines for hoiſting the barrels with the tin ſtone: the fifth is for drawing the water out of the mine. The number of miners who work below ground are 90: each man delivers 25 barrels of this ſtone per week, and receives ſomething leſs than half a crown wages. They have different inventions in the mine for ſplitting the rock, but the moſt effectual one is burſting it with gunpowder. The whole people employed in theſe mines are about 300. The main body of the mine is nearly 700 feet in diameter, and from this go ſeveral E. and W. for ſo the mineral runs. The broadeſt of theſe ways is about 2 feet, and the mineral in theſe veins is richer than what is found in the main body of the work, whoſe greateſt depth is 650 feet. The tin-ſtone is firſt burnt in kilns, which they ſay betters the tin conſiderably, and makes it much more eaſy to ſtamp. After this preparation it is brought to the ſtamp-mills, where by ſtamping it becomes like grey river ſand, which they waſh and ſeparate the tin from in the following manner. They throw it by ſhovels-full into baſons where there paſſes a current of water, and by keeping of it ſtirring it runs over by a broad conduit deſcending by ſteps, which are covered with coarſe linen cloth; and by this operation the ſand is waſhed away, and the tin remains on the cloth in form of a black ſcaly powder, and dried is fit for melting. One hundred weight of the ſtone gives only ℥iii of tin; and 150 ℔. of the clean-waſhed tin-mineral give 140 ℔. of tin. There are ten melting-ovens, each whereof can melt 9 or 10 hundred weight in 24 hours; the breadth of theſe ovens within-ſide is 8 or nine inches, and from 10 to 12 feet long, blown by two pair of bellows. The proportion of charcoal to the metal is near an equal weight. They are thrown into the oven by degrees, alternately: the reſiduum they melt three times over, which always yields new metal. They make here about 800 centers *per annum*, which is ſold from 53 to 56 imperial gouldens *per center*. They find ſometimes the black and ſometimes the white cryſtal mineral in neſts, or cluſters: the *ſtannum poledron nigrum* is a very pure and rich tin ore: they ſay the white is rich alſo, but 'tis ſo hard and difficult to melt, that the tin is burnt to an aſh before it can be brought to fuſion.

Near *Geffries*, in *Bareith*, they boil vitriol. The mineral from which they make it, is a black *ſchiſtus*, ſome of it too is brown. It has ſeveral ſmall veins of pyrites in it. When firſt taken out of the pits it has no taſte, but after it has been expoſed ſome time to the weather, and begins to moulder, it acquires a very ſharp taſte. It is laid in great heaps, under which there are ciſterns for receiving the water that runs from

from it after rain, or that they pump upon it when the weather is dry. This water is conveyed by conduits into the boiling-houſe, where there are two leaden kettles, in which it is boiled to a ſtrong lee, and then let off into receivers where it ſhoots. Theſe two kettles make from 8 to 9 hundred weight *per* week, which is all wrought by two ſervants: it not having been found neceſſary to add any new mineral to the heaps theſe 15 years paſt, as they aſſured me. But as the quantity of the mineral conſumed in that time is not known, it is impoſſible to determine how much of this ſalt has been ſupplied by the air. They only add to the quantity half an hundred weight of iron, which is conſumed in the kettles every week, and makes it ſhoot into copperas; but in place of this, if they add copper, it makes blue vitriol. Formerly they made alum here likewiſe from the ſame lee, only inſtead of iron or copper they added pot-aſh and urine: but the expence of the firſt, and the difficulty of getting the other in ſufficient quantity, has made them leave off making alum here for ſome years paſt.

An examination of the ſtrength of ſeveral of the principal purging waters, *eſpecially of that of* Jeſſop's Well; *by the Rev.* Stephen Hales, *D. D. & F. R. S. communicated in a letter to* C. Mortimer, *M. D. Secr. R. S. with a letter from* Swithin Adee, *M. D. F. R. S. to Dr* Hales, *on the virtues of the ſaid* well. N°. 495. p. 446. Apr. &c. 1750. *Read* May 24. 1750.

III. *An account of the ſeveral quantities of* ſediment *which were found in a pound averdupois of the following* purging waters, *evaporated away to dryneſs, in* Florence *flaſks, cut to a wide mouth*; viz.

	Grains
1. *Marybon-fields* near *London*	24
2. *Peterſtreet Brewhouſe, Weſtminſter*	27
3. *Ebſham*	34
4. *Scarborough*	40

And it was found nearly the ſame by Dr *Shaw* and Dr *Short*: a little more or leſs, according to the wetneſs or dryneſs of the ſeaſons $\frac{1}{3 \cdot 2}$ of this in calcarious matter; the reſt, moſtly what is called nitrous ſalts, on account of the oblong cryſtals which it ſhoots into.

5. *Dog* and *Duck, Lambeth*	$40\frac{1}{2}$
6. *Kilburn*, four miles from *London*, in the way to *Edgware*	43
7. *Acton*	44
8. *Cheltenham, Glouceſterſhire*	60

Dr *Short* found the following proportions in *Cheltenham* water; *viz.*

Sept. 1738. calcarious ſediment	$\frac{1}{10 \cdot 6}$ of 74
Dec. 1738.	$\frac{1}{14}$ of 42
July 1739.	$\frac{1}{9 \cdot 8}$ of 70

He ſays it is the beſt and ſtrongeſt nitro-calcarious water in *England*, very bitter, having only a little ſubtil impalpable earth mixed with it's ſalt.

9. *Cobham Well*, a mile ſouth of *Church Cobham, Surrey*, once 68 grains, another time 60 grains — 68

10. *Jeſſop's Well*, on *Stoke Common*, in Mr *Vincent*'s manor, about three

three miles S. of *Claremont*, *Surrey*, *Sept.* 11, 1749. after long dry weather, 82 grains in a pound of the ſurface-water 82

October 16, after a conſiderable quantity of rain, the ſurface-water yielded but 60 grains. *Nov.* 21, the ſurface-water yielded 65 grains.

This great inequality of the ſtrength of the ſurface-water put me upon trying whether the water at the bottom of the well, near the ſprings, were ſtronger than the ſurface-water. And in order to this, I procured, *Dec.* 11. a bottle of the water near the bottom, which was ten feet below the ſurface of the water; which was done by tying an empty bottle to the end of a long pole, with a line fixed to the cork, to pull it out when at the bottom, for the water to fill it: and I had at the ſame time another bottle full of the ſurface-water. The lower water yielded 82 grains; the ſurface water but 48 grains; and it was the ſame upon a ſecond evaporation of thoſe waters. Hence we ſee how much ſtronger the water near the bottom is, than at the ſurface; even when the preceding rains have been but moderate; for they had not as yet been ſufficient to raiſe the ſprings in this country much. Hence we ſee that the ſtronger lower water may eaſily be come at by means of a pump; as alſo, that the upper land-ſprings, ſoon after rains, make the water near the ſurface weaker: but, in long dry weather, when there are no land-ſprings, the ſurface-water, and that at the bottom, are nearly of an equal ſtrength: for it requires time for the ſaline mineral virtue to be equally diffuſed through a maſs of that depth of water, whoſe upper part is inceſſantly weakened by a land-ſpring of freſh water.

Hence we ſee how adviſeable it is, in order to keep out the land-ſprings, to dig a narrow trench ſome feet depth, round the well, to be filled with ſtiff clay well rammed.

The mineral virtue in this water ſeems to be much like that of *Cheltenham*, in it's ſhooting into very bitter, regular, oblong cryſtals, which are, on that account, called nitrous; though they are not a true nitre; for neither theſe, nor thoſe of *Cheltenham*, will deflagrate or flaſh in touch-paper, nor on burning charcoal, as true nitre will do; ſome of which ſtill retain their form and firmneſs for 17 months ſince they were cryſtallized; whereas the cryſtallized ſalts of ſeveral other purging waters have crumbled, and in a great meaſure waſted, away in much leſs time: a greater proportion of the ſalts of *Jeſſop*'s *Well*, ſhoot into oblong cryſtals than thoſe of *Cheltenham*; and it's water alſo gives a ſtronger green tincture, with violet-flowers. The purging quality reſides chiefly in theſe cryſtalline ſalts, and a ſmall proportion of common ſalt; ſome of which there is in all theſe mineral waters.

The proportion alſo of it's earthy calcarious matter, is but $\frac{1}{114}$ part of it; which, like that of *Cheltenham*, is but little, in compariſon of the much greater quantity of it in other purging waters: it is alſo ſoft

and

and impalpable, like that of *Cheltenham*, and not harſh and coarſe, as it is in ſome other purging waters.

And as the quantity of purging ſalt in this water is conſiderably greater than in any other, ſo it is found by experience, that, proportionably, a leſs quantity of it ſuffices, which makes it ſit the better on the ſtomach. It is alſo obſerved to exhilarate thoſe who take it.

It was obſervable of the ſediment of ſeveral of theſe waters, that, when dried, and while hot, there aſcended plenty of inviſible volatile ſalt fumes, ſo pungent that the noſe could not bear them. Hence we may reaſonably conclude, that the waters which abound moſt with purging ſalts, ſuch as thoſe of *Jeſſop*'s *Well*, ſhould be proportionably preferable to weaker waters, which are ſtrengthened by boiling half away; whereby not only the more ſubtil active parts are evaporated; and thoſe that are left are decompounded, and formed into new groſſer combinations; as are alſo the calcarious particles, which are ſo fine as to paſs the filter before evaporation, but not after it. This was the reaſon which induced me to examine, by various repeated trials, and to give an account of the ſuperior ſtrength of *Jeſſop*'s *Well* water, above all others that I have examined or heard of.

When *Jeſſop*'s *Well* was cleaned, *Oct.* 16. 1749. after a conſiderable quantity of rain, after about half a foot depth of black muddy filth was taken out, then the natural fat ſandy-colour'd clay-bottom appear'd; thro' ſeveral parts of which the water ouzed up at the rate of 160 gallons in 24 hours.

The water which then came freſh from the ſpring gave a weak bluſh with galls; but when put into bottles it did not do ſo next day; a ſign that there is ſome degree of ſteel in it.

It was very obſervable, that the man who ſtood about 3 hours bare-legged in this well-water to clean it, was purged ſo ſeverely for a week, that he ſaid he would not venture, on any account, thus to clean the well again. And it was the ſame with another man, who cleaned the ſame well about 12 years ſince. And I am credibly informed by a merchant, that, being in a warehouſe in *Egypt* to ſee *ſenna* bailed up, it had the like purgative effect on him.

In order to get a ſatisfactory account of the efficacy of theſe waters, I deſired Dr *Adee* of *Guilford*, who has long preſcribed them to his patients, to give me his opinion of them; which he has done in the following letter; *viz.*

SIR, *Guilford, March* 14, 1749.

I HAVE found very advantageous and uncommon effects from the uſe of the waters of *Jeſſop*'s *Well.* Some of my patients who have drank them ſteadily and cautiouſly have been cured of obſtinate ſcurvies. As I had a long time ago reaſon to think there was a fine volatile ſpirit in them, I therefore obliged ſome to drink them for a courſe of time at the well as an alterative, with very happy conſequences. When I have ordered

ordered them as a purge, they have worked very ſmartly, but have not diſpirited. I am glad to have it in my power to confirm your ſentiments by my own obſervations; and am ſatisfied theſe waters, if continued a proper time, and taken in a proper manner, may be rendered very beneficial to mankind, according to the beſt opinion that I can form.

IV. No accounts of the flux and reflux of the ſea were ſatisfactory, till Sir *I. Newton*'s penetrating genius deduced their true cauſe from the laws of gravitation. His principles carried ſuch conviction along with them, and gave ſuch an eaſy ſolution of ſome of the moſt remarkable *phænomena*, that mankind ſeemed to imagine a thorough knowledge of the tides might be obtained from an attentive conſideration of the principles he had eſtabliſhed, without the trouble of further obſervations; but, as he, and all Philoſophers ſince his time, have conſidered only, or principally, the influence of the moon in elevating or depreſſing the tides; their ſeveral directions, velocities, and other affections, reſulting from the influence of land, ſhoals, and winds, remain ſtill as inexplicable, and as little known as ever.

The ſtate of the tides *in* Orkney; *by Mr* Murdoch Mackenzie. N°. 492. p. 149. Apr. &c. 1749. *Read* May 25. 1749.

As a diſtinct knowledge of theſe things is not only conducive to the advancement of ſcience, but would greatly contribute to a convenient and ſafe navigation, it may not be unacceptable to communicate ſuch remarks on the tides about the *Orkney* iſlands, as came under my obſervation, while I was employed in ſurveying and navigating that and other adjacent places; hoping it may incite others to explore the various motions of that element, on which ſuch a conſiderable part of the world are daily employed, in a more extenſive and accurate manner than has yet been done.

From ſome obſervations on the tides in *Orkney*, I incline to think the water begins to riſe and fall ſooner near the ſhore than at a diſtance from it.

When ſpring-tide is at it's greateſt altitude, or depreſſion, the water continues in a quieſcent ſtate near half an hour: neap-tides continue ſo about an hour and a half.

The motion of the water, both in aſcent, deſcent, and progreſſion, is accelerated from the firſt to the fourth hour, commonly; from the fourth to the laſt hour it's velocity diminiſhes. This, however, admits of ſome variation from the influence of winds.

The greateſt ſpring-tides, and leaſt neap-tides, are commonly on the third or fourth day, after the ſyzygies and quadratures; but in this alſo the winds have a conſiderable influence; W. and S. W. winds making the greateſt floods, and leaſt ebbs; N. and N. E. winds, on the contrary, making the greateſt ebbs and leaſt floods in *Orkney*, and on the North Coaſt of *Scotland*. When flood tide is raiſed higher than ordinary by winds, the ſubſequent ebb is not ſo low as it would have otherwiſe been. When a high flood is raiſed by the moon, the ſucceeding ebb is proportionally low.

Ordinary ſpring-tides riſe 8 feet perpendicular, ordinary neap-tides $3\frac{1}{2}$; extraordinary high ſpring-tides riſe 14 feet; extraordinary low, only 5; extraordinary high neap-tides riſe above 6 feet; extraordinary ſmall neap-tides not above 2. Low-water neap-tide, at a mean, I judge is about 3 feet above low-water ſpring-tide, and high-water ſpring-tide about 3 feet above high-water neap-tide: yet the riſe and fall vary ſo much, that it would require a longer courſe of obſervations than I have had opportunity of making, to determine what is moſt frequent in this caſe.

When a ſtream of tide is interrupted by land, or rocks, or is confined within a chanel, or long arm of the ſea growing uniformly narrower, the water will riſe higher there than in neighbouring places, where it is not ſo affected. If the chanel, or arm of the ſea, has ſeveral windings, or reaches, as they are called in the *Thames*, the ſuperior elevation will not be ſo conſiderable.

The following obſervations of the riſing and falling of the water, were made in the day-time, in the bay of *Kirkwall*, *anno* 1748.

Auguſt 8. Wind. W. a breeze.
Laſt quarter 4th day.
Moon's apogee diſtant 24°.
Moon's declination 27° N.
Moon bearing at firſt W. by N.

		Feet	Inch
The water roſe	1*ſt* hour	0	$\frac{1}{2}$
	2*d*	0	2
	3*d*	0	$4\frac{1}{2}$
	4*th*	0	$9\frac{1}{2}$
	5*th*	0	$5\frac{1}{2}$
	6*th* and to the end	0	$5\frac{1}{2}$
	In all	2	5

Auguſt 15. Wind E. N. E. ſmall breeze.
New moon 3d day.
Moon's apparent diſtance 65°.
Moon's declination 7° S.
Moon bearing S. S. E.

		Feet	Inch
The water fell	1*ſt* hour	0	$1\frac{1}{2}$
	2*d*	0	$4\frac{1}{2}$
	3*d*	1	4
	4*th*	1	8
	5*th*	1	5
	6*th*	0	10
	In all	5	9

Auguſt

Auguſt 23. Wind W. almoſt calm.
Firſt quarter, 3d day.
Moon's perigee diſtant 13°.
Moon's declination 25° S.
Moon bearing E. by N.

		Feet	Inch
The water roſe	1*ſt* hour	0	6
	2*d*	1	0
	3*d*	1	8
	4*th*	1	0
	5*th*	0	8
	6*th*	0	7
	In all	5	9

Aug. 29. wind S. *b* W. breeze at firſt, afterwards calm.
Full moon 3d day.
Moon's perigee diſt. 68°.
Moon's declination 6° N.
Moon bearing N. N. W.

		Feet	Inch
The water fell	1*ſt* hour	1	0
	2*d*	0	10
	3*d*	1	4
	4*th*	1	2
	5*th*	2	10
	6*th*	1	0
	In all	8	5

Aug. 27. Wind W. S. W. a ſmall breeze, the day of full moon, moon's perigee diſtance 36°, decl. 4° S. bearing N. N. W. the water fell in all 8 feet 4 inches perpendicular.

Aug. 30. calm, 4th day after full moon, perigee diſtance 80°, decl. 13° N. moon bearing N. *b* W. the water roſe 8 feet 3 inches.

Sept. 3. Wind S. W. a ſmall breeze, firſt day of the laſt quarter, apogee diſt. 50°. decl. 27°. N. moon bearing W. the water roſe 6 feet 1 inch.

Sept. 6. Wind E. a ſmall breeze, 4th day of the laſt quarter, apogee diſt. 15°. decl. 21° N. moon bearing W. the water roſe 3 feet 9 inches.

Sept. 15. Wind S. a moderate breeze, 5th day after new moon, perigee diſt. 80°. decl. 24° S. moon bearing S. *by* E. the water fell 5 feet 9 inches.

To aſcertain all the varieties in the riſing and falling of the water, the obſervations ought to have been continued much longer, the night-tides as well as day-tides obſerved; alſo the exact times of the beginning and ending of each, the ſtrength of the wind and weight of the atmoſphere by a barometer.

The foregoing articles relate to the riſing and falling of the water; the following to the various motions of the ſtream, and their conſequences.

On the coaſt of *Orkney*, and *Fair* iſle of *Shetland*, the body of the flood comes from the north-weſt; on the eaſt and weſt coaſts of *Lewis*, one of the weſtern iſles of *Scotland*, it comes from the ſouth.

A league or two off the coaſt, the ſtrength of the ſtream is ſcarce ſenſible, except when it is confined by land, or near rocks or ſhoals.

When the tide begins to riſe or fall on the ſhore, about that ſame time the ſtream near the ſhore begins to turn or reverſe it's direction, a few irregularities excepted.

The ſtream of tide changes it's direction ſooner near land than at a diſtance from it; inſomuch that, in a place two or three miles from land, the turning of the tide is two hours, or more, later than on the adjacent ſhore: at intermediate diſtances the ſtream turns at intermediate times. Hence a veſſel may find a favourable tide near land, while it would be againſt her a mile or two from it; and the contrary.

During the continuance of flood, the ſtream varies it's direction gradually from the E. toward the S. and the ſtream of ebb from the W. towards the N: that is, if the ſtream, when it becomes firſt ſenſible, runs E. at the latter end of the tide it will run S. if the proximity of land or ſhoals does not hinder this change of direction.

The greateſt velocity of ſpring-tide in *Orkney*, in the chanels where it runs quickeſt, is about 9 miles an hour: the greateſt velocity of neap-tide is about one third or fourth of ſpring-tide. The tides are moſt rapid commonly between the third and fourth hour. Spring-tides acquire a conſiderable degree of ſtrength in leſs than one hour after their quieſcent ſtate begins; neap-tides are hardly ſenſible in two hours after.

In ſimilar ſtreights or chanels, lying in the ſame direction, and ſupplied from the ſame part of the ocean, the velocity of the ſtreams will be in the direct *ratio* of the breadth of the inlets, and the inverſe of the outlets.

If a ſound, or ſtreight between two iſlands, or continents, lies in the direction of the main body of the tide, the velocity of the ſtream in that ſtreight will be greater (all other things alike) than in any other adjacent one, not lying in that ſame direction.

If an iſland lies directly in the tide-way, the ſtream will divide, or ſplit, before it reaches the iſland, into two branches, one of which will run toward one ſide or end of the iſland, and the other toward the other end of it; and, in paſſing by, will be reflected a little from the land.

Hence

Hence a veſſel, in a calm, carried along with a ſtrong ſtream of tide, is in no danger of touching an iſland, or viſible rock, if the water is deep enough near them.

If the tide runs ſtronger, or more obliquely, by one end of an iſland than the other, from the ſtrongeſt ſtream, and from the moſt oblique, there will be a languid current toward the other; that is, the tide, along that ſide of the iſland, will ſet longer one way than the other.

If a ſtrong ſtream of tide runs acroſs the mouth of a bay that does not reach far into the land; within that bay there will be a ſlow ſtream ſetting the contrary way. Or, if a ſtrong ſtream ſets directly, or nearly ſo, along the extremity of a point, or promontory, that ſtretches ſtrait out from the coaſt, between this ſtream (before it reaches the point) and the coaſt, there will be a languid current with a contrary direction. By attending to this, one veſſel may keep her courſe, or gain a port, while another is carried away with the tide.

If a ſmall iſland lies thwart a tide-way, that part of the ſtream which runs along one end of it, will join what runs along the other, at ſome diſtance beyond the iſland, incloſing between them a curved ſpace, within which there will either be no ſenſible current, or a ſlow one, contrary to the other ſtreams. The counter current, in the middle of this almoſt ſtagnant ſpace, or eddy, when it gets near the iſland, ſplits in two; one branch of which runs towards one extremity of the iſland, the other towards it's other extremity; where meeting the ſtronger direct ſtreams that form the eddy, are by them again carried towards it's *vertex*.

Theſe eddies may be of great ſervice to ſhips or boats, by ſheltering them from a rapid ſtream, or even carrying them againſt it; or may enable them to croſs it with more advantage, according to the different places to which they are bound. The oppoſition of the contrary tides bounding the eddy, makes that part of the ſea rougher in blowing weather, and of a darker colour in calms, than the reſt, by which the limits and direction of theſe eddies are always diſtinguiſhed.

The colliſion of the oppoſite and oblique ſtreams, near the ends of the iſland, will excite a circular motion in the water, and, if the celerity of the tide is conſiderable, will occaſion whirlpools, or cavities in the ſea, in form of an inverted bell, wide at the mouth, or at the ſurface of the ſea, and growing gradually narrower toward the bottom: their width and depth are in proportion to the rapidity of the ſtreams that cauſe them, and are ſometimes ſo large as to be dangerous. Thoſe in *Petland Firth*, near the iſlands *Stroma* and *Swona*, may, with ſpring-tide, turn any veſſel quite round, but are never ſo large as to endanger one otherways: there have been inſtances, however, of ſmall boats dropping into, and being ſwallowed up by them. The *hiatus*, or cavity, is largeſt when it is firſt formed, and is carried along with the ſtream, diminiſhing gradually in dimenſions as it goes, until it quite diſappears. The ſuction, or ſpiral motion communicated to the water, does not ſeem to extend far beyond the *hiatus*. I paſſed, in a boat, within 20 yards

yards of one, without being ſenſible of any attraction; but indeed it was toward the latter end of the tide, when it's ſtrength was much abated: the diameter of the cavity, at that time, I judged to be between 2 and 3 feet. When fiſhermen are aware of their approach toward a whirlpool, or well, as it is called in *Orkney*, and have time to throw an oar, or any other bulky body into it before they are too near, the ſpiral motion is interrupted, and the continuity of the water broke; which, ruſhing in on all ſides, immediately fills up the cavity, and enables them to go over it ſafe. Hence in blowing weather, or when there is a breaking ſea, tho' there may be a circular motion in the water, there can be no cavity.

When there is a ſteep ſunk rock near the concourſe of ſuch ſtrong tides, and not very deep below the ſurface, a moſt amazing *phænomenon* will happen: for, the ſtream being interrupted in it's courſe, and falling ſuddenly over the rock, is reflected from the bottom upwards, ſwelling and bubbling on the ſurface like boiling water, and carrying ſand, ſhells, fiſhes, or other looſe bodies along with it; which, with boats, or whatever elſe is near, are driven with great force from the center all around toward the circumference, upon which, a gyration of the water enſuing, a whirlpool begins, which is carried along with the ſtream, as was ſaid above, leſſening gradually till it is quite extinguiſhed: in a little time a new eruption and ebullition, like the former, begins, which proceeds in the ſame manner, till the ſwiftneſs of the ſtream abates, or the tide riſes or falls too much above the rock.

Queries concerning the tides in a large ocean; which, if reſolved from obſervation, would render the theory more perfect.

1. Since the attraction of the moon raiſes the water directly below her, by diminiſhing it's gravity toward the earth's center, and, at that very ſame time, depreſſes it at a quadrant's diſtance, by augmenting the gravity there, ſo that the ſuperior altitude of one part of the ocean is immediately balanced by the ſuperior gravitation of another; do not, therefore, the tides in the ocean riſe and fall without any progreſſive motion, or ſenſible velocity? And do not all currents, or ſtreams of tide (not cauſed by winds) proceed from the interruption which land, or ſhoals, give to the undulatory motion which muſt accompany the perpendicular aſcent or deſcent of the fluids?

2. Is it agreeable to obſervation, that the power of the ſun and moon together, raiſes the tides within the tropics about 14 feet, as *Newton*, *Halley*, and *Maclaurin* ſuppoſe? And how high are the tides found to riſe in parts of the ocean of a greater latitude? If the water does not riſe and fall ſo much within the tropics, as in places more diſtant from the equator, what hinders the greater power to have a greater effect? For the moon muſt act with greateſt force on thoſe parts to which ſhe is vertical.

3. If the times of high and low water depend on the moon's appulſe to the meridian, is it not high or low water in all parts of the ocean, under the ſame meridian, about the ſame time? And is the difference

of

of the times, in places under different meridians, in any certain proportion to their difference of longitude?

4. Since the power of the moon to raiſe the tide in any place is greateſt when ſhe is neareſt the zenith, it is agreeable both to obſervation and theory, that the water riſes and falls more when ſhe is above, than when below the horizons of places on the ſame ſide of the equator with her; and the contrary: are not the tides alſo of longer duration in that caſe? Since a greater portion of the hemiſpheroid, into which the ſea is formed by the moon's attraction, is then above the horizons of theſe places, than is below them. If this is found to be fact, it will alſo be found, that the duration in different places (other things alike) will be in ſome meaſure proportional to their latitudes, and the declination of the moon.

5. In an oblique ſphere, all azimuth circles cut the equator and it's parallels obliquely; and therefore the moon muſt come ſooner to, or from, a given azimuth, with one declination than with another. In ſome latitudes this difference will amount to ſeveral hours. Is it not then a falſe rule to judge of the times of high or low water by the moon's azimuth, or to ſignify one by the other, as is the cuſtom of ſailors?

V. There are in this river, at ebbing and flowing, certain irregular motions, not to be found in any other river in *Scotland*, perhaps in *Great Britain*, or even in all *Europe*, called by the common people betwixt the villages of *Alloa* and *Culroſs*, Leakies, which name I ſhall likewiſe make uſe of, for want of a better. This leaky is this: when the river is flowing, before high water, it intermits and ebbs for a conſiderable time, after which it reſumes it's former courſe, and flows till high water; and, *vice verſa*, in the ebbing, before low water, the river flows again for ſome time, and then ebbs till low water. The leaky begins at a place called *Queen's Ferry*, ſeven miles above *Leith*, at neap tide, and low water, and goes to the houſe of *Maner*, which is about 25 miles above *Queen's Ferry*, which is to be underſtood by water; for in this river, from a little above *Alloa* to *Stirling*, there are ſuch a prodigious number of turnings and windings, that though it be but 4 miles betwixt theſe two places by land, yet it is 24 by water. This I take notice of rather, as I take theſe windings to be the cauſe of the leakies. At neap tide and high water, as alſo at ſpring-tide and low water, the leaky reaches as far as the ſea fills, which is to the groves of *Craigforth*, 19 miles above *Maner* houſe, and three above the town of *Stirling*. At *Queen's Ferry* there are no leakies at neaps and ſprings at high water, nor in the latter at low water; they begin betwixt *Burrowſtowneſs*, a village about 7 miles above *Queen's Ferry*, and the mouth of a rivulet called *Carron*, five or ſix miles further up the river than *Burrowſtowneſs*. What is very remarkable, in the very loweſt neaps the leaky, after it has ebbed for ſome time, before high water, makes up again, and will be two feet higher than the main tide. In the beginning

An irregular tide in the river of Forth; *by Mr* Edward Wright. N°. 495. p. 412. Apr. &c. 1750. *Read* May 3. 1750.

ning of the ſpring-tides, it does not riſe ſo high by a foot: at the dying of the ſtream, it is often two feet higher than the main tide, which is to be underſtood, before high water, when the leaky makes up again. At neap tide and low water it will ebb two hours, and fill as much, and at full water ebb an hour, and fill another.

It is likewiſe to be remarked, that at change of the moon, at low water, the leaky will continue two hours, the beginning of the tide for that time, which then ſtands, and does not ebb till flood (the beginning of the flowing), and at full water, will ebb and flow an hour or more.

It is obſervable, that at full moon, there are no leakies, either at high or low water, in the ſpring tides which are at that time, but in the neaps which follow them, theſe motions are obſervable, as before deſcribed; as alſo in the ſpring tides, which happen upon the change of the moon, called by the commonalty, the overloup, there are leakies both at high and low water. All this is to be underſtood, when the weather is ſeaſonable; for, otherwiſe, theſe motions are not ſo diſcernible.

This account I have collected partly from my own obſervation, having paſt a great part of my life at a country-ſeat near *Alloa*, where the leakies are to be ſeen in great perfection, and partly from what I have learned from people living on different parts of the river, whoſe obſervations, as well as my own, I find exactly agree with thoſe of a learned * relation of mine, who, near 70 years ago, diligently obſerved and inquired into the phænomena of this irregular tide.

A ſurprizing inundation in the valley of St John's *near* Keſwick *in* Cumberland, Aug. 22. 1749. *in a letter from a young clergyman to his friend; communicated by* John Lock, *Eſq; F. R. S.* N°. 494. p. 362. Jan. &c. 1750. *Read* March 15. 1749.

VI. In order to give you a diſtinct anſwer to your queries in relation to the inundation at *St John*'s, I took a ride to the place to ſatisfy myſelf of the matter of fact, becauſe the accounts which were given me were very different.

This remarkable fall of water happened at 9 in the evening, in the midſt of the moſt terrible thunder, and inceſſant lightning, ever known in that part in the memory of the oldeſt man living, the preceding afternoon having been extreme hot and ſultry. And what ſeems very uncommon, and difficult to account for, the inhabitants of the vale, of good credit, affirm to have heard a ſtrange buzzing noiſe like that of a malt-mill, or the ſound of wind in the tops of trees, for 2 hours together, before the clouds broke.

I am not ſo much a Philoſopher as to find out what could occaſion ſuch a vaſt collection of clouds or vapours, particularly at that time and place; but am ſatisfied from the havock it has made in ſo ſhort a time (for it was all over in leſs than 2 hours), that it muſt have far exceeded any thunder-ſhower that we have ever ſeen. Moſt probably it was a ſpout or large body of water, which, by the rarefaction of the air, occaſioned by that inceſſant lightning, broke all at once upon the tops

* His obſervations were communicated to Sir *Robert Sibbald*, in order to aſſiſt him in compiling a *Scotiſh* Atlas.

of

of theſe mountains, and ſo came down in a ſheet of water upon the valley below.

This little *Valley of St John's* lies E. and W. extending about 3 miles in length, and ½ a mile broad, cloſed in on the S. and N. ſides, with prodigious high, ſteep, rocky mountains: thoſe on the north ſide, called *Legburthet Fells*, had almoſt the whole of this cataract; for I do not find that any remarkable quantity of water was obſerved from thoſe on the S. notwithſtanding the diſtance from the tops on each ſide cannot be a mile. It appears likewiſe, that this vaſt ſpout did not extend above a mile in length; for it had effect only upon 4 ſmall brooks, which come trickling down from the ſides of the rocky mountains. But no perſon, that does not ſee it, can form any idea of the ruinous work occaſioned by theſe rivulets at that time, and (what ſeems almoſt incredible) in the ſpace of an hour and half. At the bottom of *Catcheety Gill*, which is the name of the greateſt, ſtood a mill and a kiln, which were intirely ſwept away, in 5′ time, and the place where they formerly ſtood, now covered with huge rocks, and rubbiſh, 3 or 4 yards deep. One of the mill-ſtones cannot be found, being covered, as is ſuppoſed, in the bottom of this heap of rubbiſh.

In the violence of the ſtorm, the mountain has tumbled ſo faſt down as to choak up the old courſe of this brook; and, what is very ſurprizing, it has forced it's way through a ſhivery rock, where it now runs in a great chaſm, 4 yards wide, and betwixt 8 and 9 deep.

In the courſe of each of theſe brooks, ſuch monſtrous ſtones, or rather rocks, and ſuch vaſt quantities of gravel and ſand, are thrown upon their little meadow-fields, as render the ſame abſolutely uſeleſs, and never to be recovered.

It would ſurpaſs all credit to give the dimenſions and weight of ſome rocks, which are not only tumbled down the ſteep parts of the mountains, but carried a conſiderable way into the fields, ſeveral thrown upon the banks larger than a team of ten horſes could move. Near a place called *Lobwath*, I had the curioſity to meaſure one carried a great way, which was 676 inches, or near 19 yards about.

The damage done to the grounds, houſes, walls, fences, highways, with the loſs of the corn and hay then upon the ground, is computed variouſly, by ſome at 1000 *l.* by others at 1500 *l.*

One of theſe brooks, which is called *Moſe* or *Moſedale Beck*, which riſes near the ſource of the others, but runs N. from the other ſide of *Legburthet Fells*, continues ſtill to be foul and muddy, having, as is ſuppoſed, worn it's chanel ſo deep in ſome part of it's courſe as to work upon ſome mineral ſubſtance, which gives it the colour of water huſhed from lead-mines, and is ſo ſtrong as to tinge the *River Derwent* (into which it empties itſelf) even at the *ſea*, near 20 miles from their meeting.

Theſe are moſt of the particulars I could collect concerning this wonderful inundation. I ſhall only add *Mr.* N.'s *Philoſophical Account to his Philoſophical Friend.*

" *Tueſday, Aug.* 22. 1749. was the beſt hay-day we had here that " ſeaſon, but at 8 at night it began to thunder, firſt W. from *Cocker-* " *mouth*, then in a few minutes E. from *Penrith*. Theſe thunder- " clouds, with equal force, and contrary direction, met together upon " the mountains above the valleys of *St John's* and *Threlkeld*, as at or " about the *Great Dod* and *Cova Pike*, and muſt of conſequence hover " on or about them, and thereon vent *water-ſpouts* (but not ſo on the " valleys, otherwiſe than by the violent courſe of the brooks and rivu- " lets, from the one down to the other); which would increaſe and " perpetuate the lightning, ſo ſwift in motion, and viſible to our eyes, " but retard and obſtruct the undulations of the air, which are far more " ſlow in motion, and later in coming to our ears.—For any two ſuch " bodies as thick clouds, driven by contrary winds, and meeting to- " gether with equal force, and contrary directions, cannot impel each " other backwards or forwards, but muſt remain at or about the place " where they met, and there exert their vigour: which, in this caſe, " muſt be the reaſon of ſuch water-ſpouts upon theſe mountains, and " not in the valleys; and alſo why the ſight of the the lightning was " more terrible to our eyes than the ſound of the thunder to our ears.— " Like to this is the caſe of *whirl puffs*."

A burning well; *by the Rev. Mr* Maſon, Woodw. Prof. Cantab. *& F. R. S.* N°. 482. p. 371. Jan. &c. 1746-7. *Read* Jan. 22. 1746-7.

VII. At *Broſeley* in *Shropſhire*, in 1711, was a well found, which burned with great violence, whereof ſome account is given in *Philoſ. Tranſ.* N°. 334; but it has been many years loſt. The poor man, in whoſe land it was, miſſing the profit he uſed to have by ſhewing it, applied his utmoſt endeavours to recover it; but all in vain, till *May* laſt; when, attending to a rumbling noiſe under the ground, like what the former well made, though in a lower ſituation, and about 30 yards nearer to the river, he happened to hit upon it again.

The well for 4 or 5 feet deep is 6 or 7 feet wide; within that is another leſs hole, of like depth, dug in the clay; in the bottom whereof is placed a cylindric earthen veſſel, of about 4 or 5 inches diameter at the mouth, having the bottom taken off, and the ſides well fixed in the clay rammed cloſe about it. Within the pot is a brown water, thick as puddle, continually forced up with a violent motion, beyond that of boiling water, and a rumbling hollow noiſe, riſing and falling by fits 5 or 6 inches; but there was no appearance of any vapour riſing; which perhaps might have been viſible, had not the ſun ſhone ſo bright.

Upon putting down a candle at the end of a ſtick, at about a quarter of a yard diſtance, it took fire, darting and flaſhing in a violent manner, for about half a yard high, much in the manner of ſpirits in a lamp, but with a greater agitation. The man ſaid, that a tea-kettle had been

been made to boil in 9′ time; and that he had left it burning 48 hours together, without any ſenſible diminution.

It was extinguiſhed by putting a wet mop upon it, which muſt be kept there a ſmall time; otherwiſe it would not go out. Upon the removal of the mop, there ſucceeded a ſulphureous ſmoke, laſting about a minute; and yet the water was very cold to the touch.

The well lies about 30 yards from the *Severn*; which, in that place, and for ſome miles above and below, runs in a vale full 100 yards perpendicular below the level of the country on either ſide, which inclines down to the country at an angle of 20 or 30° from the horizon; but ſomewhat more or leſs in different places, according as the place is more or leſs rocky.

The country conſiſts of rock, ſtone, earth, and clay, unequally mixed; and as the river, which is very rapid, waſhes away the ſoft and looſe parts, the next ſucceſſively ſlip into the chanel; ſo as, by degrees, and in time, to affect the whole ſlope of the land: and as the inferior *ſtrata* yield coal and iron-ore, their fermentation may produce this vapour, and force it to aſcend with violence through the chinks of the earth, and give the water the great motion it has. This might be obſtructed in one place by the forementioned ſubſiding of the ſloping bank, and might afterwards find a vent in another; in like manner as it happened at *Scarborough Spaw*, a few years ſince.

CHAP. III.

MINERALOGY.

I. IN the night-time, between the 24th and 25 of *June* laſt, a violent ſtorm of thunder and lightning happened at the city of *Norwich*, and the places adjacent; though at the city of *Norwich* it ſeemed extraordinary only for the loudneſs of it's claps, and the length of ſeveral of the flaſhes; ſome whereof continued near half a minute, and were ſo extremely bright, that they cauſed ſome thin deal ſhutters to the windows of my bed-room (which then happened to be unpainted) to appear almoſt quite tranſparent.

Some account of the ſinking down of a piece of ground, at Horſeford, *in* Norfolk; *communicated by Mr* Arderon, *of* Norwich, *to Mr* H. Baker, *F. R. S.* N°. 477. p. 527. Aug. &c. 1745. *Read* Nov. 14. 1745.

But at *Horſeford*, a ſmall country village, about four miles N. W. of this city, a remarkable *phænomenon* appeared the next day, the like whereof has not been obſerved in this county, ſince that communicated to the *R. S.* by Mr *P. Le Neve*.

A ſudden *Lapſus*, or ſinking down of the earth, happened at this village, in the night above-mentioned, and left a hole 12 feet deep, and 12 $\frac{1}{2}$ in diameter, in form almoſt exactly round. It's ſides are nearly perpendicular;

pendicular; and what ſeems moſt ſtrange, no ruffles, cracks, or chaſms, are to be found nigh it, but the ground appears intirely firm and ſolid; and for miles about is a fine champaign country, of a dry ſandy ſoil, but not hilly; neither is there any watercourſe above ground near it.

The firſt yard from the ſurface downwards is corn-mould earth; the other three are compoſed of brown and yellow ſand, diſpoſed in ſeveral different *ſtrata*.

I ſhall not pretend to account for this accident; but might it not poſſibly be occaſioned by ſome ſubterraneous current waſhing away the ſandy matter by little and little, until it had left only a cruſt, which the *tremor* of this terrible thunder had thrown down from the very ſurface; though on this conjecture, one would expect ſome overflow or appearance of water; whereas I could not perceive here the remains of a ſingle drop.

A letter from Mr J Durant *to the Hon.* R. Boyle, *Eſq*; *F. R. S. concerning a* coal-mine *taking fire near* Newcaſtle *upon* Tyne; *of the* blue well; *and of a* ſubterraneous cavern *in* Weredale; *communicated by Dr* Miles. N°. 480. p. 221. May & June 1746. *dated* Newcaſtle, Feb 9. 1673-4. *Read* June 5. 1746.

II. The fire at firſt was occaſioned by a candle, negligently placed by a pitman, as he was working in a pit about 30 years ago. So ſmall it was at firſt noticing, that half a crown was denied reward to one, who for that price would have engaged to have extinguiſhed it: now it has waſted land and mine, and grown ſo furious, as no hopes of it's ceaſing are conceived, before the failure of it's fuel.

The grounds where it began belong to a village called *Benwell*, about $\frac{1}{4}$ of a mile N. from the river *Tyne*; whence, by a ſlow progreſs, and frequent deviations E. and W. it marched N. ſometimes preying on the coals nearer the ſurface of the earth, and then ſubverting houſes and grounds lying over it; ſometimes on the deeper mines, and was conſpicuous only by it's ſmoak and fire in the night. Now it rages, and has already cauſed great devaſtation, in grounds belonging to a village called *Fenham*, near a mile N. from the place where it firſt was kindled.

It's eruptions at preſent are in many places, and various depths. I have, both laſt winter and this, in froſty nights (for then it burns moſt furiouſly) occaſionally riding by, in near 20 places, ſeen it's flames and pillars of ſmoak. That ever it has ejected ſtones, or the like, I cannot, by information or obſervation, affirm; the concreted ſalts we have from it being always found either candying the ſupercreſcent furze, or impacted in the ſurface of the earth, at it's eruptions.

If you ſhall command it, I may be able to give you ſome account alſo of a ſtream near this town, which, on it's banks, in the ſummer-time, as alſo, being evaporated over the fire, leaves behind it a blue powder. It's head is thence called by neighbouring inhabitants, *The Blue Well*; as alſo, of ſome ſubterraneal grottoes or caverns in *Weredale*, about 20 miles S. W. of this place; where, by a little hole creeping into the ſide of a vaſt mountain, is entered a ſpacious cavity, chambered with walls and pillars of decident lapideſcent waters; the hollowneſs in ſome places being pervious further than any yet has adventured to diſcover;

cover; the darkneſs of theſe caverns requiring the help of candles, which are often extinguiſhed by the dropping water.

III. 1. *July* 19. 1746. I had the pleaſure of ſeeing thoſe ſtupendous and amazing precipices which bound our N. E. ſea-coaſt; and rode ſome miles betwixt them and the ſea. I can aſſure you, I was highly delighted with viewing them; and, as Sir *Richard Steele* ſays, by the deſcription of *Dover Cliffs*, Whoever looks upon theſe precipices, and is not moved with terror, muſt either have a very *good* head, or a very *bad one*.

Extract of a letter from Mr W Arderon, *F. R. S. to Mr* H. Baker, *F. R. S. containing obſervations on the* precipices *or* cliffs *on the N. E. ſea-coaſt of the county of* Norfolk. N°. 481. p. 275. Oct. &c. 1746. *Read* Nov. 20. 1746.

Theſe dreadful heights are equally dangerous to come nigh, above or below; as they are ſo frequently tumbling down, and as often waſhed away by the raging billows: and although they are 20, 30, and in ſome places 40 yards and upwards in perpendicular altitude, yet I am credibly informed the ſea has got of the land at leaſt 110 yards in leſs than 20 years time for ſome miles on this coaſt.

The various *ſtrata*, which make up this long chain of mountainous cliffs, muſt be greatly entertaining to every one, who takes a pleaſure in looking into the many changes, which the earth undoubtedly has undergone ſince it's firſt creation.

Vegetable mould, oaz, ſands of various kinds and colours, clays, loams, flints, marles, chalk, pebbles, *&c.* are here to be ſeen at one view beautifully interſperſed; and frequently the ſame kind many times repeated; as if at one time dry land had been the ſurface; then the ſea; after, moraſſy ground; then the ſea, and ſo on, till theſe cliffs were raiſed to the height we now find them.

What makes this come up almoſt to demonſtration, are the (*a*) roots and trunks of trees, which are to be ſeen at low water in ſeveral places on this coaſt near *Haſborough* and *Walket*.

With reſpect to the tooth I ſent you ſome time ago, I could trace nothing more out than what I have before informed you; but, that bones of animals are often found here is indiſputably true; and I have now by me another (*b*) tooth of an elephant found betwixt *Munſley* and *Harborough*, which (*c*) I ſhall forbear to deſcribe to you, as I deſign it to wait upon you with ſome other foſſils in a few days.

That the reſt of the bones of theſe animals are not preſerved ſo commonly as the teeth, I am informed is their prodigious bulk and weight; which are ſo great, that the country people thereabouts have never

(*a*) Dr *Hook*, in his Poſthumous Works ſays, the like are to be ſeen on the coaſts of *Cumberland* and *Pembrokeſhire*.

(*b*) *Richard Verſtegan* ſays, nigh *Bruxelles* in *Flanders* were found the bones of an elephant, the head of which he had ſeen. *Verſt.* p. 115.

(*c*) " Only, that as this tooth is much more decayed than that I before ſent you, and " hath ſeveral pieces broken off it, yet it weighs 10 $\frac{1}{4}$ pounds; and would have weighed " as much or more than that which Mr *Tentzelius* deſcribes in the *Philoſ. Tranſ.* N° 234. " found near *Erfurt* in *Germany*. This, I think he ſays, was the biggeſt ever found in " *Europe*."

thought

thought it worth their while to ſave them from the fury of the tempeſtuous waves.

In peruſing Dr *Hook*'s Poſthumous Works, *p.* 313. in his Diſcourſe on Earthquakes, I there find, that Dr *Thomas Brown* of this city preſented the *R. S.* with a petrified bone, found at *Winterton*, a little country town on this coaſt, in the year 1666; but am far from being of (*d*) Dr *Brown*'s opinion, to take them all for bones of ſea animals, much leſs of the whale kind, which are found here.

Amongſt the many *ſtrata*, which I took notice of in theſe cliffs, there is one of a dark-grey colour, which ſweats out a yellow ſulphureous matter: I take it to be that ſort of earth from which vitriol is made; but this is of ſuch a cauſtic nature, that, if but a ſmall piece of it be held to the tongue, in a moment it cauſeth as ſharp and excruciating a pain, as if a red-hot iron had been holden to it.

An account of the ſtrata *of* ſhells, *and other* foſſils, *found at* Cantley White-Houſe *in* Norfolk. Ibid. p. 279.

2. After giving you a ſhort account of what I thought worth my notice on our N. E. ſea-coaſt, I ſhall here lay before you what I have obſerved in ſome marl-pits at a place called *Cantley White-Houſe*, about 3 miles from *Norwich*, almoſt S. E. and adjacent to the country ſeat of the honourable *Thomas Vere*, Eſq; (*e*).

Theſe marl or rather chalk-pits are made in the ſide of a long chain of hills, which runs along the ſide of the river *Yar*, and about a furlong or two now-and-then diſtant from it.

Theſe hills I take to have been formerly the boundaries to an arm of the ſea, which made *Norwich* a famous ſea-port. This ſome of our (*f*) antient hiſtories make mention of as an undoubted truth, tho' now looked upon as a mere fable, as no footſteps of it remain above-ground at this day.

In the above-mentioned marl-pits I have lately diſcovered a (*g*) *ſtratum* of ſhells, of about 2 feet thick, running nearly parallel to the horizon, and I believe nigh level with many parts of the ground in *Norwich*. This ſeems to put the matter out of all diſpute, and fairly confirm our antient hiſtory (*h*).

(*d*) "Whoever will take the pleaſure to read Mr *Blair*'s deſcription of the ſkeleton of "the elephant, now at *Dundee* in *Scotland*, inſerted in *Phil. Tranſ.* N°. 326. will find "theſe teeth before-mentioned to be the real teeth of elephants, whatever Dr *Brown*'s "may be: and though I have never ſeen that bone he preſented to the *Society*, yet I "imagine it to be a bone from the ſame kind of animal."

(*e*) *Verſtegan* ſays, that many places which were ſea became dry land, at the breaking of the *German* ocean through the *Iſthmus* which once joined *England* to *France*. *Verſt.* p. 117.

(*f*) The Rev. Mr *Bloomfield*, in his hiſtory of *Norwich*, produceth ſeveral antient writings, which aſſert the truth of this. Page 2.

(*g*) The Rev. Mr *Thomas Lawrence*, in his *Mercurius Centralis*, is of opinion, theſe ſhells, and all others under-ground, are lodged here by ſubterraneous currents. Page 47.

(*h*) Dr *C. Leigh*, in his Natural Hiſtory of *Lancaſhire*, boldly affirms all foſſils to be the diſports or *luſus naturæ*. *Book* III. *p.* 41. and many other places.

I examined

I examined carefully this *ſtratum*, where I found a great many (*i*) kinds of ſhells, but none which had withſtood time's all-devouring teeth, ſo as to bear the handling; excepting the common wilk, ſome of which were very perfect (*k*).

Amongſt the variety of things I took notice of in this *ſtratum* was a piece of coal; which I picked out from amongſt the ſhells. This muſt have lain here as long as they, and been brought from ſome other county, as nothing of it's kind is to be found here, but what is brought from diſtant parts.

This ſeems to prove, that the uſe of coals has been known to the antient *Britons*; and that they brought them hither from the more northern parts, when the ſea covered the greateſt part of *Norfolk*: though, in all probability, this muſt have been ſome thouſands of years ago.

Theſe ſhells lie 14 yards above the ſurface of the river, and nearly 6 beneath the top of the hill; and I believe 34 yards above the ſurface of the ſea at *Yarmouth*.

I have one thing ſtill to relate to you, which is really wonderful, and very much beyond my utmoſt endeavours to find out a ſufficient reaſon for: it is, that in theſe very marl-pits, and I dare be bold to ſay, 6 or 7 yards lower than the above-mentioned *ſtratum* of ſhells, are found an unaccountable quantity of ſtags horns lying in all directions: ſeveral I took out with my own hands; and the workmen, which are employed here. tell me, that they ſcarce work a day, but they find leſs or more of them.

But with my utmoſt diligence I have not yet been able to find one whole and entire; nor do the workmen ſay they ever did; which I take to be very ſtrange.

(*i*) Common Cockle. Black muſſel. Oyſter. *Pectunculus*, *&c.*

(*k*) The ever memorable Dr *Hook*, in his Poſthumous Works, ſays, that earthquakes ſeem to be the chief efficients which have tranſported theſe petrified bodies, ſhells, woods, *&c.* and left them in ſuch parts of the earth as are no otherwiſe likely to have been the places wherein ſuch ſubſtances ſhould be produced.

" That ſeveral mountains and vallies have taken their riſe from earthquakes muſt in-
" evitably be allowed; but then they are to be found in hotter countries than this. If
" the contrary is believed, why don't ſuch things happen now? And why is all hiſtory
" ſilent upon this head? Beſides, the regularity of the *ſtrata* of ſhells, and their often
" lying parallel to the horizon for many yards, I own, puzzle me very much, and prevent
" me from acquieſcing with this ingenious man on this head."

" Dr *Woodward*'s hypotheſis, or manner of bringing theſe ſhells, and all other foſſils,
" into the places where we now find them, by a total diſſolution of matter, is indeed very
" pretty; but ſo many difficulties ariſe (however plain it might appear to him) I believe
" few now-a-days are of his opinion.

" Above all, I think Mr *Petit*'s way of thinking is liable to the feweſt objections; *viz.*
" by the variation of the paralleliſm of the earth's *axis*; which, being allowed, muſt
" certainly alter the centre of gravity: if ſo, then all the fluid parrs will conform there-
" to; and then it will follow, that one part will be covered, and overflowed by the ſea,
" that was dry before, and another be diſcovered and laid dry, that was before
" over-whelmed."

Theſe

Theſe horns (*l*) have been very large ones; ſome of the ſpines meaſuring 12 inches and upwards in length. The horns themſelves, many of them, are better than 2 ½ inches in diameter, and ſeveral of ſtem above 12 inches from ſpine to ſpine.

The entire ſkeleton of a man was found in the ſame bed or *ſtratum* with the above-mentioned horns, as one of the workmen aſſured me: he ſaid, he took the pains to lay it all together upon the graſs, as regularly as he was able; but his curioſity being then ſatisfied, he left it to be ground to pieces by the carts and waggons that came thither for the marl; ſo careleſs were theſe poor ignorant people of ſo valuable a ſpecimen of the human race! What inſtructive inferences might perhaps have been drawn from ſuch a ſkeleton, with reſpect to the magnitude of men in the early ages of the world! I own, I cannot but regret the loſs of it.

Helmet ſtones (*m*) and *belemnites* are here to be found in abundance, at all depths, and in every different *ſtratum*; which I think ſhews, that the fiſh, which produced theſe foſſils have been very plentiful: and ſo they muſt have been all over the county, as the like are to be found in every place where-ever the earth is broken open, or a pit is digged.

An account of other foſſils, *found near* Hartford-Bridge *in* Norfolk. Ibid. p. 283.

3. About a mile S. of a little country town called *Kiſick*, and near two furlongs from (*n*) *Hartford-Bridge*, is a pit, in which the country people dig a particular ſort of clay to lay upon their ſandy lands. Amongſt this clay, lie a great many knots, lumps, or nodules, of a bluer ſort of earth, not widely differing from that which is found in *Harwich* cliff: theſe, when digged up, are ſoft; but when they have been for ſome time expoſed to the open air, they become almoſt as hard as flint.

In and upon theſe lumps are the impreſſions of the *cornu Ammonis*, or ſnake-ſtones in a beautiful manner, from one inch to 5 or 6 in diameter; and ſeveral I ſaw with part of the ſhells upon them of a yellowiſh white (*o*).

Many other ſhells are to be found in theſe lumps, as the *pectunculus*, helmet-ſtones, *belemnites*, common cockle, *turbo's*, *&c.* but theſe are moſt of them very ſmall.

But ſtill more curious than all the reſt are certain lumps of petrified cryſtallized matter, of a very odd form, ſuch as I have never ſeen or ever read of.

They appear to have been originally lumps of blue clay, cracked by ſome ſubterraneous heat, or other unknown cauſe, into which the water

(*l*) "The nature of theſe horns ſeems to the eye to be entirely changed into that of "chalk; only retaining their outward form, and the poroſeneſs of their inward parts."

(*m*) "Whatever *ſtrata* theſe helmet-ſtones are found in, I obſerve they are moſtly filled "with matter of the ſame nature and conſiſtency, provided they have lain there un-"diſturbed."

(*n*) About 3 miles S. W. of *Norwich*.

(*o*) "When this clay was ſoft, I found it impoſſible to get one of them whole."

has

has inſinuated, and the ſalts contained therein have cryſtallized in the cracks.

When theſe lumps are taken up, and become dry, the clay part falls from out the exterior cells; and then they may be thought groſly to repreſent an honey-comb. At firſt I took them to be bones from within-ſide the ſkull of ſome great fiſh, or other ſea animal; but ſome which I have ſeen lately, and of which I ſhall ſend you a ſpecimen, have convinced me I was miſtaken. Indeed ſince I wrote the above, I have compared this foſſil with the deſcription Dr *Woodward* gives of the *Ludus Helmontii*, and I think it agrees ſomewhat therewith.

An account of large ſubterraneous caverns *in the chalk hills near* Norwich; *by Mr* W. Arderon, *F. R. S. in a letter from Mr* H. Baker, *F. R. S to the* Pref. N°. 486. p. 244. Feb. & March 1748. *Read* March 24. 1747-8.

IV. About a quarter of a mile from the city of *Norwich*, on the E. ſide thereof, and near the entrance of *Mouſhold-Heath*, is a large ſubterraneous cavern, which has been formed in a long ſeries of time, by the digging out of chalk for the making of lime. There's but one entrance into it, whoſe breadth is about two yards, and it's height nearly the ſame; however the height gradually riſes, till at laſt it meaſures in ſome places from 12 to 14 yards. But notwithſtanding the entrance is ſo ſmall, the whole area within is of ſuch a large extent, that 20,000 men might with great eaſe be placed therein, as I believe will ſcarcely be doubted, when I aſſure you, that, from the entrance to the furtheſt part of theſe darkſome cells, meaſures full 400 yards; and that theſe paſſages are frequently 10 or 12 yards wide, with branchings out on the ſides, into various lanes and labyrinth-kind of windings, that every now and then open into one another; which renders it no eaſy taſk to find the way out, when a perſon has been a little bewilder'd in theſe ſubterraneous mazes.

Moſt of theſe vaults are arched at top, whereby the immenſe weight, which every moment preſſes on them, is well ſupported; a weight no leſs than that of hills, whoſe perpendicular altitude above the tops of theſe arches is 20 or 30 yards, if not much more. I have frequently, ſays my correſpondent, gone into theſe caverns out of curioſity; but could never perceive the leaſt appearance of thoſe damps * which are ſo common in mines, and other ſubterraneous places, where the air is ſtagnant for want of a due current; which ſhould ſeem to be the very caſe here, as there is but one entrance into it. The paſſage indeed is horizontal, and open to the W. wind; but the included air's being free from putrefaction, may poſſibly be owing to the large quantity of ſalt which the chalk contains.

How deep or thick theſe rocks of chalk are, no one, ſo far as I can find, can tell; for, in ſinking the loweſt wells, they have never, that I know of, been dug through; and conſequently muſt be exceeding deep. The chalk at the further end of this cavern is ſo very ſoft, that it may

* Thoſe are commonly cauſed by ſulphureous vapours, which never appear in chalk. *C. M.*

be moulded with the hand like paſte; which I take to be it's original conſiſtence, and what it always retains, till it becomes expoſed to the air. In the very loweſt parts of theſe vaults I have picked up ſeveral kinds of foſſils, figured by marine bodies; ſuch as *echini*, *pectunculi*, common or fluted cockle, *belemnitæ*, *&c.* and, by diligent ſearch, other ſorts might perhaps be found. Sounds made beneath theſe arched roofs are ſtrongly reflected from ſide to ſide; ſo that the leaſt whiſper may be heard at a conſiderable diſtance. The beat of a pocket-watch was heard diſtinctly full 20 yards from where it was placed.

I viſited this place *Nov.* 1. in order to try the temperature therein, as to heat and cold; and carried with me a thermometer regulated by one of Mr *Haukſbee*'s, which I ſet down at the further end of theſe caverns; and letting it remain there for ſome time, I found the mercury reſted at 52°. which comparing with the regiſter I had kept, was, I found, within half a degree of a medium betwixt the greateſt heat and the ſharpeſt cold we have known in this city for ten years paſt; and it is very probable, if the two extremes had been taken more exactly, the temperature in theſe caverns would be found to come yet nearer to the medium of heat and cold in this climate.

	Haukſbee's Therm.
The greateſt degree of heat was *July* 18. 1746.	15
The greateſt degree of cold was *Jan.* 9. 1740.	88
Which added together make —— ——	103
The medium of which is —— ——	51½

I find, by inſpecting Mr *George Martin*'s collection, and compariſon of the ſcales and degrees of heat with various Thermometers, that the temperature of heat in theſe caverns coincides with that in the cave at the obſervatory at *Paris*, within one degree; which I think comes very near, conſidering the obſervations were made with different inſtruments, and formed upon different principles.

At the foot of a high hill, adjacent to theſe vaults, iſſues out a curious ſpring, whoſe water I found exactly of the ſame temperature with that under-ground; though, when the Thermometer was expoſed to the open air, it ſtood at 57°. *

An account of the Giants Cauſeway *in* Ireland; *in a letter to the* Preſ. *from the Rev.* Rich. Pococke, *L.L.D. archdeacon of*

V. In my laſt paſſage over to this kindom, I ſaw that very remarkable curioſity, commonly called the Giants Cauſeway: † The ſea-cliffs are very high thereabouts, and what is called the Cauſeway is a low head, extending from the foot of the cliffs into the ſea like a mole. This head does not appear at firſt ſo grand as it is repreſented in the views

* A terrible thunder-ſtorm, *June* 12. 1748. ſhook the earth to ſuch a degree as to throw down thoſe chalk-vaults.

† This Cauſeway is before taken notice of in Vol. II. Part iii. chap. 3. § lxxvi.

engraven

engraven of it; but when one comes to walk upon it, and conſider it more attentively, it appears to be a ſtupendous production of nature. The head ends in two points: I meaſured the more weſtern to the diſtance of 360 feet from the cliff, and it appeared to me to extend about 60 feet further; but this part I could not meaſure, by reaſon that the ſea was then high; and I was told, that at low tides it might be ſeen about 60 feet yet further upon a deſcent loſing itſelf in the ſea. I alſo meaſured the more eaſtern point 540 feet from the cliff, and ſaw about as much more of it as of the other, when it winds about to the eaſtward, and is alſo loſt in the water. One may walk upon this head on the tops of the pillars to the edge of the water. Theſe pillars are all of angular ſhapes from 3 ſides to 8. The eaſtern point, towards that end where it joins the rocks, terminates itſelf for ſome way in a perpendicular cliff, formed by the upright ſides of the pillars, ſome of which I meaſured, and found to be 33 feet and 4 inches in height. They ſay there are in all 74 different ſorts of figures among them. Each pillar conſiſts of ſeveral joints or ſtones lying one upon another, from 6 inches to about a foot in thickneſs: ſome of theſe joints are in the middle ſo convex, as for thoſe prominences to be nearly quarters of ſpheres, round each of which is a ledge, upon which the ſtones above them have reſted, every ſtone being concave on the under ſide, and fitting in the exacteſt manner upon that which lies next below it. The pillars are from 1 to 2 feet in diameter, and conſiſt moſt commonly of about 40 joints, moſt of which ſeparate very eaſily, tho' ſome others, which are more ſtrongly indented into each other, cohere ſtrongly enough to bear the being taken away in pairs.

Dublin, *and F. R. S.* N°. 485. p. 124. Jan. 1747-8. *dated* Dublin, Jan. 5. 1747-8. *Read* Jan. 28. 1747.

But the cauſeway is not I think the moſt ſingular part of this extraordinary curioſity; the appearance of the cliffs themſelves being yet to me more ſurpriſing; theſe and their ſeveral *ſtrata* I examined from the rocks on the other ſide of a little bay, about $\frac{1}{2}$ a mile to the E. of the cauſeway. I thence obſerved, that there runs all the way a *ſtratum* from the bottom of black ſtone, to the height, as well as I could conjecture, of about 60 feet, divided perpendicularly at unequal diſtances by ſtripes of a reddiſh ſtone, looking like cement, and about 4 or 5 inches in thickneſs. Upon this there is another *ſtratum* of the ſame black ſtone divided from it by a *ſtratum* 5 inches thick of the red. Over this another *ſtratum* of ſtone 10 feet thick divided in the ſame manner; then a *ſtratum* of the red ſtone twenty feet deep; and above that a *ſtratum* of upright pillars. Above theſe pillars lies another *ſtratum* of black ſtone 20 feet high; and above this is again another *ſtratum* of upright pillars riſing in ſome places to the top of the cliffs, in others not ſo high, and in others again above it, where they are called the Chimneys.

This face of the cliffs reaches for 2 computed miles E. from the cauſeway, that is about 3 meaſured *Engliſh* miles, to the houſe of Mr *John Stewart* 2 miles W. of *Balintoy*. The upper pillars ſeem to end over the cauſeway, and, if I miſtake not, become ſhorter and ſhorter as one

goes from it, lying between two binds of ſtone like ſeams of coal, and like thoſe little pillars found in *Derbyſhire*.

Theſe binds probably meet together all round, and incloſe this extraordinary work of nature; and if ſo, the pillars muſt be very ſhort towards the extremities.

I was led to this conjecture by the following obſervations: the lower *ſtratum* of pillars is that which goes by a deſcent into the ſea, and which makes what is called the Giants Cauſeway; and where this deſcent approaches the ſea, it ſeems probable that the pillars become ſhorter and ſhorter, ſo as to end not much further off. Now the upper bind of this *ſtratum* may have been of ſo ſoft a nature, as by degrees, in proceſs of time, to have been waſhed away by the ſea. And in the cliff over the cauſeway I ſaw ſeveral pillars lying along in a rude manner almoſt horizontally, which ſeemed to me to be ſome of the pillars of the upper *ſtratum* fallen down by the giving way of the bind that was under them, and over the lower ones that compoſe the cauſeway. And here moſt probably the upper pillars ended, as they are ſeen no farther in the cliff. I ſaw the tops of pillars even with the ſhore, both on the eaſt and weſt ſides of the cauſeway, and ſome much lower than the cauſeway itſelf; and it is probable that theſe are much ſhorter than thoſe of the cauſeway, which I meaſured above thirty feet higher than the tops of them.

When I was upon the cauſeway, I ſaw in the cliff, to the ſouth-eaſt, what they call the Organs, about a quarter of a mile off, and a third part of the way up the cliff. They appeared ſmall, and ſomewhat like a black *ſtalactites*: they were not commonly known to be ſuch pillars as the others; but they are ſo, and belong to the lower *ſtratum*. When with great difficulty I climbed up the ſteep hill to them, I found they were hexagonal, and larger pillars than moſt of the others, being about 2 feet in diameter; and I meaſured 5 ſides of one of them, which were of 13, 15, 12, 21, and 16 inches reſpectively. The joints I could come at were about 9 inches thick, and each pillar, as well as I could count, conſiſted of between 40 or 50 of them: theſe joints are almoſt flat and plain, the convexities on their upper faces being ſo ſmall as to be ſcarce diſcernible. I inquired whether any of theſe pillars were found in the quarries within land, and the people there told me they were not; but ſince I left the place, I have been aſſured by others, that there are ſome found 2 or 3 miles from the ſhore.

A moving moſs in the neighbourhood of Church-Town *in* Lancaſhire; *by the Rev. Mr* Richmond; *communicated by*

VI. On *Saturday Jan.* 26. 1744-5. a part of *Pilling Moſs*, lying between *Heſcomb* houſes and an eſtate of Mr *Buttler*'s, called *Wild Bear*, was obſerved to riſe to a ſurprizing height: after a ſhort time it ſunk as much below the level, and moved ſlowly towards the ſouth ſide: in half an hour's time it covered 20 acres of land. The improved land adjoining that part of the *Moſs* which moves is a concave circle containing near 100 acres, which is well nigh filled up with moſs and water. In ſome parts it is thought to be five yards deep. A family is driven

out

out of their dwelling-houſe, which is quite ſourrounded, and the fabric tumbling down. Mr *Buttler*, *Whitehead*, and *Stephen White*, are the firſt ſufferers by this uncommon accident. An intenſe froſt retards the regreſs of the *Moſs* to-day; but I fear it will yet ſpoil a great deal of land. The part of the *Moſs* which is ſunk like the bed of a river, runs N. and S. is above a mile in length, and near half a mile in breadth; ſo that I apprehend there will be a continual current to the S. A man was going over the *Moſs* when it began to move: as he was going eaſtward, he perceived, to his great aſtoniſhment, that the ground under his feet moved ſouthward. He turned back ſpeedily, and had the good fortune to eſcape being ſwallowed up.

Edward Milward, *M. D. F. R. S.* N°. 475. p. 282. Jan. &c. 1745. *Read* Feb. 28. 1744-5.

VII. We have few or no foſſils in this country; but a friend in *Staffordſhire* [Mr *Platt*] informs me, that that country abounds much in foſſils; ſuch as ſea-ſhells, rock-plants, and other marine bodies left at the deluge. Near *Bakewell* in *Derbyſhire* was lately found the ſkeleton of a man, with ſome ſtags horns, in digging a lead-mine.

A foſſil Skeleton *of a* Man; *by* Roger Gale, *Eſq*; *F. R. S. to Mr* Peter Collinſon, *F. R S. dated* Scruton *in* Yorkſhire. Jan. 14. 1743-4. Ibid. p. 265. *Read* Jan. 24. 1744-5.

[In the ſecond letter, dated *Scruton*, *May* 19. 1744. Mr *Gale* gives the following account of this *Skeleton* in Mr *Platt*'s own words, from a letter written to himſelf by that gentleman.]

The ſkeleton I formerly mentioned to you was found at *Lathill-dale*, near *Yolgrave* and *Bakewell* in *Derbyſhire*, as the workmen were driving a ſough, or drain to a lead-mine, about 9 yards deep from the ſurface of the earth, and about 40 fathom from the beginning of the ſough. There were found with the ſkeleton ſtags horns; two pieces of which I have now in my cuſtody; *viz.* the brow-antler, which is 9 inches long, and ſeems to have about 2 inches broke off the tip-end; the other is a piece of the large horn near the head, and is 3 inches diameter. Both the horns of the ſtag, and the rib-bones of the ſkeleton, are much decayed; and as ſoon as the head of the latter was expoſed to the air it crumbled all away, except a piece of the lower jaw; now alſo ſo imperfect as not eaſily to be diſtinguiſhed what it has been. Several of the larger teeth were taken out, which were covered with their natural enamel, and perfectly ſound. The place where theſe things were found, is on every ſide ſurrounded with a rocky petrified ſubſtance, or *terra lapidea*, by the miners called *Tuft*, ſo hard (as they ſay) as to ſtrike fire againſt their tools. This ſubſtance lay above the bones and horns a yard and half thick or more, and on either ſide; and beneath them to a breadth and depth uncertain: ſo that it appears, that the ſkeleton and horns lay in a cavity, which was not however contiguous to them, there being a ſort of ſoft coarſe clay or marl interſperſed thick with little petrified balls, or pellets of the ſame kind of ſubſtance as the tuft, for near a quarter of a yard round them; but none of the bones ſeemed in any degree to be petrified. The workmen

men conjectured there was more of the ſkeleton to be found; but they dug no further than was neceſſary to complete their ſough.

The interment of this man and ſtag ſeem to me to have been accidental, by their falling into a chaſm or wide cleft of the rock in very early times; which has ſince cloſed up, and grown over them, by the accretion of the marly ſubſtance, which environs the ſkeleton, *&c.* and in time, perhaps, will grow as hard as the tuft, and reſt of the rock.

An account of ſome human bones *incruſted with ſtone, now in the* Villa Ludoviſia *at* Rome; *communicated to the* R. S. *by the* Preſ. N°. 477. p. 557. Aug. &c. 1745. *Read* Dec. 12. 1745. Fig. 46.

VIII. Something like the body of a petrified man being mentioned by ſeveral authors, as preſerved in the *Villa Ludoviſia* at *Rome*, I thought, that a drawing of that curioſity, which I procured at *Rome* ſome years ſince, might, poſſibly deſerve the notice of the gentlemen here preſent: eſpecially, as it will appear thereby, that the ſeveral accounts hitherto given of it are not very accurate, or, at the beſt, convey but a very imperfect idea of the truth.

The following paſſage occurs in the journal-book of the *Society*, for *April* 17. 1689: "Mr *Henſhaw* related, that he had ſeen, in the *Villa* "*Ludoviſia* at *Rome*, the body of a man incruſted with a ſort of a white "marble or alabaſter caſe, ſuppoſed to have been a man frozen in the "*Alps*, and after, in long proceſs of time, this incruſtation to have "grown upon him; and that one of his arms was broken off, purpoſe-"ly to ſhew, that it was no impoſition."

Mr *Richard Laſſels*, in his travels to *Italy*, printed at *Paris* 1670. *p.* 180. tells us, that in the leſſer *Caſina*, belonging to the *Ludoviſian Villa*, he ſaw, "in a great ſquare box lined with velvet, the body of a "petrified man, that is, a man turned into ſtone; one piece of the leg "(broken off to aſſure an ambaſſador doubting of the verity of the "thing) ſhewed plainly both the bone and the ſtone cruſted over it. "The head and the other parts lie jumbled up together in the box."

F. *Athanaſius Kircher* ſays, in his *Mundus ſubterraneus*, l. viii. chap. 2. "*Spectatur et hic Romæ in horti Ludoviſiani palatio, corpus humanum* "*totum in ſaxum converſum, oſſibus adhuc integris, at lapideo cortice ob-*"*ductis.*" And in the following page he gives an imperfect ſketch of the ſame thing, under the title of "*Sceleton humani corporis in ſaxum* "*converſum, ex palatio Pinciano principis Ludoviſii.*" This ſketch, however imperfect, gives a truer idea than either his or Mr *Laſſels*'s words ſeem to convey, as there is indeed nothing like the body of a man, but only a cluſter of diſjointed bones cemented together by the ſame matter that incruſts them over. Mr *Miſſon* in his travels has more truly deſcribed them, when he ſays, that "in the ſame room they ſhew "a ſmall heap of bones, ſaid to be the ſceleton of a petrified man; "which is a miſtake, for the bones themſelves are not petrified, but "there has gathered about them a ſort of candied cruſt, or ſtony in-"cruſtation, which has made them paſs for being of real ſtone." Mr *Wright* alſo, in his late obſervations made in travelling through *Italy*, &c.

&c. has taken notice, that in the *Villa Ludoviſia* " they ſhewed ſome " bones of a human body all cruſted over with a petrified ſubſtance."

When I was at *Rome* in the year 1734, I myſelf ſaw this curioſity, which is ſtill preſerved in the ſame *Caſina* of the *Ludoviſian* gardens; and in the very ſquare box lined with velvet, that is mentioned by Mr *Laſſels*, and repreſented by Father *Kircher*: and as I had before heard it much ſpoken of, and had conceived an idea of it very different from the truth, I was willing both to preſerve a true notion of it myſelf, and to be able to give ſuch a one to others. I therefore employed an ingenious young painter to make as exact a drawing of it as he could; and I afterwards very carefully compared his drawing with the original, which is the ſame I have here to produce, hoping that the ſight of it will not be unacceptable to the company. The ſtony ſubſtance that joins the bones together is of a whitiſh colour, and the ſame as that which incruſts the bones themſelves: ſmall fractures in ſeveral places diſcover the natural bones; and the ſize of the whole maſs may be judged of, by conſidering the ſcull, which is of the common dimenſions, as a ſcale to the other parts.

IX. It was ſent me lately, from *Norwich*, by Mr *W. Arderon*. It ſeems to be a grinder belonging to the left under-jaw of a very large elephant, as it's own ſize and weight may ſhew: for the circumference, meaſured by a ſtring drawn round the edge, is 3 feet, wanting 1 inch; in length it meaſures 15 inches; in breadth, where wideſt, 7 inches, in thickneſs above 3; and it's weight is upwards of 11 pounds.

A letter from Mr H. Baker, *F. R. S. to the* Preſ. *concerning an extraordinary large* foſſil Tooth *of an* Elephant. N°. 475. p 331. Jan. &c. 1745. *Read* March 28. 1745. Fig. 47.

On one ſide it is convex, and on the other concave, with 16 ridges and furrows running on each ſide tranſverſly, and correſponding with the ſame number of eminencies on the grinding edge, which appears furrowed like a mill-ſtone. On the bottom of the part that lay within the gum are ſeveral cavities for the inſertion of the nerves. The whole tooth is almoſt intire, and ſeems very little, if at all, petrefied; but, ſince it's being expoſed to the air, ſeveral little cracks appear. Other monſtrous bones were found with it, as I am informed; and particularly thigh-bones, 6 feet long, and as thick as the thigh of a man; all which belonged probably to the ſame animal, and may be conſidered as farther proofs of the creature's enormous ſize.

The place where, and the manner how, theſe bones were diſcovered, are particulars ſo well deſerving conſideration, that I ſhall make no apology for relating them.

A little town, called *Munſley*, is ſituated cloſe to the ſea-ſhore, on the N. E. coaſt of the county of *Norfolk*, where the ſea is bounded by exceeding high rocky cliffs: ſome whereof being gradually undermined by the continual daſhing of the waves when the tide comes in, great pieces frequently tumble down upon the ſhore: and by the tumbling down of one of theſe the above mentioned bones and grinder were diſcovered.

Here

Here therefore can be little reaſon for imagining (as I know ſome have done, when ſuch-like bones have been found in more inland counties), that the *Romans* brought elephants over hither; which, when dead, they buried deep in the earth, to prevent their becoming offenſive: for they could never think of burying ſuch a carcaſe in a rocky cliff, cloſe to, or perhaps over-hanging, the ſea. But, on the contrary, this diſcovery ſeems a convincing demonſtration, that the earth has undergone ſome very extraordinary alterations: for the remains of animals, of quite different climates and regions, and of kinds, which, in the preſent ſituation of the world, could never poſſibly come over hither, muſt either imply their having been placed here by Providence, originally, or, that this iſland muſt, heretofore, have been contiguous to the Continent: but, ſince we find theſe creatures in very hot countries only, it is highly probable they were never placed here by Providence; unleſs we can ſuppoſe the temperature of our climate, as to heat and cold, to have been greatly altered: and, without ſuch a ſuppoſition, it would be no leſs unreaſonable to imagine they would wander hither from warmer regions, though even all the quarters of the globe ſhould have been contiguous.

What changes have happened to our earth, and how they have been produced, no human wiſdom can poſſibly find out with any certainty: but ſuppoſe only the polar points, or *axis* thereof, to have been ſhifted at any time but a few degrees, and it's centre of gravity to have been altered (which ſome great men have imagined not improbable), what convulſions in nature, what an univerſal change in the face of things, muſt thereby have been occaſioned! What inundations, or deluges of water, bearing every thing before them! What breaches in the earth, what hurricanes and tempeſts, muſt have attended ſuch an event! For the waters muſt have been rolled along, till, by them, an equipoiſe was produced. In ſhort, all parts of the world would thereby acquire different degrees of heat and cold from what they had before. Seas would be formed where continents had been: continents would be torn in ſunder, or perhaps ſplit into iſlands. The antient bed of the ſea would be changed into dry land, and appear covered at firſt with ſhells, and other marine bodies; of which the action and nitrous ſalts of the air would, in a few years, moulder away and turn to duſt thoſe upon the ſurface; but ſuch as were buried deep would be preſerved and remain for many ages.

Such would probably have been the fate of inanimate things: and as to living creatures, they muſt have been almoſt univerſally deſtroyed and buried in the ruins of the world, as perhaps this elephant may have been. Some few, however, would in all likelihood eſcape, either by ſwimming to, or being left on, riſing lands; where, if they met with proper food, and an agreeable climate, they would continue and increaſe, or otherwiſe would wander till they found ſuch a country, unleſs prevented by interpoſing ſeas, or impaſſable rivers.

All

All this indeed is barely conjecture: but the bones and teeth of fiſhes, the multitudes of ſea-ſhells (ſome whereof are petrefied, and others not), and the many ſea-productions found buried in the earth in almoſt every country, at vaſt diſtances from the ſea, and even in the midland parts, are demonſtrations of the ſurprizing alterations that muſt have happened as to the diſpoſition of ſea and land. The horns of the great mouſe-deer, dug frequently out of the bogs in *Ireland*, and ſometimes in *England*, the bones and teeth of elephants found there, and this preſent diſcovery, together with ſome others of the like kind that have been made in *England*, ſeem to prove, that ſuch animals formerly inhabited theſe countries, notwithſtanding the mouſe-deer is known at preſent only in *America*, and elephants are not found except in *Africa* and *Aſia*.

Part of the horn and palm of a deer, found in a chalk-pit, at a village called *Baber*, 4 miles E. of *Norwich*, at the depth of 16 feet, and almoſt converted into a chalky ſubſtance, being of a kind of which, I am told, we have none in this iſland, I alſo lay before you, as another proof to the ſame purpoſe.

X. The head and horns, repreſented in *Fig.* 48. were found in a ſand-bed, in the river *Rye*, which runs into the *Derwent*, in the eaſt-riding, belonging to *Ralph Crathorn*, Eſq; They were diſcovered as he was fiſhing for ſalmon; the net happening to hang on one or two of the antlers. He ordered to pull away; by which ſome of the antlers were broke off, and diſcovered it to be part of a deer's horn. At length, with ſome difficulty, it was dug out pretty intire. Mr *Crathorn* ſuppoſes, that theſe wild moors were once inhabited by this kind of deer, not any ſuch now being known to be in this kingdom; and ſuppoſes it is, at leaſt, 7 or 800 years ſince it's death; and that by age or poverty deſtroyed, and by time buried in thoſe ſands. It is about 3 years ſince it was found (as the aboveſaid worthy gentleman told me) where he lives; which is at *Neſs* near *Malton* in *Yorkſhire*.

An account of two extraordinary Deers Horns, *found under-ground in different parts of* Yorkſhire; *in a letter from Mr* Tho. Knowlton, *to Mr* Mark Cateſby, *F. R. S.* N°. 479. p. 124. Mar. & Apr. 1746. *Read* March 13. 1745-6. Fig. 48.

a is 12 inches long. *b* the ſame. *c* the ſame. *d* is 4 inches from the main horn, and the 2 croſs branches are 8. *e* is 6 inches. *f* is 7 inches. *g* is 6 inches; and 2 feet 10 inches from the root of the horn to the tip.

All thoſe places with marks were broke, and put together again.

	St.	lb.
This ſkull and horns weigh ——	4	12

It was found in a peat-moſs, at *Cowthorp* near *North Dreighton* in *Yorkſhire*, in the year 1744.

	Feet	Inch.
The length of the ſkull, from the noſe-end marked *A*, to the back-part of the head *B*,	1	10
The breadth of the forehead, from *C* to *C*,	0	11
Length of each horn, from the ſkull *D* to the tip,	5	1

	Fect	Inch.
The extent of the horns, from *E* to *E*	6	1
The breadth of the web or palm, from *FG* to *F*,	2	1

FG and *G*, two places where the horns are broke.

1 The noſtrils.

2 The eye.

3 The teeth, which are very large and ſound.

N. B. It is evident the horns are not at their full growth, being yet covered with what is called the Velvet.

The figure above is the repreſentation and extraordinary dimenſions of the ſkull and horns of a deer, dug from the depth of 6 feet out of a peat-moſs, as above mentioned.

But what I think more extraordinary is, that the late Earl of *Carliſle*'s ſteward, Mr *Joice*, in digging the foundation of an houſe and cellars, found, at the depth of 6 feet, a part of a jaw-bone with teeth, and a horn of a buck or ſtag, of moſt exceeding large dimenſions, which lay buried under 2 feet common ſoil; then one foot of ſcalping or ſand-bed; then 18 inches of ſtone; then another vein of ſand, 6 inches; then another head of ſtone; under which lay thoſe before-mentioned jaw-bone, and piece of horn; which, in all appearance, to every one that viewed theſe *ſtratums*, had never been removed.

Dimenſions of the Deers Horns in the Muſæum *of the* Royal Society.

	Feet	Inch.
Length of the ſkull	1	4
Breadth of the forehead	0	9
Length of each horn	5	0
Diſtance of the extreme tips of the horns	6	0

N. B. Theſe horns (*Fig.* 49.) are evidently of the ſame ſort as thoſe often found in *Ireland*, of which deſcriptions are given in *Tranſact. n.* 227. *n.* 394. and *n.* 444. *p.* 389. But I do not remember to have met with any before of this ſpecies found in *England*, or any-where elſe beſides *Ireland*. *C. M.*

An inquiry into the Lapis Oſteocolla; *by* Ambroſe Beurer *of* Nuremberg; *communicated by Mr* Peter Collinſon, *F. R. S.* No. 476. p. 373. Apr. &c.

XI. The *lapis oſteocolla* is diſtinguiſhed by ſeveral names; it is however generally called *Oſtiocolla*, or *Oſteocolla*, a name compounded of 2 Greek words, ὀς-τέον, *a bone*, and κόλλα, *glue*: it is otherwiſe called *Lapis oſtites*, *olloſteos*, *oſſina*, *oſſiſana*, *oſſifraga*, *lapis Aſiaticus*, *lapis Morochius*, *flores arenae*, *foſſile arboreſcens*, *lapis ſabilis*, and *lapis arenoſus*. It is found in ſeveral parts of *Germany*: but our anceſtors had no juſt knowledge of it; for ſome took it to be petrified bones, and others believed it to be a ſort of *gypſum*.

The ſoil wherein it is produced, is always ſandy and barren; and ſeldom produces any ſort of plant, except the poplar: but none of them, as

Fig. 46.

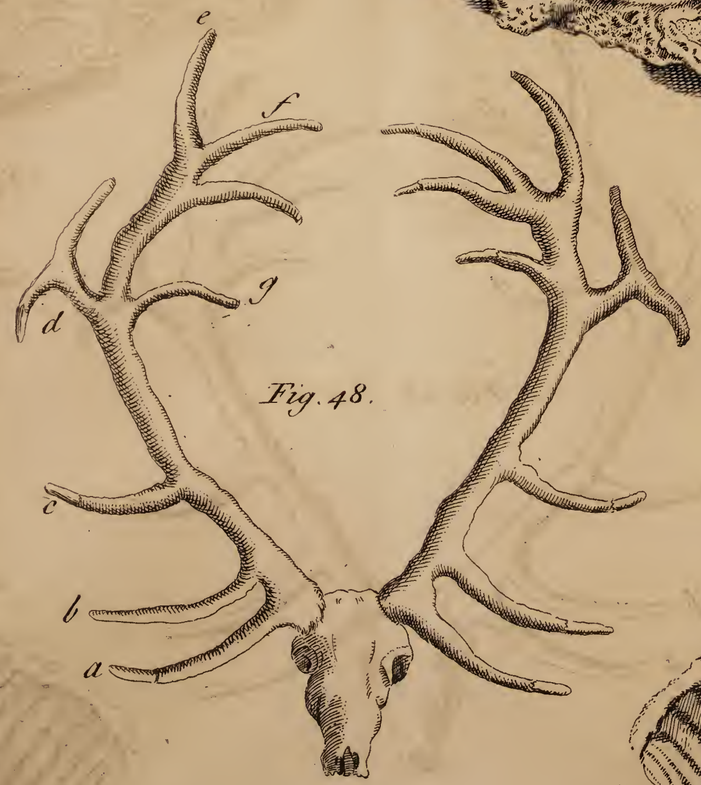
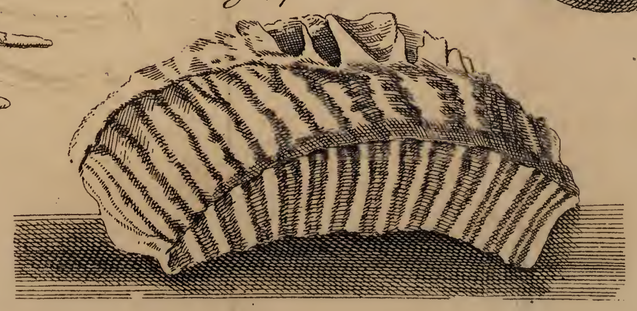


Fig. 48.

Fig. 49.

Fig. 47.



as ſome have pretended, are ever found in vallies, or ſprings, or in clay. 1745. *Read* May 9. 1745.

Kræuterman has mentioned one that had the figure of a ſort of cheſt: but it ſeems rather to have been a *tophus*, than *oſteocolla*. *Mercatus* alſo had no juſt knowledge of this ſubſtance, when he called ſome petrefactions, and *tophi calcarii* by that name, which in the opinion of *Herman* are rather *bolaria* or *ciſti*.

As to it's origin, it is produced in the ſandy ſoil mentioned above, at the depth of ſome feet, and has the form of a root. The largeſt can hardly be compaſſed by both hands, but the others are gradually leſs. As to it's conſiſtence, the *oſteocolla*, whilſt under ground, is never hard, but always ſoft and muddy, ſo that it ſeems like ſuet or fat to the touch, but when dry it is white, like any calcarious ſubſtance. As it is found under ground, it is partly grey, partly yellow or white, and the ſand adheres copiouſly to it's outſide. It's ſoft conſiſtence will not permit it to be taken out whole, ſo as to ſhew the true form of a root, unleſs the work is carried on with great art, and ſome weeks, or even months are beſtowed upon it: for if it is taken up haſtily out of the ſand, it will break: wherefore the following rules are to be obſerved:

1. It muſt be ſought for with labour and patience. 2. The ſand muſt be gently wiped off. 3. It muſt be taken far from the root. 4. An examination muſt be made, whether there are any ſmall ſecondary roots ſhooting forth, of which great care muſt be taken, that they are not loſt. 5. Several muſt not be taken at the ſame time; but liberty ſhould be given them to grow dry and hard. 6. The pit ſhould be covered with boards, to keep off any rain that may happen to fall. 7. The digging muſt not be repeated, till thoſe which are already expoſed, are grown ſufficiently hard. 8. In warm, dry, and clear weather, the planks ſhould be removed, that they may be the ſooner dry. 9. The digging ſhould be begun at a good diſtance, as water is generally found underneath.

Many authors have obſerved, that *oſteocolla* is hollow within; but they do not agree, whether it is to be referred to the vegetable or mineral kingdom. Many of the Ancients thought they were bones tranſformed; but many denied it, becauſe there are not found any perfect fragments of bones, nor is any trace of animal parts diſcovered by Chemiſtry. *Eraſmus* wrote very probably concerning it: thoſe who would not acknowledge it to be bones, have ſuppoſed it to be a mineral naturally generated from the ſand. Prof. *Teichmeyer* calls it a marle. The ſkilful Metalliſt *Henckelius* refers it to minerals; but is ſilent as to it's generation. Prof. *Juncker* will have it to be generated in the ſand, but does not add, whether it proceeds from the trunks or roots of trees. For my part I think it is a root, to which the ſand adheres, and that it is thus gradually generated. And tho' there is a great quantity of *oſteocolla* found, yet there is never any wood, at leaſt ſuch as is green, found ſtanding above ground; and tho' I made a very exact inquiry into the

origin of this *oſteocolla*, and of it's tree, the root of which had degenerated into *oſteocolla*; yet it was a long time before I diſcovered a dry bough, and green twig adhering to a tree, which ſtill remained woody in the upper part, and was at the ſame time changed into mere *oſteocolla* in the lower part, and upon a more ſcrupulous examination appeared to be a ſort of poplar. It's origin therefore is to be ſought in the black poplar, on the roots of which, when the tree is cut down, the *oſteocolla* is formed.

In all the parts of *oſteocolla*, ſomething ligneous is found, tho' rotten, and the decay of the wood leaves them hollow; ſo that they have the reſemblance of bones. But I never found any *oſteocolla* on the trees near them, and growing in the ſame ſoil: whereas if the cauſe of it's production was in the ſoil, it would be found on pines, birches, and other trees.

Hence we may learn, 1. That *oſteocolla* is hardly ever found in *ſaline* places. 2. That it is probable, that there have always been poplars, where *oſteocolla* is found. 3. That all the diggers up of *oſteocolla* may ſee that it has been a root. 4. That another *criterion* may be made, that when any *oſteocolla* is found, ſome calcarious bones are prominent, which ſeem to expreſs a flower: whence many have fancied, that it grows and flowers.

The *oſteocolla* therefore is always under the ſand, but never in any place but where it's roots had been, in adhering to which it has gradually grown hard, and ſo far as it has appeared above ground become white; and if any difference is found, it muſt be imputed to accident. When the root is once found, if you dig a ſpan deeper, you will certainly find *oſteocolla*. So long therefore as it remains under ground, it is ſoft, or about the conſiſtence of macerated lime mixt with ſand; but as the moiſture is evaporated, it becomes gradually harder.

To the generation of it therefore is required, 1. The root of a poplar. 2. If the root of a poplar cannot be ſeen, yet by diſtillation it's vegetable ſubſtance may be demonſtrated in the empyreumatic oil 3. The acid of ſalt cloſely united with ſand conduces much to it's generation. 4. that, as we know by Chemiſtry, that there always remains ſomething of an acid in ſand, and conſequently ſomething lapideſcent; ſo by the force of theſe, the acid of ſalt with much moiſture of the ſand conſtitutes a ſubſtance, only here the free acceſs of the air is ſtill wanting, whereas otherwiſe it muſt have been transformed into a ſtone in the earth.

It appears from this, becauſe 1. The maſs hardens as ſoon as it is expoſed to the air and dried. 2. Diſtillation diſcovers ſomething of an empyreumatic *petroleum* compoſed of vitriolic and bituminous parts. 3. If oil of vitriol is poured on *oſteocolla*, an acid of common ſalt ſeparates from it. 4. I infer that *oſteocolla*, is not a *calx*, becauſe it can by no means be extracted from it.

I have tried the *oſteocolla* with various menſtruums what weight of each would be diſſolved in each of them: wherefore I always took ℥ ſs of

of *oſteocolla*, and ℥ ſs of each *menſtruum*; and obſerved, 1. That oil of vitriol diſſolved gr. iv. of it, that the ſolution was yellow, and that the reſidue was of a yellowiſh white. 2. That ſpirit of vitriol reduced all into a ſaline form. 3. That Sp. of Nitre diſſolved ℈i. gr. iv. And 4. The acid of common ſalt ℈i. gr. vi. 5. That *Aqua Regia* diſſolved ℈i. gr. iv. and that both ſolutions were yellow, and the reſidue of a yellowiſh white. 6. That diſtilled vinegar diſſolved ℈iſs, and that the ſolution of it was yellowiſh; but that the reſidue, like the others, remained free from the *menſtruum* as to any change of colour.

Oſteocolla therefore, as it is uſed in the ſhops, is to be reputed a mineral from which a vegetable has been ſeparated: it may be called a calcarious earth; but it does not change the ſyrup of violets.

By diſtillation with an open fire, it affords an urinous ſpirit, it ferments on the pouring on of a fixt alkali, and diſcovers at the ſame time an urinous ſpirit: the reſidue being mixt with water, and reduced to a lixivium, did not produce any thing ſaline; I endeavoured at the ſame time to reduce one part into a *calx*, but could not obtain any. But if oil of vitriol is poured on *oſteocolla*, an acid of common ſalt ſeparates from it. *Oſteocolla* being calcined with an alkali ſeems to conſtitute an opake glaſs, but it may be reſolved again into water, and therefore it cannot be looked upon as a true glaſs. *Oſteocolla* being put into a tubulated retort, and ſet over a ſlow fire, and having oil of vitriol poured thro' the tube, gets free from the ſpirit of ſalt, which may alſo be obtained by diſtillation. I have alſo ſaturated the Sp. of ſalt thus obtained with a fixt alkali, and produced a regenerated common ſalt: I have put it again into the retort after it has been dried, and poured oil of vitriol upon it, and at laſt have obtained by diſtillation an acid ſpirit of common ſalt.

The chief *baſis* of *oſteocolla* is ſand; ſome will have *lac lunae*, *medulla ſaxonum*, and *lapis oſteocollae* to be one and the ſame thing, but they are miſtaken. If any redneſs is found in the *oſteocolla*, it is owing to ſomething of iron. It's uſe in medicine is abſorbent; whence ſome preſcribe it for the *Fluor albus*.

XII. An accident calling me yeſterday to *Hedgerley*, the place where there is dug an earth commonly called *Windſor* loam, and famous not only in *England*, but many other parts of the world, I took an opportunity of going to the pits, and informing myſelf of the preſent condition of them: and as there appears too much probability that this earth will be exhauſted, and loſt intirely to the world, in a few years, I preſume it may not be unacceptable to you to have an account of the pits of it, and whatever elſe relates to it, taken on the ſpot; which I here do myſelf the honour of communicating to you, and ſhall take the liberty of adding to it what has ſince occurred to my thoughts in regard to the ſupplying it's place when loſt, in the many different occaſions on which it is now uſed.

A letter from Mr John Hill, *Apothecary, to the* Preſ. *concerning* Windſor Loam. N°. 483. p. 458. Mar. &c. 1747. *dated* May 28. 1746. *Read* Mar. 19. 1746-7.

This

This earth itſelf is a coarſe harſh loam, compoſed of a very large ſhining ſand, of extreme hardneſs, and a fine ſoft tenacious clay: it's value is it's remarkable quality of ſtanding the force of the moſt violent fires without running to a glaſs; which makes it extremely uſeful to all who have occaſion for ſuch fires, and is the reaſon of it's being ſent not only into all parts of *England*, but to *Holland*, *Germany*, and many other parts of the world. It is uſed for making the bricks employed in building the wind-furnaces for melting iron, for coating over the inſides of aſſay-furnaces, uſed by the workers on metals, and on many occaſions of like kind at the glaſs-houſes, both in *England* and other nations.

The place where it is dug is *Hedgerley* before-mentioned, a ſmall village about 22 miles from *London*, ſurrounded with hills, under one of which this loam lies. The pits are about $\frac{1}{4}$ of a mile S. W. from the town, and 5 miles N. of *Windſor*: they extend over 4 acres of ground, ſituated on the deſcent of a hill; and were intended to have been carried over much more ground by the perſon who now works them; but, on trials, the loam is found not to extend as was imagined.

They dig, before they come at this, a very good common brick-clay, a tile-clay, and a potter's earth, a kind of clay of a firmer texture, and deeper colour, than either of thoſe; but the *ſtrata* of theſe are ſeldom pure or regular, and at the boundaries of the *ſtratum* of loam a pure hard ſand, evidently the ſame with that in the compoſition of the loam, but left looſe, from there not having been clay in the way to bring it into the condition of the perfect maſs. They have already worked the *ſtratum* ſo far as to find it bounded E. and W. by beds of this ſand, and N. by chalk, and are therefore afraid it will be ſoon exhauſted; at leaſt, whatever they get hereafter, muſt be procured with more labour and expence, as they have no where to ſearch for it but higher up in the hill; from whence it muſt be fetched at greater depths, and much more expence: and this increaſing difficulty of procuring it has been the reaſon of it's riſing in it's price to that it is now ſold at, which is 5 ſhillings a buſhel in *London*; but which is not to be wondered at, ſince on the ſpot the quantity that makes 1000 bricks, which uſed to coſt 1 *s.* and 8 *d.* now coſts 10 *s.* the digging, and will every year coſt more and more, unleſs a new *ſtratum* of it ſhould be diſcovered ſomewhere thereabouts, which their many unſucceſsful trials make them at preſent deſpair of.

It is to be obſerved, that this valuable earth forms but a ſingle *ſtratum*, and that does not riſe and dip with the elevation and deſcent of the hill, as the *ſtrata* of the earth, ſtone, *&c.* in hills uſually do, but ſeems to be even and flat at it's bottom; for the higher up the hill they open their pits, the deeper in proportion they find the *ſtratum* of loam lie.

It is worthy obſervation, that this hill appears from this not to have been formed as the hills and mountains on the earth in general have been

been by a diſruption and elevation of the *ſtrata* by violence from within the earth; for, in that caſe, this *ſtratum* of loam muſt have been elevated with them, and would have been as near the ſurface, or nearly ſo, in one part of the hill as in another, and need have been dug for no deeper from the top than from any other part; whereas, on the contrary, it appears to lie flat and level underneath the whole maſs of earth, which makes the hill, and was, in all probability, the ſurface, on the firſt ſettling of the terreſtrial and other matter from among the waters of the Deluge.

The earth, which makes the hill, ſeems to have been a prodigious maſs of matter, rolled along by the irreſiſtible force of that immenſe body of water, and afterwards lodged upon it.

That this might be the caſe, the immenſe force of that vaſt quantity of water, and the eaſe with which heavy bodies are moved in water, may ſerve to make probable; and what the more favours the conjecture is, that the earth which makes the hill is not diſpoſed in ſuch regular pure *ſtrata* as the earths ſettled regularly from the waters always are, but ſeems evidently a mixed maſs, made by the jumbling together of various kinds of clay, *&c.* which are, in ſome parts of it found pure, tho' not in whole *ſtrata*; and in others irregularly blended in different proportions one with another; which, as the principal matters that compoſe it are of very different colours, *viz.* a red and a white clay, is the more apparent. And this is further confirmed, by there being none of thoſe common extraneous nodules found lodged in it, which are ſo frequent in the *ſtrata* of clay formed by ſubſidence; ſuch as the *Ludus Helmontii*, *pyritæ*, *&c.* Theſe have ſettled with, and lodged themſelves almoſt every-where among thoſe *ſtrata*; but it is no wonder there are none of them here, if this hill has been formed, as I imagine; ſince, in the rolling it along, they muſt naturally have been left behind: and I promiſe myſelf, that the frequency of theſe bodies in almoſt all our little clay-pits, and the intire abſence of them in the vaſt quantities of clay that have been dug here, will be eſteemed, by all who have looked deeply into theſe ſtudies, one great argument of the truth of this ſyſtem; which may alſo extend perhaps to many other hills as well as this.

As the workmen are now obliged to dig this loam at 26 feet deep, inſtead of about 14, at which depth they long found it, and muſt hereafter, as they are obliged to aſcend the hill, dig it at 38 or 40 feet, the price of it will, I am afraid, rob us of it, before the vein is exhauſted. I think it would be a matter worthy conſideration, whether, from examining the parts it is compoſed of, a *ſuccedaneum* might not be found for it, by an artificial mixture of ſimilar ſubſtances. In order to attempt this, I have, by means of water, diſunited it's parts, and procured them ſeparate; and, on comparing them with the various earths and ſands from different parts of *England*, which I have at times procured, I think that I can exactly match the ſand with one from *Hampſtead-Heath*,

and

and the clay with one from a pit near the lower end of *Highgate:* the proportions may be eaſily learned, by accurate obſervation of the quantities of each, where diſunited; and a *ſuccedaneum* on theſe principles eaſily made.

It is evident to me, that the only reaſon why it endures the fire ſo much better than other clays, is the extreme hardneſs and great quantity of the ſand it contains: and as I imagine it eaſy to throw a ſand of equal hardneſs, and in equal quantity, into an artificial loam, I ſee no reaſon to doubt of making it equally uſeful.

The formation of Pebbles; *in a letter from Mr.* W. Arderon, *F. R. S. to Mr* H. Baker, *F. R. S.* Ibid. p. 467. *Read* April 2. 1747.

XIII. In my late ſearches after ſands, pebbles, and other foſſils, in our county of *Norfolk* (ſome whereof I had the pleaſure to ſend you not long ago) I made ſuch occaſional obſervations on the ſituation and condition of the ſeveral bodies I met with, as reaſon muſt, I think, ſuggeſt to every man that conſiders them. I ſhall trouble you with no hypotheſis, nor form any random gueſſes, to account for ſuch their ſituation, and the condition wherein they are found; but, if a relation of true facts, and concluſions naturally deducible therefrom, may prove acceptable, they are intirely at your ſervice.

In all *ſtrata* of pebbles, that I have yet examined, there are ſome which are broken, and whoſe pieces lie together, or very near each other; but, as bodies of ſuch hardneſs could not be broken without ſome conſiderable force or violence, their ſituation implies, that they ſuffered ſuch force or violence as broke their parts aſunder, in or near the place where they at preſent lie.

Others again have had pieces broken from them, though not the leaſt fragment of thoſe pieces can now be found: from whence we muſt conclude, that whatever might be the cauſe of their fracture, they muſt either have been broken at ſome place diſtant from where they now lie, or the pieces broken from them muſt at ſome time or other have been removed to ſome diſtant place.

Several of theſe pieces of broken pebbles have their edges and corners ſo very ſharp, that it ſeems as if they had never been removed from the place where they received the damage. Others have their ſides and corners ſo blunted, rounded, and worn away, that one cannot help imagining they muſt have been very roughly toſſed backwards and forwards againſt other hard bodies, and that too with great violence, or for a very long continuance; ſince, without a great deal of friction, ſuch hard bodies could ſcarcely have been reduced to the forms they are now found in.

It may poſſibly be objected, that theſe pieces of ſtones grew in the figure wherein they now appear; but I am fully ſatisfied, that any man who will take the pains to examine theſe bodies carefully, will ſoon be convinced, from their veins, or grain, or coats, which ſurround each other, ſomewhat like the different years growth in trees, that they muſt

once

once have been complete and intire: and this will be more fully evident, if they are compared with a ſtone broken by art.

Among theſe *ſtrata* of pebbles are ſeveral fragments of various kinds of marble, various kinds of ſand-ſtone, and various kinds of *gypſum* (though this part of the kingdom affordeth no ſuch thing); moſt of which have attained the hardneſs of the very hardeſt of our pebbles, as it ſhould ſeem, by lying amongſt them.

Such pebbles as are found here in *ſtrata* near the ſurface of the earth, are much more brittle, and break eaſier without compariſon, than thoſe which lie in deeper *ſtrata*: for, if the firſt of theſe fall, but with their own weight, upon any other ſtone, from the height of 3 or 4 feet, they will break very frequently into 10 or 12 pieces; whereas ſuch as are found deep in the earth will endure being thrown againſt one another with all the force one can give, and that too 20 times perhaps, before the leaſt ſplinter of them can be broken off.

I have conſtantly found, that the more clean and tranſparent the ſands are with which our pebbles are mixed, the more beautiful the pebbles themſelves are, however different their colours be.

It is wonderful to obſerve and conſider with what amazing ſkill the Creator of all things hath diſpoſed the different *ſtrata* of the earth, to ſerve the purpoſes of His Wiſdom.

The vegetable mould or ſurface of the earth is compounded or made up of ſands, clays, marls, loams, rotten ſtalks, and leaves of herbs, *&c.* ſerving as a proper bed and covering, as well as a receptacle and conductor of moiſture, to the roots of trees and plants in general.

Sands and pebbles may be conſidered as drains for carrying off the redundant moiſture, to where it may be ready to ſupply the place of what is continually riſing in exhalations; but, leſt the *ſtrata* of ſand ſhould be too thick, ſmall ones of clay are often placed between, and ſeem intended to prevent this moiſture from departing too far from where it may prove of general uſe. And, leſt theſe curious but thin partitions of clay ſhould give way, by their ſoftneſs, for the particles of ſand to inſinuate into them, and thereby let the moiſture paſs through, thin cruſts of a ferrugineous ſubſtance are placed above and beneath each of theſe clayey *ſtrata*, and ſerve effectually to keep the clay and ſand aſunder.

The obſervations you have now read muſt be underſtood to relate to the county of *Norfolk* only; for I have never had any opportunity of ſearching the bowels of the earth in other places; but the general uniformity of nature makes me ſuppoſe the ſituation and circumſtances of pebbles, ſands, *&c.* in other countries may not be very different.

XIV. Gems or precious ſtones, of all ſpecies, are ſometimes found of regular ſhapes, and with a natural poliſh; and ſometimes of irregular ſhapes, and with a rough coat. The firſt ſort may be conſidered as of the pebble kind; and they are ſaid to be found near the beds of rivers,

Some obſervations upon Gems or Precious Stones; more particu-

larly ſuch as the Ancients uſed to engrave upon; by Robert Dingley, *Eſq*; Ibid. p. 502. *Read* May 7. 1747.

rivers, after great rains: the others are found in mines, and in the clefts of rocks.

The gems of the firſt ſort were what the Ancients moſt uſually engraved upon: theſe are commonly called *intaglio's*; and they are moſtly of a long oval figure, inclining to a point at each end, convex as well on the engraved face, as on the others, with a ridge running from end to end on the under ſide, which is hereby, as it were, divided into two faces; both which are alſo, though not ſo diſtinctly, parted from the upper face, by another ridge running quite round the oval.

The ſtone moſt commonly found engraved is the *beryl*; that moſt frequently found next is the *plaſm*, or prime *emerald*; and then the *hyacinth* or *jacinth*. The *chryſolite* is ſometimes, but rarely, found engraved; as are alſo, but that very ſeldom, the *cryſtal*, or *oriental pebble*, the *garnet*, and the *amethyſt*.

Of the *beryl* there are three ſpecies; the red, inclining to orange-colour, tranſparent and lively; the yellow, of an ochre-colour; and the white, commonly called the *chalcedon*, of the colour of ſheer milk. Theſe two laſt have leſs life than the firſt.

The *plaſm* or prime *emerald* is green, nearly of the colour of ſtagnated water; ſometimes tolerably clear, but, for the moſt part, full of black and white ſpecks, and rather opaque.

The *jacinth* is of a deep tawny red, like very old *port* wine, but lively and tranſparent.

The *chryſolite* is of a light-green graſs-colour, and is ſuppoſed to have been the beryl of the Ancients, tranſparent, but not lively.

The *cryſtal* or *oriental pebble* is harder and more lively than the common rock *cryſtal*; is of a ſilveriſh hue, and but very little inferior to the white *ſapphire*.

The *garnet* is of the ſame colour as the *jacinth*, but more inclining to the purple, and not ſo lively.

The *amethyſt* is of a deep purple, tranſparent and lively.

There were ſome other ſpecies of ſtones engraved upon by the *Romans*; but rarely before the latter times of the empire, when the art itſelf was greatly upon the decline.

All the before-mentioned ſorts of ſtones are ſaid to have been of the produce of *Egypt*, or of the *Eaſt-Indies*; and to have been brought from the borders of the *Nile*, or of the *Ganges*.

Here follows a general table of what are uſually called Precious Stones.

The *beryl*, is red, yellow, or white.
The *plaſm*, is green.
The *jacinth*, of a deep tawny red.
The *chryſolite*, of a light graſs green.
The *cryſtal*, or *oriental pebble*, of a ſilveriſh white.
The *garnet*, of a deep red claret-colour.
The *amethyſt*, purple.

The

The *diamond*, white.
The *ruby*, red or crimſon-colour'd.
The *emerald*, of a deep green.
The *aqua marina*, of a bluiſh ſea-green, like ſea-water.
The *topaz*, of a ripe citron yellow.
The *ſapphire*, of a deep ſky-blue, or of a ſilver white.
The *cornelian*, red or white.
The *opal*, white and changeable.
The *vermilion-ſtone*, is more tawny than the *jacinth*.

All theſe ſtones are more or leſs tranſparent: the following are all opaque:

The *cat's-eye*, brown.
The red *jaſper*, called alſo thick *cornelian*, is of the colour of red ochre.
The *jet*, black.
Agates, are of various ſorts.
The *blood-ſtone*, is green, veined or ſpotted with red and white.
The *onyx*, conſiſts of different parallel *ſtrata*, moſtly white and black.
The *ſardonyx*, of ſeveral ſhades of brown and white.
The *agat-onyx*, of two or more *ſtrata* of white, either opaque or tranſparent.
Alabaſter, different *ſtrata* of white and yellow, like the *agate-onyx*, but all opaque.
The *toad's-eye*, black.
The *turquoiſe*, of a yellowiſh blue inclining to green.
Lapis-lazuli, is of a fine deep blue.

Of moſt of the ſpecies before-mentioned there are ſome of an inferior claſs and beauty. Theſe are commonly called by Jewellers Occidental Stones: they are moſtly the produce of *Europe*, and found in mines or ſtone-quarries; and are ſo named, in oppoſition to thoſe of a higher claſs, which are always accounted oriental, and ſuppoſed to be only produced in the more eaſtern parts of our continent.

The *onyx*, *ſardonyx*, *agate-onyx*, *alabaſter* of two colours or *ſtrata*, as alſo certain ſhells of different coats, were frequently engraved by the Ancients in *relief*; and theſe ſorts of engravings are commonly called *Cameo's*. They alſo ſometimes ingrafted a head, or ſome other figure in *relief* of gold, upon a *blood-ſtone*.

Beſides which there are ſome antiques, moſtly *cornelians*, that are covered with a *ſtratum* of white. This *ſtratum* has by ſome been looked upon as natural; but it was really a ſort of coat of enamel that was laid on. This was uſed only in the times of the lower empire.

The ſtones eſteemed the beſt for engraving upon were the *onyx* and *ſardonyx*; and next to them the *beryl* and the *jacinth*.

The Ancients engraved moſt of their ſtones, except the *onyx* and *ſardonyx*, juſt as they were found; their natural poliſh excelling all that can be done by art; but the beauty of the ſeveral ſpecies of *onyx's* could only be diſcovered by cutting.

The merit both of *intaglio's* and *cameo's* depends on their erudition, on the goodneſs of the workmanſhip, and on the beauty of their poliſh.

The antique gems of *Greek* work are the moſt eſteemed; and next to them the *Roman* ones, in the times of the higher empire.

An account of certain perfect minute cryſtal ſtones; by J. Parſons, *M. D. & F R. S.* N°. 476. p. 468. Apr. &c 1745. *Read* June 27. 1745. Fig. 50.

XV. The drawing here annexed, repreſents a ſmall cryſtal magnified; it is one of a great number brought by a very curious gentleman from *Gibraltar*, who has cauſed many of them to be ſet in buckles of different kinds, for the wear of his lady and himſelf: and although they are formed and poliſhed by nature; yet they look very bright, and produce a very good effect in the buckles.

They were found accidentally. This gentleman ſaw a man cleaving a rock near that town, and obſerved a great quantity of fine black powder fall from it's crevices; and, being very curious, he examined the powder, and found theſe little ſtones in cluſters, conſiſting of no more than 12 or 14 each; and each cluſter lying at conſiderable diſtances from one another. They are all of the ſame form, ſome leſs perfect than others, and are in general hexagonals.

The ſpecific Gravity *of* Diamonds; *in a letter from Mr* John Ellicot, *F. R S. to the* Preſident. Ibid. *Read* July 4. 1745.

XVI. As, from ſome experiments I have lately had the opportunity of making, it appears highly probable, that what has formerly been publiſhed concerning the ſpecific gravity of diamonds, is not to be depended upon; I hope a ſhort account of theſe experiments will not be unacceptable to you, eſpecially as I do not find the leaſt notice taken of the ſpecific gravity of diamonds in any of the tables publiſhed in the *Philoſophical Tranſactions*.

In the account the hon. Mr *Boyle* has given of diamonds *, he relates it "as the opinion of a famous and experienced cutter of diamonds, "that ſome rough diamonds were conſiderably heavier than others of "the ſame bigneſs, eſpecially if they were cloudy or foul; and Mr "*Boyle* mentions one that weighed 8 grains and $\frac{8}{16}$; which, being carefully weighed in water, according to the rules of hydroſtatics, proved to an equal bulk of that liquor, as 2 $\frac{2}{2}\frac{2}{3}$ to 1; ſo that, as far as "could be judged by that experiment, a diamond weighs not thrice "ſo much as water." And yet, in this table of ſpecific gravities, that of a diamond is ſaid to be to water as 3400 to 1000, or as 3, 4. to 1; and therefore, according to theſe two accounts, there ſhould be ſome diamonds, whoſe ſpecific gravity ſhall differ nearly the $\frac{1}{8}$ from others; which I am perſuaded, is a much greater difference than could be ex-

* Pag. 83. Vol. V. new edition of Mr *Boyle*'s works in *folio*.

pected

pected in any bodies of the ſame kind, or that which, on a more nice examination, will be found to be in diamonds.

The firſt diamonds I had the opportunity of ſeeing weighed, were two very large ones from the *Braſils*, which were furniſhed by Mr *Chace*, a merchant in *Auſtin-friers*: the ſpecific gravities of theſe were found to be much greater than the heavieſt of Mr *Boyle*'s, the one being to an equal bulk of water as 3518, and the other as 3521 to 1000, and the difference between them leſs than the $\frac{1}{1000}$ part. There were two ſmaller *Braſil* diamonds weighed at the ſame time, which indeed were not quite ſo heavy as the former, the lighteſt being but as 3501, the other as 3511; but, as theſe were of the ſame kind, and comparatively ſmall, I judged this difference could not be much depended on. Having therefore an opportunity ſome time ſince of a large parcel of *Eaſt-India* diamonds, I choſe out 10, which, both in ſhape and colour, and every other reſpect, were as different from each other as poſſible. Theſe being weighed in the ſame ſcales and water as the former, the lighteſt proved to be as 3512, and the heavieſt as 3525; the very near agreement of theſe laſt with each other, and with the former, tho' weighed at about eight months diſtance, makes it highly probable, that ſo great a difference as appears from the place above-cited, and Mr *Boyle*'s table, is not to be found in any diamonds whatſoever, much leſs ſo great a difference as appears between the lighteſt of his and the heavieſt of mine, being above $\frac{1}{7}$ of the whole.

I had never made any experiments myſelf, by which I could form a judgment, how much of the difference between theſe and former trials might ariſe from the different tempers and qualities of the waters uſed; warm water being lighter than cold, and pump-water generally heavier than river-water. But, taking it for granted, that all perſons who make ſuch experiments uſe common and not mineral waters, and waters of the natural temper, and not heated deſignedly, I am aſſured by a friend, who has made many careful trials for this particular purpoſe, that the ſpecific gravity of any body will not differ above $\frac{1}{200}$ at the moſt, on account of the quality of the water and temper taken together; whereas the heavieſt of Mr *Boyle*'s diamonds, as in his tables, differs from the lighteſt of mine by above $\frac{1}{35}$ part, which is about ſix times as much as $\frac{1}{200}$: and yet I can think of no other way of accounting for the reſt of this difference; unleſs it ſhould ariſe from the ſmallneſs of the diamonds, or any defect in the inſtruments with which his experiments were made.

The ſcales in which theſe diamonds were weighed turned very ſenſibly with the $\frac{2}{100}$ part of a grain; and as one of the diamonds weighed above 92 grains, it was capable of being weighed to leſs than the 18000th part: ſeveral of them were weighed twice over both in water and air, and the weights found to agree to the greateſt exactneſs; and if to this is added the very near agreement of the weights of the ſeveral diamonds, though weighed at different times, and at a conſiderable

diſtance

diſtance from each other, I think it highly improbable, that there could be any conſiderable miſtake in theſe trials; and therefore their ſpecific gravities, as in the following table, may fully be depended on.

I have ſet down the weights of the ſeveral diamonds both in air and water, that if any miſtake ſhould have happened, it may be the more eaſily rectified.

N°.		In Air	In Water	Specif. Grav.
	Water	. .	. .	1000
		Grains	Grains	
1	A *Brazil* diamond, fine water, rough coat	92,425	66,16	3518
2	A *Brazil* diamond, fine water, rough coat	88,21	63,16	3521
3	Ditto. fine bright coat, ...	10,025	7,170	3511
4	Ditto. fine bright coat, ...	9,560	6,830	3501
5	An *Eaſt-India* diamond, pale blue, .	26,485	18,945	3512
6	Ditto. bright yellow, ...	23,33	16,71	3524
7	Ditto. very fine water, bright coat, .	20,66	14,8	3525
8	Ditto. very bad water, honeycomb coat,	20,38	14,59	3519
9	Ditto. very hard blueiſh caſt . . .	22,5	16,1	3515
10	Ditto. very ſoft, good water, . . .	22,615	16,2	3525
11	Ditto. a large red foul in it, . . .	25,48	18,23	3514
12	Ditto. ſoft bad water, ...	29,525	21,140	3521
13	Ditto. ſoft brown coat, ...	26,535	18,99	3516
14	Ditto. very deep green coat, . . .	25,25	18,08	3521

The mean ſpecific gravity of the *Brazil* diamonds appears to be 3513.
The mean of the *Eaſt-India* diamonds . . . . 3519.
The mean of both to be . . . . . 3517.

An extract, by Phil. Hen. Zollman, *Eſq; F. R. S. of a* Philoſ. Account *of a new opinion concerning the origin of* Petrifactions *found in the* Earth, *which has been hitherto aſcribed to the univerſal Deluge; as contained in*

XVII. The *Italian* author has adopted a new ſyſtem concerning marine petrifaction, the cauſe of which he refers to fire, inſtead of water, according to the opinion commonly received.

The place of his abode has furniſhed him with particular opportunities of comparing marine petrifactions found in the mountains, with the true marine bodies produced by the ſea. The ſaid place is called *San Vito di Tagliamento*, 6 hours journey from *Venice*, under the Biſhop of *Concordia*, belonging to the patriarch of *Aquileia*.

The author is a clergyman; but never entered into any eccleſiaſtical community, nor into any univerſity as profeſſor; to be out of the way of envy: however he keeps a boarding-ſchool for young men. He has publiſhed the book at his own expence; which has brought him into ſome trouble, and rendered the book at firſt very ſcarce. He ſhews a great conformity to the principles of Sir *Iſaac Newton*, and other modern

dern Philoſophers, not very common in *Italy*, grounding himſelf upon experience, and mathematical proofs.

Having in the firſt part formed the ſtate of the queſtion, he examines the ſyſtems of *Burnet* and *Woodward*, almoſt generally received by the learned, though the former does not make any expreſs mention of petrifaction. He refutes their opinions about the Deluge, and of it's being the cauſe of petrifactions. He lays down for a fundamental maxim, that the Deluge ought to be believed, according to the Scripture, as a miracle, and not to be proved by natural rules; from which he proceeds to another; *viz.* That whoever lays down, for a foundation, a principle which does not fit the ſeveral *phænomena*, builds upon an erroneous principle.

After having refuted at large Dr *Woodward*'s opinions, he proceeds to the eſtabliſhing his own ſyſtem, grounded upon ſubterraneous fire, with various arguments of his own, and with the refutation of thoſe of others.

He firſt lays down ſome general principles, according to Sir *Iſaac Newton*, *&c.* and then applies to them ſeveral inſtances for ſupporting his ſyſtem.

The firſt is the new iſland riſen out of the ſea in the year 1707, near the iſland *Santorini* in the *Archipelago*.

The ſecond is a mountain, which roſe out of the earth in 1538. near *Pozzuolo* in the kingdom of *Naples*, overwhelmed the little town *Tripergula*, and dried up a navigable lake named *Lucrano*; being now called the *New Mountain*, equal in height to a neighbouring old one, called *Monte Barbaro*. From the circumſtances attending thoſe events, he endeavours to prove his new hypotheſis. He calls to help the ſeveral eruptions of the mountains *Veſuvius* and *Ætna*; and then forms his theſis; *viz.* "That marine animals and productions (for inſtance, "ſhells, *&c.*) which are now found in high mountains, were firſt ge- "nerated in the ſea: but when thoſe mountains were raiſed, by ſub- "terraneous fire, above the ſurface of the ſea, were petrified ſo as they "now appear."

This theſis *Moro* endeavours to ſupport, by giving the detail of the 12 ſeveral *ſtrata* found in the territories of *Modena*, when they are digging for wells, mentioned by *Woodward*, *Camerarius*, *Vallisnieri*, and *Ramazzini*; whoſe remarks, as well as the newer ones of *Whiſton* and *Bourguet*, he will not allow to be ſatisfactory; the greateſt difficulty being this, that, from the nature of ſome of thoſe *ſtrata*, it ſeems that the ſea has twice covered the plain of *Modena*, now above ſome hundreds of feet above the level of the ſea; and that from another *ſtratum* it may be inferred, that, in the intervals between thoſe overflowings of the ſea, the land has been inhabited and cultivated. His theſis he endeavours to ſupport, by a remarkable paſſage from *Pliny*, *book* II. *chap.* 87. *Ingens terrarum portentum L. Marcio, Sex. Julio Coſſ. in agro Mutinenſi!*

an Italian *book, intitled,* De Croſtacei ed altri marini Corpi che ſe trovano ſu' Monti, di *Anton. Lazzaro Moro,* Venice 1740. *communicated together with ſeveral remarks; by Dr* Belthaſar Ehrhart, *phyſician in ordinary at* Memmingen, *and member of the Acad* Nat. Curioſ. *in* High-Dutch at Memmingen, 1745. 4*to.* N°. 479. p. 163. Mar. & Apr. 1746. *Read* April 24. 1746.

Mutinenſi! Namque montes duo inter ſe concurrerunt, crepitu maximo aſſultantes, recedenteſque; inter eos flamma fumoque in cælum exeunte, &c.

Dr *Ehrhart* compares with this the ſeveral *ſtrata* found in digging in the neighbourhood of *Memmingen* laſt year.

Moro touches next upon the hypotheſis of ſome, that the ſea increaſes about one foot in height in about two centuries; and of ſome others, that it decreaſes 5 feet in one century; as alſo, how the ſaltneſs of the ſea may be deduced from his hypotheſis.

Dr *Ehrhart* hopes that *Moro*'s ſyſtem may one time prevail againſt prejudices, as well as thoſe of *Vergilius*, *Galilæus*, *Harvey*, &c.

A letter from Mr James Simon, *of* Dublin, *to* M. Folkes, *Eſq*; *P. R. S concerning the* Petrifactions *of* Lough-Neagh *in* Ireland: *to which is annexed a letter from the R. Rev. Dr* Geo. Berkeley, *L. Biſhop of* Cloyne *to* Tho. Prior, *Eſq*; N°. 481. p. 305. Oct. &c. 1746. *Read* Dec. 18. 1746.

XVIII. Moſt of the antient writers, that have treated of *Ireland*, have made mention of the peculiar qualities of *Lough-Neagh* of turning wood into ſtone; ſome of them (*a*) have gone ſo far as to ſay, that it would turn that part of the wood which was in the mud into iron; the part in the water into ſtone, whilſt the part above water remained wood.

Some later writers, particularly Meſſieurs *William Molyneux*, *Francis Nevill*, and *Edward Smyth*, and from them the late learned Dr *Woodward*, (*b*) the author of the notes on *Varenius*'s Geography, and others (*c*), ſeem rather to think, that this petrifying quality doth not lie ſo much in the lake itſelf, as in the ground near or about it.

Mr *Edw. Smyth* (*d*), who inlarges the moſt on this ſubject, and ſeems to have led the others, and drawn them into his opinion, tells you, "That no experiment or obſervation yet made, that he could hear of, "could prove that this lough has really the quality of petrifying wood, "or that the water doth any way help or promote the petrification." He there gives you an example of a gentleman of worth and credit, "who had fixed two ſtakes of holly in two different places of the lough, "near that place where the *Upper Bann* enters into it, and that the "parts of the ſtakes which had been waſhed by the water for about 19 "years, yet remained there without any alteration, or the leaſt advance "to petrification."

Another reaſon for his doubting of this quality is, "That tho' it is "reported that the water hath this virtue, eſpecially where the *black-*"*water* diſcharges itſelf into the lake, yet that, as it ſeems evident, "from the nature of liquid bodies, that any virtue received in one part "muſt neceſſarily be diffuſed thro' the whole, at leaſt in ſome degree; "therefore (ſaith he) there is good reaſon to believe, that the water is "wholly deſtitute of this petrifying quality:" But a few lines lower he tells you (*e*), "That he had ſufficient ground to conjecture, that

(*a*) *Boëtius* Hiſt. Gem & Lap.
(*b*) Catal. of *Engliſh* Foſſils, Part II. p. 19.
(*c*) Sir *James Ware*'s Antiq. by *Walt. Harris*, p. 227. Edit. 1745. folio.
(*d*) Afterwards Biſhop of *Down*. *See* Phil. Tranſ. N°. 174.
(*e*) *Ibid. ut ſupra.*

"other

" other wood as well as holly had been petrified about this Lough; be-
" cauſe ſome fiſhermen, being tenants to a gentleman from whom he
" had this relation, told him, that they had found buried, in the *mud* of
" this *Lough*, great trees, with all their branches and roots petrified;
" and ſome of that bigneſs, that they believed they could ſcarcely be
" drawn by a team of oxen; that they had broke off ſeveral branches
" as big as a man's leg, and many bigger, but could not move the great
" trunk."

I ſuppoſe Mr *Smyth* (or the gentleman his friend) ſaw theſe branches, and was thereby convinced of their real petrification, as he was by the bulk of thoſe trees of their being oak, and not holly; " becauſe, ſays
" he, no other tree in that country, theſe excepted, grows to that
" prodigious bigneſs; at leaſt it is certain, that holly never grows to
" that bigneſs."

But how Mr *Smyth* came to be convinced, that theſe trees were oak, and not holly, and yet was not convinced of the petrific quality in ſome parts of the Lough, tho' theſe trees were found petrified in it's mud, is amazing to me: for, if a team of oxen could ſcarcely draw them from thence, it was as hard, in my opinion, to draw them from any adjacent ground (where they muſt have grown, lain, and been petrified) into the mud of the lake, where they were afterwards found: for it muſt be ſuppoſed, that either theſe trees grew on the banks of the lake, and, thro' age, or any other accident, fell into the water or mud, and were there petrified; or that, with great labour and expence, they were brought into it from ſome adjacent ground, after their actual petrification, which is hardly to be ſuppoſed.

Mr *Smyth* (*a*) tells you farther, that " Two gentlemen of the north
" (of *Ireland* where this Lough lies) had told him, that they had ſeen the
" ſame body, partly wood, and partly ſtone; but the only reaſon for
" thinking ſo, being the diverſity of colours, which might well enough
" proceed from ſeveral degrees of petrification, we may properly think
" them deceived; for they made no experiment on that part which
" they reputed wood. The bark is never found petrified, as I am in-
" formed by a diligent inquirer; but often ſomething rotten about the
" ſtone, anſwerable to the bark."

Mr *Smyth* I think contradicts himſelf no leſs in his laſt ſuppoſition than he did in the firſt. His friends aſſured him, that they had ſeen one or more of the *Lough-Neagh* ſtones partly wood and partly ſtone; but they were deceived, he ſays: the diverſity of colours, by which they judged one part of the ſtone by it's colour to be wood, and the other part likewiſe, by it's colour different from the other, to be ſtone, were no more than different degrees of petrification. What are we to underſtand by theſe different degrees of petrification? by this ſomething rotten about the ſtone often found? if not, that ſome part of the wood

(*a*) *Ibid. ut ſupra.*

was actually turned into ſtone, ſome other part in a degree leſs petrified, and ſome other part not petrified at all, as theſe gentlemen aſſured him: the diverſity of colours, ſeeing and feeling, was enough to convince them, and to determine the point.

As to his aſſertion, That, becauſe the water of this lake has not everywhere, and in every place, that petreſcent virtue, it muſt therefore be a good reaſon to doubt of it's having that peculiar quality in ſome particular places, I think it may be denied for theſe reaſons; *1ſt.* Becauſe a ſpring, tho' ever ſo much impregnated with petrific, mineral, or metalline particles, iſſuing out in ſome particular place of the lake, can no more communicate it's petrifying virtue to the waters of the whole lake, than the river *Thames* it's ſweetneſs to the ſea, and make all it's water freſh.

Secondly, Becauſe that if this lapideſcent quality was equally diffuſed thro' the water of the whole lake in a degree ſufficient to turn a whole tree, or any of it's larger branches into ſtone, in all parts of the lake without exception, that petreſcent virtue muſt act equally on all the plants or vegetables whatſoever that grow in the lake, and upon all other bodies, gravel, ſand, mud, and clay, that are in, or are daily brought into it; and, at laſt, by a general aggregation, agglutination, and attraction of theſe different bodies together, the whole bottom of the lake, nay the whole lake itſelf, by the different degrees of coalition of particles, muſt become a ſolid body; unleſs you would ſuppoſe, that that this petrific quality has no power on any other matter but wood, which is contrary to experience, ruſhes, or other plants, having been found petrified on the ſhores of this lake, as alſo ſhells, clay, and ſand petrified in different ſhapes, of all which I have ſpecimens.

" The earth, ſays the great *Robert Boyle* (*a*) harbours different kinds " of petreſcent liquors, and many of them impregnated with one ſort " of mineral or other." There are no ſprings, no waters, but are more or leſs impregnated with ſuch mineral and ſaline particles; which appears from the moſt limpid; which, after evaporation, ſtill in the *reſiduum*, gives ſome particles of ſalt together with ſome ſtony and mineral ones.

I have found by experience, that petrifying ſprings are generally impregnated, ſome with calcarious and particles of other ſtones, and others with ferrugineous and vitriolic particles. Thoſe of the ſtony or calcarious kind, I have obſerved, when they drop on wood, or other vegetables, act on them for the moſt part by incruſtation, having different degrees and periods for their reſpective incruſtations and coalitions, which yet ſtick cloſe to one another: they ſeldom turn the wood into ſtone; but, ſticking to the wood, plants, *&c.* coagulate on it, and by degrees cover it with a cruſt of a whitiſh ſubſtance of different thickneſs, whereby the wood is immerged or wrapped in a ſtony coat, which,

(*a*) *R. Boyle*, of the origin and virtues of Gems.

if

if it be broken before the wood be rotten, you will find it in the heart of the ſtone or incruſtation, as is ſeen in thoſe petrifications at *Maudlin Meadows* in *Glouceſterſhire*, at *Hermitage* near *Dublin*, and many other places: or, if the wood be rotten, you will find a cavity in the ſtone, which very often is filled by a ſubſequent incruſtation or petrification; the ſtony particles then taking the place of the rotten wood.

Sometimes indeed, theſe waters, permeating the pores of the wood either longitudinally or tranſverſely, inſinuate themſelves therein, fill them up with their ſtony particles, ſwell, and, by their burning or corroding quality proceeding from the lime-ſtone, deſtroy the wood, and aſſume the ſhape of the plant, the place whereof they have taken.

Theſe kind of petrifications generally ferment with acids and ſpirit of vitriol, and, by calcination, may be reduced to lime.

Ferrugineous or metallic petrifying waters moſtly act by inſinuating their fineſt particles through the pores and veſſels of the wood, or other vegetables, without increaſing their bulk, or altering their texture, tho' they greatly increaſe their ſpecific gravity: and ſuch is the petrified wood found in or on the ſhores of *Lough-Neagh*; for it doth not ſhew any outward addition or coalition of forcing matter ſticking to or covering it (except in ſome places, where a thin ſlimy ſubſtance, taken notice of hereafter, is ſometimes obſerved), but preſerve the grain and *veſtigia* of wood; all the alteration is is in the weight and cloſeneſs, by the mineral particles pervading and filling the pores of the wood: theſe ſtones, or rather wood-ſtones, do not make the leaſt effervescence with ſpirit or oil of vitriol, nor *aqua fortis*; which ſhews, that they are impregnated with metalline particles, or ſtony ones, different from the calcarious kind, and may be the reaſon why the petrified wood, mentioned by *N. Grew* (*a*), made no ebullition, at which it ſeems he was ſurprized (*b*). Theſe ſtones I could not reduce into lime by the moſt intenſe fire, nor, with proper ingredients, procure a vitrification or fuſion (*c*).

Altho' mines have not perhaps been diſcovered near the *Lough*, I have reaſon to believe that there are ſuch in its's neighbourhood, from the great quantity of iron-ſtones found on it's ſhores, and places adjacent to it, and from the yellowiſh ochre and clay to be met with in many places near it. Of theſe iron-ſtones, which are very ponderous, outwardly of an ocheriſh yellow colour, and inwardly of a reddiſh brown, I have calcined many, and do find the powder of all to yield ſtrongly to the magnet.

(*a*) Reg. Soc. Muſ. p. 270.

(*b*) This contradicts an obſervation of Mr *John Beaumont* (*Phil. Tranſ.* N°. 129. p. 731.) That moſtly mineral ſtones will ſtir with acids; whereas all thoſe that I have tried, whether *Engliſh* or *Iriſh*, did not at all ſtir with acids.

(*c*) Stones of the calcarious kind turn to lime by calcination, and ferment with acids; but other kinds, ſuch as ſlate, fire-ſtone, free-ſtone, rag, grill, *&c.* will do neither, as experience has hitherto teſtified.

Gerald Boate (*a*) mentions an iron mine, in the county of *Tirone*, not far from the *Lough*, and ſuch others at the foot of *Slew-Gallen* mountains.

That mines are generated and found in the bowels of hills and mountains, is obvious to any that have the leaſt knowledge of Metallurgy; and that ſprings alſo proceed from mountains, is no leſs obvious; therefore ſhould a ſpring happen in the bowels of any of theſe mountains to run thro', a vein of mineral of any kind ſoever, it will waſh and dilute ſome parts of ſuch mineral, impregnate itſelf with the unctuous, ſaline, and metallic particles of ſuch mines, and convey them along with it's water; and if in it's way, whether under-ground, or at it's iſſuing out of the cliffs of a mountain, of the ſides of a river, or of the lake in queſtion; or whether it riſes under water, in the middle of ſuch a river or lake in any particular place, and in it's courſe meets with wood, vegetables, or any other lax bodies (lodged in the mud or gravel), whoſe pores, by the natural heat of the mineral ſteams, or any other accident, being open and duly prepared, theſe metallic *moleculæ* and ſaline particles will penetrate through, inſinuate and lodge themſelves in the pores and veſſels of ſuch wood, *&c.* fill them up, and, by degrees, turn them into ſtone; (*b*) "There being ſome of theſe *lapideſcent juices* of "ſo fine a ſubſtance, yet of ſo petrifying a virtue, that they will pene-"trate and petrify bodies of very different kinds, and yet ſcarce, if at "all, viſibly increaſe their bulk, or change their ſhape and colour."

That ſuch ſprings there are, hidden under the water or mud of this lake, I hope will appear probable, from what has been ſaid, and perhaps evident, from the account I have ſince received, that, in the great froſt of 1740, the lake was frozen over ſo as to bear men on horſeback, yet ſeveral circular ſpaces continued unfrozen. But how the ſeveral attempts, made, as mentioned, by Meſſieurs *Molineux*, *Nevil*, and *Smyth*, to procure wood half petrified (by fixing ſtakes of holly in the lake, which received no alteration) proved unſucceſsful, the reaſon I think is plain, becauſe they were not fixed in the proper place, *viz.* the courſe or vein of the ſpring, where nothing but chance could have directed them. This petrified wood is often found in different places on the ſhores of the *Lough*, but generally in greater plenty when the water has been diſturbed by great ſtorms; which makes it impoſſible to fix on the particular place where the petrifying juice moſt prevails; except a tree, or any large piece, ſhould be found ſo fixed as to reſiſt the force of the waves.

Mr *Smyth* (*c*) makes this further obſervation: "This virtue is cer-"tainly, if *not only*, in the ground or ſoil, he judges (ſays he) for theſe "reaſons; That there are many ſtones turned up daily, eſpecially at

(*a*) *Nat. Hiſt.* of *Ireland*, *Dub.* 1726.
(*b*) *Rob. Boyle*, of Gems, p. 124. 8*vo*.
(*c*) Philoſ. Tranſ. *ibid. ut ſupra*.

"their

" their breaking up new ground, which we cannot in any probability " think were brought thither; they are often found at two miles dif- " tance from the *Lough*, ſeldom farther, in great numbers, and very " deep in the ground; and a gentleman (on whoſe credit I received " the information) ſaw a ſtump of a tree digged out of the ground at a " ſmall diſtance from the *Lough*, which, by handling of it, he found to " be petrified. He aſſured me, the roots and all were ſtone, and alto- " gether like thoſe ſtones that are ordinarily found, and go by the " name of *Lough-Neagh* ſtones. This gentleman was of opinion, theſe " were *lapides ſui generis*, till this obſervation convinced him: and that " theſe ſtones were once wood, is, I think, very certain; for they ſhew " the plain *veſtigia* of wood; they likewiſe burn, and cleave: filings " of this ſtone thrown into the fire emit a fragrant ſmell; and they cut " kindly with a knife, tho' not ſo eaſily as other wood (*a*).

That this petrific quality is in ſome peculiar parts of the lake, I have endeavoured to prove; that it is or may be in ſome peculiar places of the adjacent ground, I grant; tho' as yet, I could not procure any of thoſe ſtones found in the ground, with wood continuous. Such as I have, or have ſeen, are of the white whetſtone-kind, and ſeem to be holly or aſh, petrified by ſome ſtrong nitrous and ſtony particles; for, in a ſolution of it in *aqua fortis* and oil of vitriol, it leaves no tincture, but the liquor growing muddy, like pipe-water after great rains, therefore ſhews, that they are not ſo ſtrongly impregnated with metalline particles, as thoſe ſtones found in or on the ſhores of the lake.

I need not add any more, to ſhew how mineral ſprings may petrify wood, or any other vegetables under-ground; but as to whole trees found petrified and buried within a ſmall diſtance from the lake, I ſhould think that the *Lough* might have been formerly broader than it is at preſent, or perhaps hath loſt on one ſide what it has gained on the other; by which means, what is now dry ground was formerly under water, and the other ſide *vice verſa*: if ſo, ſuch trees as are found under-ground might have been petrified in that part which was over-flowed, and is now dry land.

Mineral ſteams or exhalations, being highly ſaturated with ſtony and mineral particles, are often found to have a petrifying virtue, as is ſeen at the bath called *Green Pillars* (*b*) in the city of *Buda* in *Hungary*. If ſuch ſteams ſhould, in certain places, find or force their way thro' the ſand or pores of the earth, they may operate on wood, *&c.* buried in the ground, permeate it's veſſels, and, by degrees, turn it into ſtone; and ſuch, I apprehend, is the moſt probable, if not the only reaſon, that can be aſſigned for thoſe petrifications of wood found in ſand, as mentioned by *Boyle* and *Plot*.

(*a*) An anſwer to this, ſee in the deſcription of the city of *Down*, *p.* 162. The argument is confuted by the deſired proof.

(*b*) *Philoſ. Tranſ.* N°. 59. p. 1049.

It

It may be obſerved, that the finer the lapidific particles are, the more beautiful and natural the petrification will appear; ſuch is a petrified root of the flag or *iris ſylveſtris* in my poſſeſſion, which is ſolid ſtone at the bottom, the pith being turned into a white or ſparry ſubſtance, and the growing knots of the root, tho' petrified, preſerving their ſkin brown, and ſomewhat flexible. This *phænomenon* indeed has been lately ſolved in the deſcription of the county of *Down*, *p.* 162. The *luſus naturæ*, or ſportings of nature, is a general ſolution, too often brought in, and comes in very *à propos* to anſwer queries concerning petrifications, ſuch as wood, ſhells, worms, *&c.* If the ſhells, or other like petrified bodies (found in marble or lime-ſtone) which preſerve the moſt exact reſemblance of the fiſh or body they repreſent, were not formerly a real fiſh, ſhell, worm, *&c.* how comes it that ſuch ſhells and other bodies are found unpetrified in marble, lime-ſtone, rock, marl, or any other ſtone? The R. Rev. Dr *Robert Clayton*, Biſhop of *Clogher*, hath ſhewn me in his collection a piece of *Italian* marble, where petrified ſhells are ſeen, and others no way petrified, but that may be crumbled to duſt with the fingers. I have a ſtone (which I found lately in the river *Liffy* at *Chapel-Izod)* of the *lapis vermicularis* kind, the ſurface whereof, on one ſide, is covered with a number of ſmall petrified worms or plants; part of which, ſticking and continuous to the ſtony ones, are ſtill ſoft and flexible.

To return to the *Lough-Neagh* petrifications; I received laſt ſummer, 1745. from the Rev. Mr *Richard Barton*, about 30 of theſe ſtones, found on the ſhores of the lake, ſome in the water, ſome in the mud, ſome in the ſand, and others in a yellowiſh clay. That they were petrified in the lake is probable, but whether in the water, mud, ſand, or clay, is no matter; for certain it is (to uſe Mr *Smyth*'s own words), that they were not brought hither from any diſtance, ſuch as 2, 4, 6, 8 miles, after being dug out of the ground, and then thrown and diſperſed on the ſhores of the lake: and beſides, the difference in the colour of theſe ſtones, thoſe found in the lake, and thoſe found in the ground ſomewhat diſtant from it, is ſuch that they cannot well be miſtaken one for the other. Thoſe found in the ground are white, and of a looſer texture; thoſe found in or on the ſhores of the lake are black, cloſer, and heavier. That theſe laſt were petrified by a mineral ſpring, appears from the few following obſervations. They do not ferment with acids, ſpirit and oil of vitriol. The ſolution of this ſtone in *aqua fortis* gives a beautiful red tincture, and in oil of vitriol leaves a tincture of a brown dark red. The woody part of theſe ſtones in *aqua fortis* alſo gives a red tincture, tho' ſomewhat paler; and, when taken out of the liquor, ſhews red ſpots in it's pores, which I take to be particles of iron and ſulphur: theſe ſpots, when the wood began to dry, became black; and the wood, when dry, turned of the colour of a deep red *Jeſuit's-bark*.

In

In ſome of theſe ſtones, ſeveral curious veins, of a red and blueiſh colour, are very remarkable, being intermixed with black and white *ſtriæ*.

Having broken ſome of theſe ſtones, I found in the inſide a kind of white, and ſeveral cluſters of ſmall white and black angular cryſtals, which thro' the miſcroſcope appear tranſparent, and of different ſhapes, but moſtly hexagonal. I diſcovered ſuch cryſtals in ſome of the woody part of theſe ſtones.

One piece of a white ſtone I calcined in a crucible for 24 hours, but could neither reduce it to coal or lime. The powder yielded faintly to the magnet. This ſtone was found in the ground at ſome diſtance from the lake.

One piece of a black ſtone, found in the lake, I likewiſe calcined for 24 hours, and could not reduce it to coal or lime: the powder yielded briſkly to the magnet.

I calcined one piece of another ſtone, about one inch thick, for about 4 hours, in an intenſe fire, until it grew as red as it could be, when I took it out of the crucible. I obſerved ſeveral veins (not diſcernible before) of a ferrugineous matter, about $\frac{1}{10}$ of an inch thick, and when reduced to powder, it applied ſtrongly to the magnet.

In other ſtones I found ſome veins of wood, about one and two inches thick, no way petrified, though the ſtones were every way ſo outwardly.

Some of that woody part I alſo burnt in a crucible; it emitted a bluiſh flame, as if impregnated with ſulphur, and had the ſtrong ſmell of burning charcoal. When burnt to a coal, and reduced into powder, it faintly yielded to the magnet.

How wood happens to be found in theſe petrifications, ſound and untouched, is ſomewhat ſurprizing, and to account for it not very eaſy. It may be attributed to this, that the texture of the wood is not everywhere equal; eſpecially where knots happen, that part is much harder and cloſer than any other; and if the petrefcent particles ſhould be once ſtopped, they will fix there, coagulate, and go no further; by which means that part of the wood will remain free from petrification, while the reſt will be turned into ſtone; or the pores of the wood may happen in ſome places, and in the very heart of it, to be ſo full of a reſinous matter, that it will keep out the petrific juices, and hinder their further penetrating into them equally: to this may be attributed the ſtrong ſmell of this wood when burning; and the more ſo, as I ſuſpect that moſt of this petrified wood was fir, there being a good deal of that kind found daily in turf-pits near the lake; ſome not above 20 yards diſtance from it; and the laſt piece of wood and ſtone continuous that I have received, appearing by the grain to be of that kind of wood.

Laſtly, the petrific juices may happen to be ſo ſtrongly impregnated with ſalts and metal, or any other mineral particles, that they will immediately ſwell and fill the minuteſt pores of the wood, and, by a ſud-

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den coalition, hinder their further penetrating into it; which ſeems to appear clearly from ſome cavities in one of theſe ſtones, which I ſuppoſe to have been worm-holes, and which were no way filled by the petreſcent liquor which was ſtopped round it; all the ſides of this hole being overlaid with ſmall brown cryſtals, occaſioned by the evaporation of the aqueous parts, and their being ſtopped and ſoaked by the neighbouring ſtone or wood.

The woody part of theſe ſtones, as I have obſerved, will burn to a coal, and emit a flame: that part intermediate betwixt the ſtone and wood, and which is but partly petrified, being harder than wood, and ſofter than ſtone, will grow red in the fire, emit a kind of flame, or rather ſparks of fire, but doth not conſume, and is properly what Dr *Grew* * calls incombuſtible wood. The ſtony part doth not burn, though it grows as red as coal.

I calcined another of theſe ſtones, weighing 1 *oz.* 13 *penywts.* 12 $\frac{1}{4}$ *gr.* after burning 4 hours it weighed but 1 *oz.* 10 *penywts.* 8 $\frac{1}{4}$ *gr.* and loſt 3 *penywts.* 4 *gr.*; which proceeds, I ſuppoſe, from unpetrified veins of wood in the heart of the ſtone, which were deſtroyed by the fire, as in the crucible it emitted now-and-then a blueiſh flame, as brandy doth when burning. This ſtone, when taken out of the crucible, and cooled, had the colour of iron, when heated in, and cooled from the forge.

Part of another ſtone, which, by viſible veins of ore, appears to contain a good deal of iron, I likewiſe calcined for 4 hours; the powder yielded moſt ſurprizingly to the magnet; ſo that it appears, that the opinion of *Nennius*, *Boëtius*, and other ancient writers, was not abſolutely deſtitute of foundation.

The white wood-ſtones are generally found in the ground at 2, 4, 6, and 8 miles diſtance from the lake, and ſometimes very deep in the earth.

The black ones are always found in the water, or on the ſhores of the Lough; ſometimes at the mouths of rivers or rivulets that empty themſelves into it; but thoſe with wood continuous have not yet been found above 20 yards diſtance from the water of the lake; that is, where the water reaches in the winter, or at other times.

Some of theſe ſtones are outwardly covered with a thin white ſubſtance, which hath run thro' the pores of that part of the ſtone that was expoſed to the air, and not covered by the water, mud, or clay; and on ſome others it is rather an incruſtation of that white ſubſtance, which I take to be the ſlimy, unctuous, ſaline parts of the petreſcent juices that filled the outward pores of the ſtone, or coagulated on it. This white part ſcraped, and put into a crucible in a violent fire, could not be reduced to lime, tho' it grew red as coal. This powder calcined appeared thro' the microſcope quadrangular, like grains of ſalt; which makes me ſuſpect, that theſe petrifications contain, beſides metalline, a great

* *Muſ. R. S.* p. 269.

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deal of ſaline particles, whoſe ſides being ſtrongly attracted to each other, and cloſely joined, hinders the fire from expanding the pores of theſe ſtones, and their being reduced to lime.

This black ſtone, when broken, appears thro' the microſcope very beautiful, and like cloth of ſilver, the pores and veſſels of the wood being filled with white minute cryſtals.

Of theſe ſtones I have ſome with wood outwardly continuous; others with wood inwardly; one, the leaſt part whereof is ſtone, the reſt wood; another *vice verſa*; another intirely wood, except a thin coat of ſtone on one ſide, which appears to be the very bark; one ſtone which at one end diſtinctly ſhews the annual ringlets of the wood; one that ſhews the wood, before it was petrified, had been bent, and partly broken, the fiſſure being filled with a ſparry matter, and appears plainly from the preſent appearance and poſition of the fibres of the ſtone. Some of theſe ſtones ſtrike fire with a ſteel, and others, by a ſtrong colliſion, emit a train of ſparks.

Some of theſe ſtones ſhew the grain of holly, aſh, and fir. I have but one piece of oak petrified, eaſily diſtinguiſhed by it's grain; it ſhews the very knots of the wood where young twigs were cut; and has a hole made thro' it before it was petrified.

As for theſe ſtones being fit for ſharpening or ſetting of razors, *&c.* the black ones are rather too hard, and the white ones too ſoft. The whet-ſtones or hones, vulgarly ſo called, which are ſold for *Lough-Neagh* ſtones, are none of theſe, but of a ſoft gritty kind, and found near *Drogheda*.

When theſe ſtones with wood continuous are taken out of the water, mud, or clay, the woody part dries, cracks, and falls away; which is the reaſon why few can be well preſerved; and beſides, every body, unwilling to truſt their eyes, will touch and ſcrape the wood, and by theſe means, deſtroy the moſt curious part of the ſtone.

The curious gentleman above-mentioned, who hath already begun, and intends, at his leiſure, to take an accurate ſurvey of the lake, will, I hope, be able to give a more juſt and ſatisfactory account of it's petrifying virtue than I poſſibly can; my deſign in the preſent attempt being only to pave the way, and induce others to make further experiments in ſearch of truth, and for improving natural knowledge.

P. S. I had lent the above papers to the Biſhop of *Cloyne*, from whom I received them yeſterday, with the original letter to *Thomas Prior*, Eſq; of which follows the copy.

Cloyne, May 20. 1746.

A letter from the Right Rev. Dr George Berkeley, *L. Biſhop of*

I Here ſend you back the curious Diſſertation of Mr *Simon*, which I have peruſed with pleaſure; and though variety of avocations gives me little time for remarks on a ſubject ſo much out of my way, I ſhall never-

Cloyne, *to* Tho Prior, *Esq*; *in* Dublin.

nevertheleſs venture to give my thoughts briefly upon it, eſpecially ſince the author hath been pleaſed to invite me to it by a letter.

The author ſeems to put it out of doubt, that there is a petrifying quality both in the lake and adjacent earth. What he remarks on the unfrozen ſpots in the lake is curious, and furniſheth a ſufficient anſwer to thoſe, who would deny any petrifying virtue to be in the water, from experiments not ſucceeding in ſome parts of it; ſince nothing but chance could have directed to the proper places, which, probably, were thoſe unfrozen parts.

Stones have been thought by ſome to be organized vegetables, and to be produced from ſeed. To me it ſeems, that ſtones are vegetables unorganized. Other vegetables are nouriſhed and grow by a ſolution of ſalt attracted into their tubes or veſſels. And ſtones grow by the accretion of ſalts, which often ſhoot into angular and regular figures. This appears in the formation of cryſtals on the *Alps:* and that ſtones are formed by the ſimple attraction and accretion of ſalts, appears in the tartar on the inſide of a claret-veſſel, and eſpecially in the formation of a ſtone in the human body.

The air is in many places impregnated with ſuch ſalts. I have ſeen at *Agrigentum* in *Sicily* the pillars of ſtone in an ancient temple corroded and conſumed by the air, while the ſhells which entered the compoſition of the ſtone remained intire and untouched.

I have elſewhere obſerved marble to be conſumed in the ſame manner; and it is common to ſee ſofter kinds of ſtone moulder and diſſolve merely by the air acting as a *menſtruum*. Therefore the air may be preſumed to contain many ſuch ſalts, or ſtony particles.

Air, acting as a *menſtruum* in the cavities of the earth, may become ſaturated (in like manner as above-ground) with ſuch ſalts, as, aſcending in vapours or exhalations, may petrify wood, whether lying in the ground adjacent, or in the bottom of the lake. This is confirmed by the author's own remark on the bath called the *Green Pillars* in *Hungary*. The inſinuating of ſuch ſalts into the wood ſeems alſo confirmed by the author's having obſerved minute hexagonal cryſtals in the woody part of the petrifactions of *Lough-Neagh*.

A petrifying quality or virtue ſhews itſelf in all parts of this terraqueous globe, in water, earth, and ſand; in *Tartary* for inſtace, and *Afric*, in the bodies of moſt ſorts of animals, it is even known that a child hath been petrified in the mother's womb. *Oſteocolla* grows in the land, and coral in the ſea. Grottoes, ſprings, lakes, and rivers, are in many parts remarkable for this ſame quality. No man therefore can queſtion the poſſibility of ſuch a thing as petrified wood; tho' perhaps the petrifying quality might not be originally in the earth or water, but in the vapour or ſteam impregnated with ſaline or ſtony particles.

Perhaps the petrification of wood may receive ſome light from conſidering amber, which is dug up in the king of *Pruſſia*'s dominions.

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I have written theſe haſty lines in no ſmall hurry; and ſend them to you, not from an opinion, that they contain any thing worth imparting, but merely in compliance with your and Mr *Simon*'s requeſt.

And yet, before I have done, I muſt needs add another remark, which may be uſeful for the better underſtanding of the nature of ſtone. In the vulgar definition, it is ſaid to be a foſſil incapable of fuſion. I have nevertheleſs known ſtone to be melted, and when cold to become ſtone again. Such is that ſtuff, by the natives called *Sciara*, which runs down in liquid burning torrents from the *craters* of mount *Ætna*, and which, when cold and hard, I have ſeen hewed and employed at *Catania*, and other places adjacent. It probably contains mineral and metallic particles; being a ponderous, hard, grey ſtone, uſed for the moſt part in the baſements and coinage of buildings.

Added from a letter to Dr J. Fothergill, Dublin, Aug. 8, 1746.

Hence it ſhould ſeem not impoſſible for ſtone to be caſt or run into the ſhape of columns *, vaſes, ſtatues, or *relievo's*; which experiment may perhaps, ſome time or other, be attempted by the curious; who, following where nature has ſhewn the way, may (poſſibly by the aid of certain ſalts and minerals) arrive at a method for melting and running ſtone, both to their own profit, and that of the public.

XIX. It was found in a cavern, that was diſcovered amidſt the vaſt marble rocks at *Cat-down* near *Plymouth*. It hung perpendicularly from the top of the rocky cavern, and was a cylindrical tube of 20 inches long at leaſt; but was unluckily broken into ſeveral pieces in bringing to me. This I have ſent was by much the longeſt of them; but Mr *Long* (the maſter of the quarries) aſſured me the whole was above 20 inches long, and quite cylindrical, and quite hollow. I went to the cave the next day, and found 5 or 6 of ſuch kind of tubes, but none above 2 inches long. They all ſprang from a broad, hollow, protuberating baſis, in ſome ſort as a nipple ariſes from the breaſt. Theſe alſo were cylindrical and hollow. There were in the ſame cavern many other petrifications, which had formed a kind of hollow pilaſters againſt it's ſides; and alſo ſeveral large ſolid maſſes, which aroſe from the continual dropping of the petrefying water through the crevices of the ſuperior rock. Theſe all afford very good alabaſter.

An account of a beautiful Stalactites *now in the* Muſeum *of the* R. S. *by* John Huxham, *M. D. F. R. S. in a letter to Dr* Mortimer. N°. 474. p. 207. June &c. 1744. *Read* Dec. 13. 1744.

XX. The *belemnites* is a foſſil of different magnitudes and colours, ever regular in ſhape, which is either cylindric, conic, or thereunto approaching. Numbers of them have, on one ſide only, a chap or ſeam running their whole length; others have it in part; and in others it is not at all to be obſerved: it conſiſts of a talcy matter, with an intermixture of ſpar or cryſtal, diſpoſed in *ſtriæ* from or near it's centre to

A diſſertation on thoſe foſſil figured ſtones called Belemnites; *in a letter from Mr* Emanuel Mendez da Coſta *to* Martin Folkes, *Eſq*; *Pr. R. S.* N°. 482. p.

* To confirm what the Biſhop ſays, I remember when I was in the college in *France*, that I went to ſee a relation of mine, a frier, at *Fontevraud*, where he ſhewed me in their church two pillars of ſtone, about 60 feet high, all of one ſolid piece, which he ſaid had been run. *J. S.*

397. Jan. & Feb. 1747. *Read* Jan. 29. 1746-7.

it's circumference, and is made up of cruſts incloſing each other, the innermoſt whereof is as regular as the outermoſt. Sometimes, though ſeldom, in compariſon to the numbers of the *belemnites*, in the centre is a cavity ever conic, whatever the external ſhape of the *belemnites* be. This conic cavity is at different times empty, or elſe filled, either with a ſolid body of mineral matter, cryſtal, ſtone, *pyrites*, *&c.* or with a regular-jointed conic body, called by Lithologiſts the *Alveolus* of the *Belemnites*; which, though conſtantly regular and jointed, is nevertheleſs found compoſed of various mineral or metallic ſubſtances.

The *alveolus* above-mentioned, tho' not fully proved ſuch, yet ſeems, by the aſſent of moſt of the preſent Naturaliſts, to be a body of marine origin; a ſhell the nigheſt related to the *nautilus* kind: it is concamerated, and even in ſome is diſcovered another great characteriſtic of the *nautilus* kind, I mean the gut or *ſiphunculus*. Therefore, taking this body for granted to be of marine origin (for what reaſons, or of what kind, is not my preſent intended ſubject to prove) it remains to diſcuſs, Whether this body became accidentally lodged in the *belemnites*? or, Whether the *belemnites* itſelf is alſo of marine origin, and a part dependent on it's *alveolus*?

Various have been the opinions of Lithologiſts concerning the origin of the *belemnites*; ſome have even aſſerted them of the vegetable kingdom; others, that they are teeth or horns of fiſh, appendages of ſhells, bodies caſt in ſhells of the *tubuli* kind, or the very ſhells themſelves, ſpines of *echini*, or a kind of ſtrait *nautilus*. The three laſt opinions are what I ſhall ſtrive to confute, as they ſeem ſomewhat probable, and are now the moſt prevailing; and prove the *belemnites* to be a natural foſſil or *lapis ſui generis*. I deſire no recourſe to the ſubterfuges uſed by others, but hope you will agree with me in the axiom, that all *belemnites* are of one and the ſame origin.

That the *belemnites* are not teeth or horns of fiſh, I ſhall refer you to the letter Dr *J. Woodward* wrote on that ſubject to Mr *Bourguet*, of *Switzerland*, wherein he fully proves the erroneouſneſs of thoſe opinions. But a further argument againſt their being teeth, which that learned Naturaliſt has not touched upon, is, that no *belemnites* have that natural varniſh or poliſh, which always covers the teeth of all animals; whereas the greateſt part of thoſe foſſil bodies, which we know to be ſuch, as the *bufonitæ*, *gloſſopetræ*, *&c.* are found with that ſame varniſh or poliſh. As for their owing their form to being moulded in ſhells, it will appear contradictory to reaſon, when we conſider, 1. Their conſtitution to be ever as regular as their figure; and, 2. That their inner layer or *nucleus* is as equally regular as the outer cruſt or whole body; which particular could never have happened, had they been moulded in ſhells; as is evident, by the *turbinitæ*, *conchitæ*, and other bodies, which owe their figures to that cauſe. That the *belemnites* are not ſpines of *echini*, let us firſt conſider, that no kinds hitherto diſcovered have been ever found to have ſpines analogous to theſe bodies; nor indeed has any marine

marine ſhell whatever ſuch a texture. The immediate ſubterfuge for an anſwer to this objection is, that the kinds of ſhells unknown to mankind are far more in number than thoſe yet diſcovered. I allow it; but think that cannot be an argument in the preſent caſe, ſince no one ſingle ſpecies is yet diſcovered with ſuch, nor even any *genus*, which have ſpines analogous to the *belemnites*. Nature bears an analogy through all her works; and though all the ſpecies of any one *genus* is not known to any man, yet that analogy nevertheleſs capacitates us to judge of thoſe undiſcovered by thoſe we know. Thus we find of the *echinus* kind, all the ſpecies now known are ever found near the ſhores; conſequently, are not ſubject to be eternally hidden from us; as is undoubtedly the caſe of the *cornua ammonis*, and *conchæ anomiæ*. They are no pelagian ſhells, as thoſe are; bays and harbours are the places where they are fiſhed; their ſtructure even evinces the reaſons for it. We may therefore with probability conclude, that all the *echinus* kind are of the ſame nature, and have the ſame way of living; that they only inhabit ſuch places, and that none are pelagian ſhells; conſequently might have been diſcovered.

I am ſenſible there are ſome ſpecies of foſſil *echini*; as, the moſt common *conoid* or *pileated echini*, the common *echini galeati*, the *echini clypeati*, and ſome kinds of the *echini ovarii*, &c. which though we are certain that they have been marine ſhells, yet thoſe particular ſpecies are not known in the ſea: but then ſeveral other ſpecies of that ſame *genus* are. The caſe of this is quite different, ſince not one ſingle ſpecies of ſuch a *genus* has ever been found.

The exceſſive bigneſs and thickneſs of numbers of *belemnites* deſcribed by authors, *viz.* of near 2 feet in length, and above 2 inches in diameter in the thickeſt part, others of 3 feet long, and others as thick and long as a man's arm; not to enumerate thoſe only under a foot length, and of proportionable thickneſſes, concludes *echini* of a vaſt bigneſs, to have a number of ſuch ſpines to move.

The varieties of the *belemnites*, how can they quadrate to the ſpines of one *genus* of *echini* only? ſolid *belemnites*, *belemnites* with a ſingle cruſt, or like a tube, with a conic cavity only; that empty, or otherwiſe filled with a ſolid maſs, or with a regular-jointed body, as the *alveolus*, *belemnites* of various magnitudes and thickneſſes, *&c.* can all theſe varieties be imagined to belong to one *genus* of ſhells, which we ſuppoſe to exiſt to maintain a favourite ſyſtem?

The number of ſpecies of *echini* diſcovered is great; and the ſpines of all thoſe agree in having a hollow *axis*, which runs proportionably from their *baſis* to their *apex*, quite different to the *belemnites*: and for their conſtitution, a foreign Naturaliſt, a Member of the *R. S.* Mr *Klein* of *Dantzick*, who has profeſſedly written on this ſubject, could only find of 2 kinds, *viz.* thoſe of a porous conſtitution, which he obſerved only to belong to one *genus*; and thoſe of a ſolid ſhattery ſubſtance,

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like a talcy ſpar not ſtriated; which is the moſt general, and is exactly the ſame conſtitution as all the foſſil ſpines, or *lapides Judaici* are.

Further, the *Lapides Judaici* have, at ſome times, been found adhering to their *papillæ* or tubercles, and with fragments of their ſhells; whereas no Naturaliſt has ever known to be found foſſil either the ſhells, or the fragments of ſuch a *genus* of *echinus*; not even any remains proportionable to ſuch large ſpines. In whatever manner the greater part of ſuch ſhells may have periſhed (which is unlikely, if we conſider their texture and ſtrength), ſome muſt have eſcaped, when the ſpines are found in ſuch exceſſive numbers every-where, and always perfect and regular; whereas the foſſil ſpines, or *Lapides Judaici*, as they are called, as likewiſe the *echini* or ſhells, and all the foſſil bodies of marine origin, are found broken and ſhattered in all kinds of manners.

As for their being ſhells of the *tubuli* kind, my reaſons againſt it are; Were the *belemnites* ſuch, they muſt be all tubular more or leſs; or otherwiſe muſt have ſuffered ſome degree of petrifaction to fill up their cavities. The unreaſonableneſs of that argument is demonſtrated by all *belemnitæ* being of one and the ſame texture and conſtitution; though numbers are ſolid, and numbers are tubular, in different degrees. Now one kind of petrifaction, or any other change in the earth, which they might have undergone, could never have given ſo regular a texture and ſubſtance, and cauſe ſuch different effects as ſolidity and tubularity. And if, on the other hand, we allow it to be inconſiſtent, as it is, to form the idea of a ſhell of the *tubulus* kind, by a ſolid body, without that body having ſuffered ſome change in the earth, while buried in it, we muſt either deny all ſolid *belemnites* to be ſuch *tubuli*, and run to ſubterfuges, by owning them to be natural foſſils; or elſe allow a great inconſiſtency, to uphold a wrong ſyſtem.

That the *belemnites* are not a tubular caſe, which is part of, and covers a ſhell of the *nautilus* kind, as is it's *alveolus*. The variety of circumſtances already alledged of the *belemnites* ſerve to demonſtrate the improbability alſo of this opinion, as it has done of the other two. The numbers of *belemnites* of all kinds, ſo plentiful every-where, and the conſideration of how few are furniſhed with *alveoli*.

Numbers, I am ſenſible, have conic cavities; but that thoſe cavities never did contain *alveoli*, is evident; that the ſides of the ſaid cavities are even, and without any circular or other impreſſions, which a *belemnites* that has ever contained an *alveolus* muſt have; that body being in cloſe contact to all parts of the inveſtient *belemnites*, muſt conſequently impreſs it with it's concamerations; which impreſſions muſt be therefore found on the ſides of the cavities of all *belemnites* which ever contained them.

As for aſſerting, that all the *alveoli*, which are now found looſe, were originally lodged in *belemnites*, it cannot be; without inferring alſo, that all *belemnites* which are now devoid of *alveoli*, contained ſuch formerly;

merly; which, by ſome external or other agent, have been forced out and looſened from them.

To conſider ſuch an agent, we muſt alſo conclude it's force to have been exceeding great, to looſen out the *nucleus* of a body in cloſe contact with all it's inveſtient parts; and ſtrengthened further to it by ridges and grooves, ſuch a force muſt have compreſſed, ſhattered, and otherwiſe broken and deſtroyed the *belemnites* that contained them; which is contrary to obſervation. Further, forcing out the *alveolus* might perhaps eaſily have happened to the conic *belemnites*; which hath a baſis of a larger diameter than the middle, where the *alveolus* is lodged; but we cannot conceive the ſame by the cylindric, fuſiform, and other *belemnites*, of which the two ends or extremes terminate pointed; while the middle, where the *alveolus* is lodged, is thick and ſwollen.

To force an *alveolus* out of ſuch ſhaped *belemnites*, it is evident, that the narrow ends of the ſaid *belemnites* muſt be quite forced open, broken, and ſhattered, before a broader and more capacious body could be forced through, eſpecially to ſuch a brittle ſhattery foſſil as the *belemnites* is. The evident facts to the contrary of this are alſo too common to inſiſt on, ſince all theſe *belemnites* are ever found regular, perfect, and intire.

Further, let us conſider the *alveoli* which are now found in *belemnites*, they are very ſeldom if ever found as mere ſhells, but ever differently changed or petrified. They are moulded of ſtone, *pyrites*, cryſtal, *&c.* Now it can never be argued, that the contained bodies can ever be ſo differently changed or petrified in their covers or ſhells, and thoſe covers or ſhells which admitted ſuch different petrifying particles to undergo no change or petrification whatſoever.

Another proof againſt this opinion, is the diverſe forms of *alveoli* now diſcovered by Naturaliſts, as conic, cylindric, curved, ſpiral at the *apex*, &c. whereas all *belemnites* which have cavities have none but conic ones.

Theſe cylindric, *&c. alveoli* are now found in *Pomerellia* in *Poland*, in the marble of the iſland of *Oeland* in the *Baltick Sea* belonging to *Sweden*, and in the marble of *Sweden*; in *Gothland* in maſſes of building-ſtone; in *Ingria*, in ſeveral parts of *Pruſſia*, *&c.* and are commonly of an immenſe bigneſs, to ſeveral feet in length, and proportionably thick, yet not perfect. For ſuch *alveoli*, which are only *nuclei*, we muſt ſuppoſe immenſe large *belemnites*; and ſuch we have never heard of, ſo with probability we may conclude none ſuch to exiſt.

I do not doubt the growth of this error, of the *belemnites* being a part of it's *alveolus*, to have been cauſed by too raſh concluſions, and too little an inſight into the mineral kingdom; which has propagated that aſſertion of the *alveoli* being found only in the *belemnites*; which experience daily contradicts, ſince we find them looſe, as well as imbedded in many other foſſil ſubſtances, as in marble, ſtone, *&c.* as has been above obſerved.

Theſe

Theſe are the arguments which I alledge for the improbability of the ſaid opinions. I could advance a number of other proofs; but as I have already extended my letter beyond a due length, I beg leave, before I conclude, only to offer ſome few reaſons for their being a natural foſſil, or *lapis ſui generis*.

The very view of a *belemnites* ſufficiently evinces it's mineral origin, and ſhews it evidently compoſed of two foſſil ſubſtances, a talc, and a ſpar, or baſtard cryſtal; whereof the former is the baſis, and from which principle I do not heſitate to attribute it's ſtriated texture. Moſt of the talcy bodies are of a fibrous nature, and ſeveral are compoſed of cruſts incloſing each other, in the ſame manner as the *ſepta* of the *ludus Helmontii*, ſome of the *aſbeſtos* kind, the *Hæmatites* cruſts, *&c.* Of the *ſtalactites* tribe there are ſeveral, which ſo intirely approach the texture and conſtitution of the *belemnites*, that were their ſhapes a little more regular, the moſt experienced Lithologiſt might eaſily be deceived: and I remember, when abroad, to have ſeen ſuch, of a prodigious bigneſs, which, though I was then ſomewhat converſant in the foſſil ſtudy, I could not help taking for *belemnites*. I do not therefore wonder, that *Petrus Aſſaltus, in notis ad Metallothecam Mercati*, p. 282. and *Langius, Hiſt. Lap. figurat. Helvetiæ*, p. 133. ſhould judge them a native figured foſſil, formed in the earth, of the *ſtalactites* kind, if that term for the *belemnites* might with propriety be uſed.

The cavities of *ſtalactites* in ſome meaſure illuſtrate, and are adequate to the cavities of *belemnites*; they are placed in as various poſitions, and are only different from them by not being exactly conic. As for the regular figure of the *belemnites* being excepted againſt, I believe few Foſſiliſts will argument that, when we ſee as perfect regular figures in the mineral kingdom as in any other parts of the creation; as witneſs the ſalts and cryſtals of all kinds; the rhomboid, hexagonal, columnar, and other *ſelenites*; the cubic, octangular, dodecaedral, and other *pyrites*; the quadrangular pyramids of tin, the rhombs of iron, cubes of lead, and infinite other native foſſils, which would take up time to enumerate, and which are far more perfect figures than the *belemnites* are. Chemical and other trials and teſts (which I hope to lay before you in ſome future letter) demonſtrate a greater certainty of its mineral origin. As for that marine body the *alveolus*, I cannot think otherwiſe than that it is of the *nautilus* kind; and which, at the concretion or formation of the *belemnites*, became accidentally lodged in it's cavity, in the ſame manner as all other marine bodies became lodged in the various foſſil ſubſtances we now find them in.

Some remarks on the precious ſtone called the Turquoiſe;

XXI. This ſtone has received it's modern name of *turcheſia*, and *turquoiſe*, from it's being moſt commonly brought from *Turky* into various parts of *Europe*. *De Boodt* * ſays, the colour of this gem is a variegation of green, white, and blue; and that there are two ſorts of

* Gemmar. & Lap. Hiſt.

it,

it, the oriental, from the *Eaſt Indies* and *Perſia*, and the occidental, from *Spain*, *Germany*, *Bohemia*, *Sileſia*, *&c.* that in *Perſia*, where it is found in greateſt plenty, adheres to black ſtones, as if it were an excrement or a tranſudation from them. A ſtone of this ſort is ſeldom found to exceed a walnut in ſize; and he mentions one in the Great Duke's *Muſeum*, on which the head of *Julius Cæſar* is engraven as a very extraordinary ſample: he adds, that he never ſaw one bigger than an hazel-nut; that ſome of the oriental ones have the faculty of preſerving their colour perpetually, which are called Stones of the old Rock; and that others loſe their colour gradually, and are called of the new Rock. He then gives an inſtance of a turquoiſe which had loſt it's colour upon being laid by ſome time after it's owner's death, which recovered it's beautiful colour upon our author's wearing it upon his finger in a ring.

by Cromwell Mortimer, *Sec. R. S.* &c. Ibid. p. 429. *Read* Feb. 26. 1746-7.

Cæſius, in his Treatiſe *de Mineralibus*, p. 601. ſays, This ſtone is called *Turcoïs* by *Mylius*, in his *Baſilica Chemica*; by *Albertus Magnus*, in his Treatiſe of Minerals; and by *Rueius*, in his Treatiſe of Gems: but *Turca*, by *Cauſſinus de Lapillis Symbolicis*. *De Boodt*, and Dr *Woodward*, * with other modern writers, take it for the *Callaïs* of *Pliny*. *Salmaſius*, in his *Plinian. Exercit.* p. 142. ſays, Many have miſtaken the modern *turquoiſe* for the *cyanus*, but that the *cyanus* was tranſparent like the ſapphire; whereas the *turquoiſe* is a ſort of jaſper.

Dr *Woodward*, in his Letter to Sir *Jo. Hoſkyns*, † ſays, That the *turcoïs*, or *callaïs* of *Pliny*, is nothing elſe but foſſil ivory tinged with copper. I do not deny, that ſome ſtones ſold for *turquoiſe*, and poſſibly all that the Doctor ſaw were certainly ſuch; but I imagine thoſe which the authors call of the old Rock, and in which the colour is permanent, are real mineral ſtones: this ſample now before us ſeems to ſhew this, from both the form and ſize: it's ſhape ſhews it not to be part of any animal bone; but it's botryoïd form is to me a demonſtration that it is the product of fire, which had once melted this ſubſtance; and that when it cooled, it's ſurface was formed into bubbles and bliſters, in the ſame manner as the *hæmatitis botryoïdes* or blood-ſtone, whoſe ſurface conſiſts of knobs, reſembling a bunch of grapes.

That the *elephas* ἐρυκτὸς, or *ebur foſſile* of *Theophraſtus* ‖, ſaid to be of various colours, I do not in the leaſt deny to be tinctured with copper, and to be what Dr *Woodward* calls the *turquoiſe*: indeed I ſuſpect it to be what *De Boodt* calls of the new Rock; and ſays is liable to loſe it's colour, which it recovers again from the *effluvia* of the perſon who wears it. I therefore, for diſtinction ſake, think all theſe ſtones of the ivory origin ſhould be called *Pſeudo-Turcheſiæ*, or baſtard *Turquoiſe*;

* Method of *Foſſils*. Letters, *p.* 17. † Ibid. p. 16.

‖ See *Theophraſtus*'s Hiſt of *Stones*, tranſlated, *&c. John Hill, Lond.* 1746. 8vo. p. 94.

and the other ſort, of which this before us is one, the true or real *turquoiſe*; for, by examination in the chemical way, I find it to be a very rich copper ore; ſome of it pounded and diſſolved in ſpirit of hartſhorn gives a deep blue; in *aqua fortis* a fine green; and an iron wire put into it was in 1 hour's time incruſted with copper: ſome of it calcined, without any flux in a crucible, run to a ſlag, or half vitrified ſubſtance; whereas the ſame heat, had it been ivory or bone, would have reduced it to a white aſh like bone-aſhes; for I expoſed it to ſuch a fire as vitrified the tile that covered it. It's hardneſs and conſiſtence to an engraver's tool ſeems to be the ſame as common white marble: it's colour is not mended by heat, but it grows brittle when red hot.

This ſpecimen, now ſhewn to the *Society*, was about 12 inches long, 5 inches broad, and in ſome places near 2 inches thick; rough on the under ſide, as though broken off from the rock it had been affixed to; and the upper ſide was compoſed of ſmooth poliſhed knobs, in form like to the botryoïd iron ore.

Sir *Hans Sloane*, in his noble *Muſeum*, has ſeveral ſpecimens of theſe orienta! *turquoiſes*, all botryoïd; eſpecially a maſs from *China*, about 3 inches long, 2 broad, and near an inch thick: all which ſeem to be copper ores: and he has likewiſe ſamples of *turquoiſes* from *Spain*, and the South of *France*; which are all ſmall, and ſeem really to be pieces of ivory tinged with copper.

A deſcription of a curious Echinites; *by Mr* Henry Baker, *F.R.S.* Ibid. p. 432. *Read* Feb. 26. 1746-7.

XXII. 1. Mr *Baker* takes the liberty of ſhewing the *Society* a very extraordinary *echinites*, the like to which he has never ſeen in any *Muſeum*, or found deſcribed by any author. For the *echenitæ* uſually met with, are made up either of chalk or flint, or ſome ſtony, chalky, or ſparry matter, formed within the ſhell of the *echinus*, and taking their figure thence as in a mould: which ſhell is oftentimes broken off and gone, but remains at other times impregnated with talcy or ſparry particles: whereas the ſubject now laid before us is compoſed of a tranſparent cryſtalline ſubſtance, which has received it's general figure by having been circumſcribed within the ſhell of ſome *echinus*, and ſhews linear ridges and diviſions correſpondent to the lines and plates found in this kind of *echinus*.

Was this all, it would be a very uncommon production, as theſe bodies have been very rarely known to be formed of cryſtal * ; but it is rendered much more curious and extraordinary, by having exact rows and ſeries of little cells, all of the ſame regular figure, though leſſening gradually in ſize, as they aſcend from the baſe upwards. Fig. 51.

* Sir *Hans Sloane* has a maſs, which was formed within an *echinus*; the ſhell being broken off; it is one half or ſide cryſtal, the other ſide of a ſubſtance like chalk, but much harder.

This

This body having been formed within the ſhell of an *echinus*, one would expect (as is the caſe in all other *echinitæ* uſually known), that it's figure ſhould be exactly anſwerable to the mould wherein it was formed; but Mr *Baker* begs leave to take notice, that the *echinus*' ſhell is perfectly ſmooth internally, having no riſing parts correſpondent to theſe cells or cavities; and therefore, as it could not receive it's configuration from thence, it muſt be owing to the natural ſhooting of the cryſtalline matter (tho' unlike every thing of that kind yet deſcribed), or to ſome other cauſe, which he don't pretend to know *.

The configuration ſeems nevertheleſs in ſome meaſure to correſpond with the nature of the ſhell wherein it was formed: as to the number of the rows of cells, they being ranged by fives, as the *papillæ*, *indentings*, *lines*, or other *marks* on the recent ſhells of *echini* conſtantly are; theſe rows are twenty in number; *viz.* five double ranks of large and extremely regular cells, as at *a a*, &c. between which lie five other double rows of ſmaller and leſs diſtinct *cellulæ*, ſhewn at *b b*, &c. Theſe cells, which are hexagonal, and whereof thoſe in every row lie alternately to thoſe of the next (by which means they fill up the whole ſpace), decreaſe in their ſize gradually, as they approach nearer to the top; all the rows at laſt almoſt concentring at the *apex*, leaving only a ſmall ſpace or vacuity, where in the ſhells themſelves of this kind of *echinus* there is an aperture. The ſmooth part at *A* is formed of a pebbly ſtone, bearing the ſame marks as are uſually found in the impreſſion of theſe *echinitæ* dug up in gravel-pits; which proves, that this muſt have received it's general figure from one of thoſe ſhells, whatever has been the cauſe of this remarkable configuration of the cryſtalline part.

This curious *echinite* was found in a marl-pit at *Baborough*, about 3 miles W. of the city of *Norwich*, and preſented to Mr *Baker* by Mr *Wm. Arderon*, F. R. S.

2. I have the honour of laying before you (in order to communicate to the *R. S.* if you think it worthy) the deſcription of two *echinites*, or ſtones moulded in foſſil *echini* ſhells, hitherto undeſcribed, as far as I know.

A letter from Mr Emanuel da Coſta, *F. R. S. to the* Preſ. *concerning two beautiful* Echinites. N°. 492. p. 143. April &c. 1749. *Read* May 11. 1749.

Theſe *echinites* are undoubtedly moulded in ſhells, of a *genus* of which we at preſent find ſome ſome ſpecies now living in the ſeas; moſtly in the *Weſt Indies*. The *echinometra* of *Ariſtotle*, *Aldrovand*, and of Dr *Grew* (*a*), is of this *genus*. Dr *Breynius* (*b*) calls the whole *genus Echinanthus*; and Mr *Klein* (*c*) *Scutum*. *Woodward* (*d*) in his diſtribution of Foſſil *Echini*, calls them the *Pentaphylloides*, from the rays on the upper part forming a beautiful cinquefoil figure; but wrongly fixes their characteriſtics in having only one aperture, and that

* Perhaps to ſome cells or membranes belonging to the body of the *echinus*. *C. M.*

(*a*) *Muſeum Reg. Soc.* p. 139. (*b*) *Schediaſma de echinis*, p 60.
(*c*) *Nat. Diſp. Echinodermatum*, p. 20. Tab. 17. *A*, & Tab. 18. *B*.
(*d*) *Cade* of foreign extraneous Foſſils, *p.* 16.

at the baſis; in which he not only contradicts nature, but alſo the very ſpecimens he quotes in his own Collection, which have all two *foramens* or apertures, and are elegantly figured ſo by *Agoſtino Scilla* (*e*), who was the perſon that ſent them to the Doctor; and Sir *Hans Sloane* (*f*) has alſo figured and deſcribed two ſpecies of this *genus*, whereof one ſpecies is an inhabitant of our *Engliſh* ſeas.

I obſerved above, that, to my knowledge, no author has ever deſcribed *echinites* or ſtones moulded in the foſſil *echini* of this *genus*; nor even have the foſſil *echini* or ſhells themſelves been ever exhibited by any Lithologiſt, except by the above-quoted *A. Scilla*, who ſent them to Dr *Woodward*, and found them in *Malta*; to which the Doctor in his Catalogue recounts two other ſpecimens, which were dug up in *Maryland*; ſo rare are the inſtances of the foſſils of this whole *genus!*

The two *echinites* here deſcribed (as alſo ſome few other ſpecimens of this ſort, which I hear are in ſome cabinets in this metropolis) were all found in the midſt of ſome rocks, which were blown up at *Port Mahon* ſome years ago, and from whence they were all brought.

The firſt or largeſt is in the poſſeſſion of the Right Rev. Dr *Lavington*, L. Biſhop of *Exeter*; it is compoſed of a hard or ſtony arenaceous greyiſh ſubſtance, and is of an eſcutcheon or heart-like ſhape: it meaſures about $14\frac{1}{2}$ inches in circumference, or quite round the limb or edge, about two inches high from the flat or baſis to the tip of the *apex*, five inches in length at the baſis, and $4\frac{1}{4}$ in breadth. On the upper part it riſes nearly gradually from the edge quite to the *apex*. A central point, with a ſlight declining ſpace, tops the ſaid *apex*; from which ſpace the body regularly divides into five parts figured like leaves to the edge. Theſe leaves are narrow at the *apex*, greatly widen toward the bottom, and narrow a little again at their end. Each diviſion or leaf is bounded on each ſide by a row of parallel ridges, which are accompanied alſo on each ſide of every ſaid row, with two other ranges of points or knobs; all which rows do not meet or cloſe together at the lower end of the diviſion, but leave a void unwrought ſpace: a row of larger irregular knobs runs through the midſt of each leaf. From the diviſions between each leaf runs a rugged knobbed pillar, which is joined to the edge: the other parts between the leaves and the edge are hollows, or void ſpaces. The edge or *limbus* is of a thick cylindric make, runs quite round the whole body, and only has ſome ſigns of being disjoined at the one extreme of the length, or where the aperture was; the ſtone anſwering which is here extended a little cylindrically outward like an appendage, and was ſo formed by the ſtony matter being too much in quantity for the ſhell, and ſo was protruded through the ſaid *foramen*. On the outer edge of the *limbus* are ſome few irregular ſtony concretions. The baſis is flat, and is likewiſe divided into five

(*e*) *La vana Speculazione diſingannata dal-ſenſo*, Tab. 9, 10, and 11.
(*f*) *Nat. Hiſt.* of *Jamaica*, Vol. II. Tab. 242. *Fig.* 3. *& ſeq.*

parts

parts from the center, which is one of the *foramens*; the other *foramen* (as has been above deſcribed) being placed at one of the extremes of the length. This *foramen* or center is about the ſize of a ſhilling. The 5 diviſions extend to the utmoſt edge of the body, or quite over the *limbus*, contrary to the diviſions on the upper part, which extend only to it. Each diviſion is formed by a ſtony line edged on each ſide with ſtony cylindrical bodies of the thickneſs of a pin, but of different lengths, ſo as to appear like the teeth of a comb, or the gills of a fiſh; the interſtices between all which is a rugged ſtony work, and hollows pervading quite through the body to the upper part.

I cannot but think theſe five pectinated diviſions on the baſis owe their figure to ſome parts of the included fiſh; which I am more confirmed in, as I have ſeen ſome ſpecimens of the common pileated and galeated *echinites*, which have been hollowed at their *apex*, and marked ſtar-wiſe; that concavity, and the ſtellar mark proceeding from the interpoſition of the fiſh between the ſtony matter then filling the ſhell, and the top of the ſhell itſelf.

The other *echinite* I have the honour of producing before the *Society*, belongs to Mr *Edward Jacobs* of *Feverſham*. It is of a different ſpecies, though of the ſame *genus*, of a heart-like ſhape, and about one third the ſize of the above-deſcribed. This is greatly copped, the *apex* lying very high, and the five diviſions running near perpendicularly down to the edge. The upper part of this is elegantly perfect; the work is near the ſame as on the other; only that, by the perfection this is preſerved in, we obſerve that the rows of parallel ridges, which adorn each ſide of each leaf or diviſion, riſe into a kind of arched work or bridge, made up of arched cylindrical bodies, through which the middle row runs, joined or connected in a long ſtrait cylindrical ſtem, in a moſt curious and elegant manner. The baſis or under part of this ſpecimen is very imperfect, and only ſeems to differ in the center being greatly excavated or concave, anſwering to the great copping or height of the *apex* or upper part. This foſſil alſo conſiſts of a hard ſtony arenaceous ſubſtance like the other.

From the inſpection of the ſeveral hollows of theſe *echinites*, it is evident they were not immediately moulded in the ſhells, but were formed in cavities which thoſe ſhells formerly filled in the rocks they were lodged in. The rocks were apparently of a looſened arenaceous texture, and the water, *&c.* continually pervading them, rotted and deſtroyed the incloſed ſhells, and bore away their whole ſubſtance. In the ſame manner, and by the ſame means, were the ſtony particles replaced into thoſe very cavities which the ſhells formerly filled; conſequently theſe bodies were moulded exactly to the ſaid cavitities.

This remark carries a concluſion with it, if obſervation be made, that the hollows and ſolid parts of theſe ſtones exactly anſwer to the hollows and ſolid parts of the very ſhells themſelves; which, had they been moulded in the very ſhells, muſt have happened directly contrary; the

ſolid

ſolid parts of the ſhells forming hollows in the ſtone, and *vice verſa*. In all ſandy or lax earthy matter foſſil ſhells are very ſeldom found, but only the moulded ſtones; the looſe texture of thoſe ſubſtances giving free acceſs to water, vapours, and mineral exhalations, *&c.* which intirely corrode and deſtroy the ſhells buried in it.

I have taken the liberty to produce before the *Society* a recent *echinus* of this *genus* from the *Weſt Indies*, to elucidate my ſubject; as alſo two drawings done by Mr *Mynde*; *viz.* of the baſis of the large *echinite*, and the upper part of the ſmall *echinite*.

Fig. 52, 53, 54. *Fig.* 52. A view of the upper part of a curious *echinite*, in the poſſeſſion of Mr *Edward Jacobs*, of *Feverſham* in *Kent*.

Fig. 53. A view of the under ſide of the ſame *echinite*.

Fig. 54. A view of the under ſide of a curious large *echinite*, in the poſſeſſion of the Right Rev. Dr *George Lavington*, Biſhop of *Exeter*.

N. B. The upper part of this echinite having nothing remarkably particular or different, it was not judged neceſſary to give a figure of it.

A letter from Mr Joſeph Platt *to Mr* Peter Collinſon, *F. R. S. concerning a flat* Spheroidal Stone *having lines regularly croſſing it.* N°. 496. p. 535. Nov. &c. 1750. *Dated* Mancheſter, Dec 9. 1749. *Read* Nov. 8. 1750.

3. A little while ſince a man brought me a ſtone, which he found at *Ardwick*, 7 feet deep, near this town, in driving a ſlough through ſome gret-ſtone. It is what I call a nodule, of a cloſe, compact, ſmooth matter; was incruſtated with coarſer earth, or ſoft ſtone; is 3 inches and a half diameter; formed not unlike one of the *echini marini*; except the *papillæ* or ſmall protuberances, which it wants. Upon examining it, I find four white ſeams, about the bigneſs of a horſe-hair, which quarter the ſtone very correctly. The angles are exactly the ſame, and correſpond ſo well, that it would require the niceſt mathematical head and hand to draw the like.

Fig. 55.

The diameter *A B* is 3.7 inches; the ſtrait line *C D* at the bottom, or greater baſe, is .42 of an inch; that at the top of the ſtone is .21 of an inch, which make the angle *C D* equal at top and bottom, tho' of different diameters. The ſeams are like talc or ſpar. It weighs about 3 pounds. I have ſeveral nodules, but none like this. There is nothing curious in this ſtone but the lines, which I have deſcribed in the beſt manner I can. I am confident chance had no hand in forming it; and I am as certain, that no artiſt was ever concerned or able to do the like; therefore I conclude it has been ſomething formed before the Flood, and is of marine production *.

Fig. 56.

The deſcription and figures of a ſmall flat ſpheroïdal Stone, *having lines formed upon it; by* C. Mortimer, *M. D. & Sec. R. S.*

4. I lately received, by a friend, from the iſle of *Shepey* in *Kent*, a ſmall ſtone, with ſimilar lines upon it. Mine is only $\frac{7}{8}$ of an inch in diameter, of a brown colour, and of the conſiſtence of marble. As a

* The ſtone having been ſince cut in two, it was found that thoſe regular lines, compoſed of a ſparry matter, penetrated the whole ſubſtance of the ſtone quite thro', and that they grew wider as they were nearer to the centre. *See* Fig. 56. *C. M.*

deſcrip-

Pla. XI. *Vol.* X. *Part* II. *Pag.* 638.

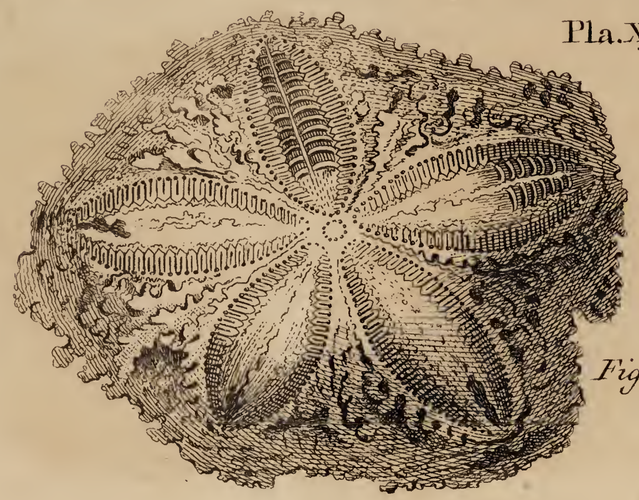


Fig. 52.

Fig. 53.

Fig. 54.

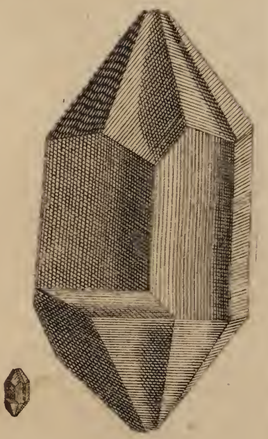
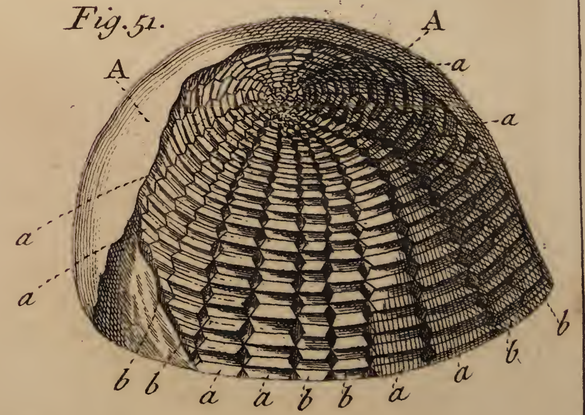


Fig. 50.

Fig. 51.



deſcription in words does not convey ſo clear an idea as an exact draw- Ibid. p. 602.
ing, I have endeavoured to give repreſentations of this ſtone in different views.

Fig. 57. repreſents the top of the ſtone, on which the lines are moſt Fig. 57.
regular, being depreſſed into the ſtone, and of the ſame colour with it. *a*, *c*, *d*, *e*, are the four principal lines, anſwering to thoſe on Mr *Collinſon*'s ſtone, and are connected, as in his, by the tranſverſe line *g h*. The line *b* is an irregulariry in this ſtone, and ſo is *f*, which are not in the other; theſe irregular, or ſupernumerary lines being continued to the other hemiſphere,

Fig. 58. or bottom of the ſtone, make the directions of the other Fig. 58.
lines very irregular, as may be ſeen in the figure; only the lines *c*, *d*, and *e*, being connected by the tranſverſe line *g h*, which here ſtands at right angles with that in *Fig.* 57.

The following figures repreſent the ſection of the ſtone through it's equator, as nearly as poſſible; only the mill cut away the ſubſtance to about the thickneſs of a ſhilling. In theſe ſections the ramifications appear quite white.

Fig. 59. ſhews the ſection of the upper hemiſphére, as Fig 59.

Fig. 60. does that of the lower hemiſphere; in both of which the Fig. 60.
letters of reference anſwer to thoſe in the other figures, ſhewing where the outſide lines abutt upon theſe ſections.

XXIII. This curious foſſil ſeems to be compoſed of a ſtony matter like marble. which has penetrated the cells of the *nautilus* while in it's natural ſtate. The diaphragms or partitions remain ſtill diſtinct and viſible. The different colour of the ſtony matter in ſome cells of a dark-brown or hair-colour, in others of a light-brown or aſh-colour, with the natural poliſh of the outſide, gives it a beautiful appearance; as it is repreſented in the figure, where it is drawn of it's natural ſize in three different views.

A ſhews the ſide view of it. *B* the fore part. *C* the back part.

It was found in *Pool*'s *Hole* in *Derbyſhire*. It's ſutures or diaphragms reſemble thoſe of ſome of the larger *cornua ammonis*; but it's ſhape beſpeaks it to be a ſpecies of *nautilus*; and it is thought to be a non-deſcript, both in it's natural and foſſil ſtate.

A beautiful Nautilites, *ſhewn to the* R. S. *by the Rev.* Charles Lyttleton, *L.L.D. F.R.S. and Dean of* Exeter. N°. 487. p. 320. Apr. &c. 1748. *Shewn* May 5. 1748. Fig. 61.

XXIV. Various have been the opinions of authors concerning the origin of the *belemnitæ*, and as various the ſyſtems and hypotheſes advanced by them in ſupport of their opinions; ſome having imagined them vegetable productions; others have taken them for the different parts of animals, as teeth, horns, bones, *&c.* in which even theſe again have differed, as to the referring them to land or marine animals; and they have been by others ſuppoſed of mineral origin, or *lapides ſui generis*. What they really are, will, I doubt, be ſtill very difficult to determine; but, as one principal objection to their being originally marine

Conſiderations on two extraordinary Belemnitæ; *in a letter from Mr* David Erſkin Baker, *to* M. Folkes, *Eſq*; *P. R. S.* N°. 490. p. 598. Dec. 1748. *Read* Nov. 24. 1748.

marine bodies (which ſuppoſition ſeems to carry the greateſt colour of probability) has been, that no marine bodies have been found adhering to them, that objection will be obviated by no leſs than two ſpecimens, from the ſame place, of *belemnitæ*, whereto undoubted marine ſubſtances are found firmly affixed; by which inſtances, as ſome further light may be thrown on this ſubject, that conſideration will, I hope, ſtand as an excuſe for my troubling you with this paper.

Theſe curious foſſils were found in a chalk-pit in *Norfolk*, from whence they were ſent not long ſince to my Father Mr *Hen: Baker*, F. R. S.

Fig. 62. *Fig.* 62. Is a *belemnites*, whoſe *apex* is perfect; the conic cavity, and the longitudinal ſeam, evidently diſtinguiſhable; which, as well as the contexture of the ſubſtance whereof it is compoſed, ſhew it to be a true *belemnites*; but on it's ſurface are placed, in their natural condition, by which I mean not at all ſeemingly petrified, or otherwiſe altered, two of thoſe *vermiculi* that are ſo frequently found ſticking to oyſters, ſcallops, and many other kinds of ſhells, when taken out of the ſea.

Fig 63. *Fig.* 63. A *fruſtum* of another *belemnites*, the *apex* whereof is broken, but the conic cavity is ſtill remaining, and ſhewn at *a*. To this *belemnites* adheres a ſhell of the oyſter-kind, which is faſtened thereto ſo ſtrongly, that they are not to be ſeparated without breaking: which ſhell, as well as the before-mentioned *vermiculi*, ſeems not altered in it's ſubſtance, but appears like a recent one, of which many are to be met with in the cabinets of the curious.

Fig. 64. *Fig.* 64. Shews the other ſide of the ſaid ſhell, wherein the *cardo* or hinge at *b* is plainly diſcernible; at *c* appears the broken end of the *belemnites*, where the radiated contexture (well known to belong to their bodies) is repreſented, as alſo the longitudinal ſeam at *d*.

As theſe ſpecimens are undeniable proofs of marine bodies adhering to *belemnitæ*, ſeveral of the curious who have ſeen them, are of opinion, that they tend likewiſe to prove the *belemnitæ* to be marine productions. It may probably be objected, that theſe ſhells might have been brought and depoſited near the *belemnitæ* whereto they are affixed, by whatever mighty change it came to paſs that productions of the ſea are diſcovered in moſt countries at great depths in the earth, and in the bowels of mountains at great diſtances from the ſea (even ſuppoſing the *belemnitæ* to be *lapides ſui generis*, and produced in the earth) and that theſe ſhells might be cemented to them afterwards by ſome mineral, ſtony, or other matter. But the following obſervations will render this improbable; for,

1. The *vermiculi* of *Fig.* 62. are not any ſpecies of the *tubuli marini*, found ſometimes recent, and ſometimes foſſil, detached intirely from every other body; but are of that ſort, which is perhaps never ſeen ſeparate,

Fig. 55.

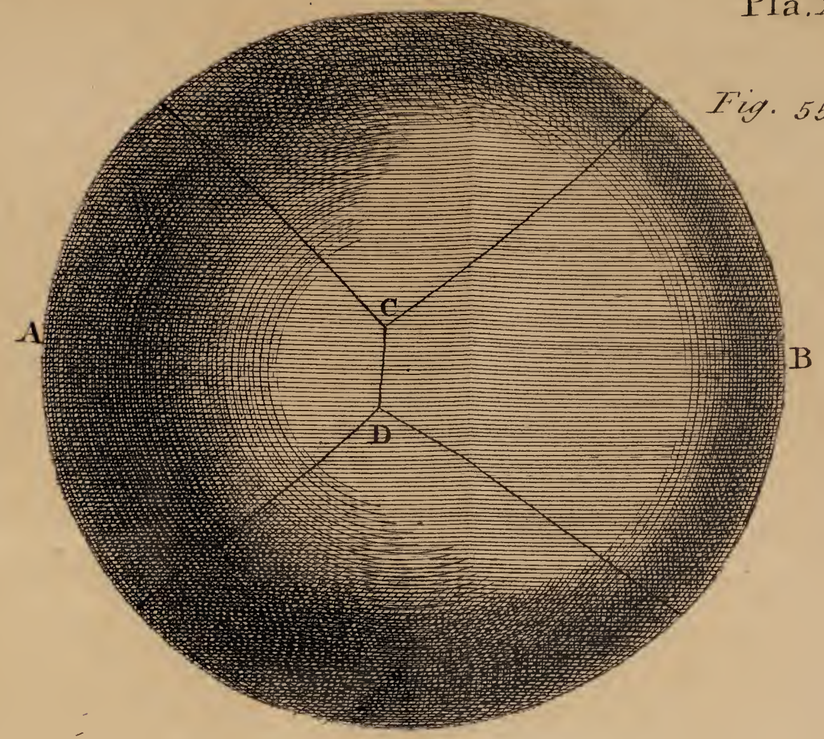


Fig. 57.

Fig. 58.

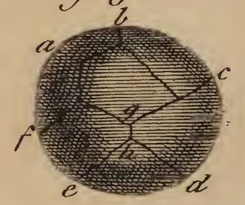


Fig. 56.

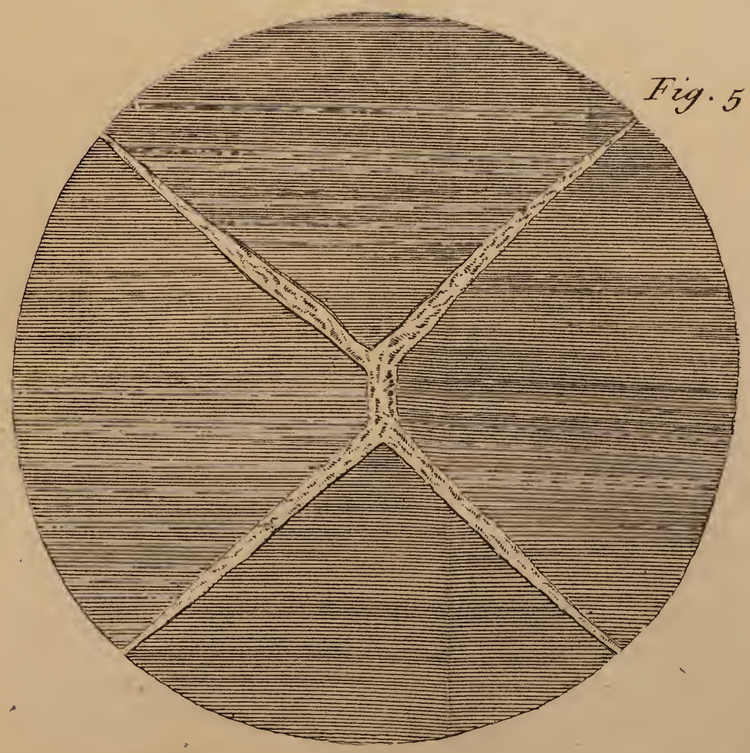


Fig. 59.

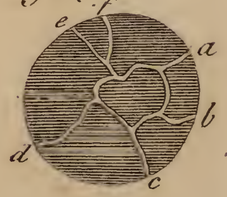


Fig. 60.

ſeparate, or in any other manner, when recent, that attached and faſtened to other ſhells or ſtones; and they are placed on this *belemnites* exactly in the ſame manner as they are commonly found on other marine bodies; *viz.* lying on their broadeſt ſide, with their ridge upwards, and glued as it were thereto by a ſhelly ſubſtance.

2. In *Fig.* 64. at *e*, is plainly to be diſtinguiſhed, that the ſhell has been faſhioned thus by the convex ſurface of the *belemnites*, in the ſame manner as theſe ſhells commonly receive a form from whatever ſubſtance they adhere to; which plainly implies, that this ſhell was faſtened to the *belemnites* when itſelf was very ſmall, and in a growing ſtate; and that the ſhell in it's growth was formed according to the figure of the body on which it was affixed: but ſuch growth could not poſſibly have proceeded any-where but in the ſea; and therefore theſe two bodies muſt neceſſarily have been in the ſea at one and the ſame time. Fig 64.

There is now but one way more, whereby theſe ſhells (ſuppoſing the *belemnitæ* to be ſtones *ſui generis*) could poſſibly become affixed to them; which is, that the *belemnitæ* might have been by ſome accident thrown on the ſea-ſhore; and that there the ſhells might faſten themſelves to them, as well as to any other ſtone. But as this muſt imply ſome former convulſion in nature, whereby they were caſt out of their natural beds upon the ſea-ſhore; and again a ſecond convulſion to carry them to the chalk-pit where they were found; ſo far-fetched an objection will, I believe, carry but little weight.

To conclude, I ſubmit to your opinion, whether the ſides of the conic cavity, whereto the oyſter-ſhell is affixed, has moſt the appearance of a ſtone or of a ſhell.

XXV. I lay before you a curious and moſt extraordinary foſſil, which was lately ſent to me for that purpoſe by my worthy friend Dr *Miles*, of *Tooting*, *F. R. S.* It conſiſts of 26 joints, which he calls *vertebræ*, and I believe ſuppoſes to have been the joints of the back-bone or tail of ſome animal; but, upon conſidering them with attention, they will perhaps rather be judged to be the ſeveral articulated diviſions that compoſe the body of ſome kind of *nautilus*, or of ſome one or other of the various ſpecies of the *ammonitæ*: which opinion is I think ſupported not only by the ſpiral figure, which they form when put together, but likewiſe by the traces or markings of ſuch-like articulations, found on ſome particular kinds of foſſil *nautili* and *ammonitæ*; one whereof I alſo lay before you, as a proof of this conjecture.

Some Vertebræ *of* Ammonitæ, *or* Cornua Ammonis; *in a letter from Mr* H. Baker, *F. R. S. to the* Pref. N°. 491. p. 37. Jan &c. 1748. *Read* Feb. 9. 1748-9.

You will obſerve all the parts of this uncommon foſſil are converted into a ſort of ſparry ſubſtance, and that they are articulated with one another in an exact and beautiful order. I have faſtened them together in two diviſions, that they may be examined more eaſily than they could be, if they were all ſeparate, and in confuſion: and indeed I am not quite ſatisfied that theſe two bundles belonged both to the ſame individual animal; if they did, ſome joints muſt be wanting that came be-

tween them, and united them together, as the two ends do not at present match: and what makes me ſuſpect they did not, is a different articulation to be obſerved on one ſide of that diviſion made up of the largeſt joints: beſides, the whole number appears rather too much, and the ſmaller joints ſeem to make up a body whoſe figure is nearly perfect.

Fig. 65. *Fig.* 65. The larger joints.

Fig. 66. *Fig.* 66. The ſmaller joints.

Fig 67, 68. *Fig.* 67. and 68. The fore and back ſide of a ſingle joint.

P. S. Dr *Miles* ſays they belong to Dr *Clark* of *St Alban*'s; that they were found in *Oxfordſhire*, and were formerly in the poſſeſſion of the late Mr. *William Becket*, ſurgeon, *F. R. S.*

An enquiry into the original ſtate and properties of Spar, *and* Sparry Productions; *particularly, the* Spars, *or* Cryſtals, *found in the* Corniſh *mines, called* Corniſh Diamonds; *in a letter to* Emanuel Mendez da Coſta, *Eſq; F. R S. from the Rev. Mr* Wm. Borlace. N°. 493. p 250. Oct. &c. 1749. *Read* Dec. 14. 1749. May 3. 1750. Sect. 1.

XXVI. I ſhall conſider ſpar here as the genus, at the head not only of all the ſpecies of common ſpar, and incruſtations of what colour ſoever, but of cryſtals and gems, which are here underſtood only as finer and purer ſubſtances of the ſpar kind (1).

There are ſeveral ſorts of theſe ſparry productions, which are carefully to be diſtinguiſhed from each other; but they may all be conſidered, firſt, with regard to their original ſtate, or what they have been; and, in the next place, as to what they now are, that is, as to form, ſize, colour, hardneſs, texture of parts, and direction of their ſhoots in the mines or quarries. Theſe particulars, ſeparately diſcuſſed, may poſſibly lead us to ſeveral probable conjectures concerning the original and moſt diſtinguiſhing properties of theſe bodies, ſuch as may afford ſome light to this perplexed and intricate ſubject.

That all ſpar has been, at one time or other, in a ſtate of fluidity, may be maintained, I think, with great reaſon, as well as ſupported by the authority of ſome of the moſt eminent Naturaliſts (2). In ſome ſpars are found ſtraws, and other light bodies; and we may therefore as juſtly conclude them to have been once fluid, as the amber that incloſes the bee. In ſome ſtones, whereon were ſparry concretions, Dr *Woodward* found fragments of ſhells, and pellicles of the ova of fiſhes; a

(1) The properties of cryſtal aſſigned by a late treatiſe (*Mr* Hill's *Nat. Hiſt. of Foſſils*), ſuch as keeping itſelf unaffected by acid menſtrua, remaining unaltered in a moderate fire, and giving forth ſparks of fire by colliſion (whereby that author diſtinguiſhes it from ſpar), are here reckoned, rather to be accidental than claſſical differences, owing to a purer ſtony juice, leſs friable and terrene than that of the common ſpar, than to any eſſential and radical difference in the *Principia* of theſe bodies. [" There is in all ſpar " more or leſs of cryſtal." *Woodward's Nat. Hiſt. Foſſils*, 158.] For many ſpars there are, which are opaque, and yet in the ſame hexagonal form as cryſtals; whence it appears, that ſpar and cryſtal do not differ in ſubſtance and nature, but in tranſparency, colour, and different degrees of purity. " Spars much the ſame with cryſtals, ſays Dr " *Plott, Oxf. p.* 98. §. 52." And *Boëtius* doubts not, but they (*viz.* Spars) are of the ſame matter with gems, *ib.* §. 53.

(2) *Woodward*'s Cat. Foſſ. Vol. I. p. 151, and 157. N°. 78. *alibique paſſim.*

certain

certain evidence, that this ſparry production was not anterior to the Deluge; for the ſtone muſt have coaleſced and hardened upon the ſhell, before the ſparry concretion could have fixed upon the ſurface of the ſtone; and as I apprehend, the Learned are now very well ſatisfied, that ſuch extraneous foſſils as are mentioned above, are not the *luſus naturæ*; but the exuviæ of animals brought where we find them by the waters of the Deluge. Wherever any number of the ſhotten ſpars occur, there may be ſeen ſucceſſive incruſtations and cryſtals fixing on other cryſtals, ſome incruſtations broken off, and ſhewing their concave baſe, ſhaped by the cuſpis or apex of the diamonds on which they were once faſtened; which ſhews, that there has been a ſucceſſion of ſeparate and diſtinct indurations. In ſeveral places we find wavy proceſſes form- Fig. 69.
ed in thin plates, on the perpendicular ſides of the rocks, by the ſpars flowing down in the ſame manner, as one wave ſucceeds another on the ſea-ſhore; to which we may add, the frequent formation of ſparry effloreſcencies, accretions on walls, and ſtalactites hanging down from the vaults and caverns of deſerted mines, evidences ſufficient of the modern date of ſuch productions. That we may the eaſier apprehend this truth of ſpars having been once a fluid (upon which much depends), it may be here obſerved, that ſomething very like this proceſs, (I mean liquors hardening into ſtone) is commonly ſeen in the effects of petrifying waters; where as ſoon as the ſtony juice meets a proper nidus of wood, reed, graſs, or the like, it will forſake it's ſtate of fluidity, and become a ſolid ſtone: why then ſhould it ſeem more unaccountable or difficult, to conceive that the ſame alteration ſhould happen in the bowels of the earth, and in larger maſſes of matter? For as the ſame cauſe will in like and equal circumſtances produce the ſame effect, ſo to produce a greater effect (*viz.* an alteration of form or motion in a greater quantity of materials), there is need only of a proportionably greater force in the cauſe; it being as eaſy for a powerful effort to produce a rock, or a mountain, as for a ſmaller force to congeal a pebble, or form the ſmalleſt gem. If it were poſſible, therefore, for us to be as attentive witneſſes of the changes which happen under the ſurface, as we are of thoſe which appear on the banks of every petrifying ſpring, we ſhould diſcover many new ſtones produced every now-and-then, which by their firmneſs appear now to have been as old as the world (3). It is indeed a vulgar miſtake to imagine, that time has added, or ſhall add, to the firmneſs of a ſpar; or becauſe it is ſo hard and compact a body, that it cannot therefore but be as old as the firſt formation of things; for ſpar becomes as hard at the firſt time of it's conſolidating, as it will be ever after, as we find by the exact ſhape, and the ſmooth ſides which *Corniſh Diamonds* make in incruſtations, and all after and ſecondary

(3) Since the writing of the following Treatiſe, Mr *Hill (Nat. Hiſt. of Foſſils, p.* 157.) by a curious chemical inveſtigation of the lapideous contents of water, ſays, "That ſtones "and minerals, formed of cryſtal and ſpar, need not be ſuppoſed all of them as old as "the Creation or Deluge; but may be, and unqueſtionably are, formed to this day."

concretions. This ſparry liquor is ſtiff and ſluggiſh, and apt to harden; but it is a liquor however, before it becomes a ſtone. Nor is this opinion ſingular, but adopted by many of the Moderns as well as Antients. *Pliny* (4), from the reſemblance that cryſtals have to water, carried this hypotheſis much too far, and thought them to be nothing more than water congealed by exceſſive cold; and *Diodorus* eſteems them no better than a concretion of pure water, aſſigning however a different cauſe, concluding them hardened by a divine heat. *Agricola* makes the *ſuccus lapideſcens* the original matter of which ſtones are formed, ſome by the heat, others by the cold (5) they meet with, during the ſtate of fluidity (6). Mr *Geoffroy*'s hypotheſis ſuppoſes cryſtal to be formed of thin equable plates, that water is the vehicle of cryſtalline parts; and when thoſe parts meet together in any quantities, the water eaſily evaporating leaves the cryſtals to form themſelves into hard, pellucid bodies. Mr *Boyle*'s opinion was, that theſe bodies were originally in a fluid ſtate (7).

Sect. 2. *Whence this fluidity.*

'Tis water that firſt occaſions, and afterwards maintains, this fluidity; and the reaſon why we find none of this ſparry maſs in it's fluid ſtate, nor ever ſee this lapideous juice, is, becauſe whilſt it remains incorporated with the water, it is not to be diſtinguiſhed from the liquor in which it ſwims; and as ſoon as ever it is deſerted by the water that circulated it in the bowels of the earth, and other neceſſary circumſtances concur to produce that change, it becomes ſtone: by water it is that the ſparry atoms are waſhed forth out of their repoſitories (8), collected into a thick, tranſparent, or opake juice (the ſtony particles attracting each other as much as the intermediate water will give leave); and as ſoon as the redundant water is drained off, or evaporated, the lapideous parts (now more at liberty) accede to a cloſer union, and are aſſiſted greatly therein, as well by the condenſing nature of cold, which compreſſes the parts, and forces them nearer one to the other, as by ſudden evaporating heats; and thus the ſtone forms itſelf, ſo much water reſting in the pores and interſtices of the parts (in proportion to the number and magnitude of thoſe pores), as is neceſſary to fix it into a conſiſtency; for, as I apprehend, there is no compound body, but by means of the Chemiſt's fire will yield ſome water; but as ſoon as all the water is thrown off, the body loſes it's hardneſs and continuity, and turns to a calx and powder.

(4) *Lib.* 37. *c* 2

(5) "*Utroque enim modo effici poteſt Lapis.*" *Ag. de Ort. Subt. Lib.* 4. *Baſil. Edit. p.* 57.

(6) *Ib. p.* 56.

(7) See *Boerhaave*'s Theory of Chemiſtry, by *Shaw*, *Not.* 120.

(8) *Woodward*'s Nat. Hiſt. of the Earth, 2d Edit. p. 189.

"Water is the only agent that educes the matter, of which they (*viz.* ſpar and "cryſtal) conſiſt, out of the ſtrata, and compiles and forms it in the perpendicular "fiſſures." *Woodward's N. Hiſt. Foſſ.* Vol. I. p. 150.

Here

Here I beg leave to propoſe a few queries.

Whether ſpar is not the univerſal glue of ſtones, diſtinguiſhed from each other by the various mixtures of earthy, mineral, or metallic particles, but all united by the ſparry liquor? for it ſeems to me, that there is ſcarce any ſand, nodule, ſtone, or ore, which either by the naked eye, or glaſſes, may not be diſcerned to have a certain portion of ſpar, clear, or opake, in it's compoſition. *Query* 1.

Whether it is not reaſonable to believe, that ſtones in all ages have been, and are ſtill forming in the earth, in ſome ſuch manner as is here mentioned, whenever the neceſſary materials and cauſe concur with proper incidents? *Query* 2.

Whether this hypotheſis is not better adapted to account for teſtaceous, and other extraneous bodies, found incloſed ſo often in maſſes of ſtone, than Dr *Woodward*'s ſuppoſition, that all ſtones were reduced into a fluid maſs by the waters of the Deluge; which waters being thoſe of the ocean, we cannot allow to have any ſuch diſſolving power inherent in them, and therefore they produce no ſuch effect? *Query* 3.

Whether there are not quarries of ſtone, which when left idle, or unwrought for ſome time, yield a freſh ſupply of ſtone in the chanels and hollows of the ſaid quarries, which had been before thoroughly cleared by the workmen (9); and whether this will not confirm the ſuppoſition, that ſtones formed ſince the Deluge, in places where ſhells, teeth, and the like bodies, were depoſited by the waters, incloſed them in their ſubſtance? *Query* 4.

Our *Corniſh* ſpars are either plain, ſimple, and unfigured, or figured into various and rectilineal ſhapes. Sect. 3. *Different appearances of Spar.*

All ſparry liquor is in itſelf ſtiff and ſluggiſh, and covets no ſhape; but, being intimately mixed with water, which is the reſtleſs agent, to diſperſe, collect, and renew all ſubterraneous nature, it moves as a fluid by the rules of gravitation, that is, from a higher to a lower poſition, till meeting with a retentive bed, the water no ſooner retires, and leaves it expoſed to a drier or colder air, than it dries, and hardens into ſtone, in ſhape and ſize, as the attraction and quantity of it's own parts determine, or the circumambient bodies will give it leave to fix and extend itſelf.

Sometimes we find the ſparry liquor ſpread into thin plates on the horizontal or oblique planes of rocks; ſometimes we trace it in ſheets down the ſides of fiſſures; and where it meets with impediments of gravel, or ſtone, it will reſemble branched limbs, clay, boughs, and ſtumps of ſhrubs; ſometimes it drops from vaults, and roofs of caves, whence it has the name of *Stalactites* (10). In all theſe caſes it is plain, that the juice had no other motion, whilſt a juice, nor appears in any *Plain Spars.*

(9) See *Addiſon*'s Travels into *Italy*, and Bp. *Burnet*.

(10) It alſo veins or granulates, or both, every kind of ſtone; and is oftentimes found to compoſe whole loads or veins, without any metallic or mineral mixture, or any particular ſhape, more than the fiſſure in which it reſted compreſſed it into.

other

other ſhape now a ſtone, than what it's own weight or gravitation, during it's ſtate of fluidity, inclined it to. In theſe uninformed rude productions, it is very plain, I think, that the juice wanted thoſe active principles (whatever they be), which enable it at other times to ſhoot into regular forms.

Fig 70. *Fig.* 70. is a ſpar pebble, it's ſurface about the roughneſs of the peach-ſkin, incloſed in part of it's ſocket, which is alſo of ſpar, angular, and puculated (which latter property is rarely met with): the coat or ſocket is mixed with ſolid white mundic, and cockle; which laſt (or the ſame principle, which throws cockle (11) into this oval figure) ſeems to have determined this ſpar to it's ſingular, *viz.* orbicular ſhape; for it is obſerved, that where cockle is plenty, ſpar-nodules round as muſket balls, and black, in ſockets of the ſame colour and ſubſtance, are frequently found. But as this pebble was not black, as cockle always is; it may therefore be queſtioned, whether the ſhape of it may not be owing to ſome metallic (*viz.* iron or copper) principle, rather than to cockle, and whether cockle itſelf be not more probably indebted to other powers for it's orbicular nodules, in ſuch ſheaths, than derive them from any inherent activity of it's own. The exterior of the ſhell or ſocket has a thin incruſtation of gritty cinereous mundic. It came out of *Wheal Royal* mine, in the pariſh of *Cambron*, *Cornwall.*

Of incruſtations. The next appearance of ſpar bodies, which I ſhall here take notice of, is that of incruſtations; theſe ſometimes make one continued ſheath, lump, or maſs, and incloſed in them we find *Corniſh Diamonds*, grains of tin, and other adventitious bodies, plainly of a different texture and colour from the cruſts which ſurround them; ſo that incruſtations muſt be cautiouſly diſtinguiſhed from the entire ſheaths, or laminæ, which compoſe the column of hexagonal cryſtals, and which are really formed at the ſame time with that column, whereas incruſtations are additional, and after concretions made on the before ſettled original grains and columns, (12) ſometimes theſe incruſtations, are but ſprinklings of the cryſtal drops, without any continuity, or mutual contact; and in this caſe, when the pearly drops are themſelves bright and tranſparent, and the ſtone they fix on, of an agate colour, or any lively oppoſition, the incruſtation is exceedingly beautiful. On one ſhotten bliſtered ſpar,

Fig. 71. I find the incruſtation white, not pellucid, flowing in parallel threads by each other in ſeveral places paſſing from one tubercle to another,

(11) Cockle is a black, ſhining, light ſtone, free of all metal, different from mock-lead, common in the tin-mines of *Cornwall.*

(12) Incruſtations are ſo many evident proofs of ſtones not being formed all at the ſame time; for many *Corniſh Diamonds*, and columnar ſhoots of tin, cubes of mundic, and grains of lead, are often broke off from theſe their incloſures; but the angular cavities, with their ſtrait edges and ſmooth ſides, ſtill appear in the incruſtation; which plainly ſhews, that the diamonds and tin-ſhoots, *&c.* were firſt formed and hardened, and then ſurrounded and united into one lump, by a ſucceſſive induration of theſe cryſtal or ſpar cruſts.

without

without touching the interſperſed hollows; by which I conclude, that this ſpar was fixed on the perpendicular ſide of a fiſſure; that the juice of this incruſtation was of the *ſtalactites* kind, and, proceeding from the ſame cauſe, deſcended in a ſimilar direction.

Fig. 72. is a bunch of ſemi-pellucid ſpar, ſhot into reclined cones, Fig 72.
making an angle of 30°, with the ſurface of the ſtone; the ſides of theſe cones are a very curious fretwork of little ſpires or briſtles, many of them ſharp as the ſmalleſt needle, and pointing nearly in the ſame direction, as the cone on which they riſe. The ſurface of theſe ſhoots is of a ferrugineous tint, but their inner ſubſtance pellucid, very little ſhort of that ſpar, which for it's clearneſs is called Cryſtal, and more tranſparent than many hexagonal ſhoots; 'tis the only one I have ſeen of it's kind.

Fig. 75. is an aſteriſk of the cleareſt ſpar; it's ſhoots or rays are hexa- Fig 75.
gonal, ſwelling, or gibbous, in the middle; their ſides not plain, or of one level ſurface, as our *Corniſh Diamonds* generally are, but ridged near the edges, and ſomewhat hollow, but not uniformly, in the middle; the points or terminations were entire and ſharp, but not to that degree aculeated as the former conic ſpar; but it is very plain, that theſe ſpires never had any hexagonal apices: the undermoſt ſhoots ſpread horizontally; but the other riſe gradually, making a greater angle, till the middle ones make nearly a right angle with the baſe, which has a ferrugineous circular ſpot in the middle, from whence the rays regularly proceed on every ſide. Fig. 76.

Theſe are ſome of the moſt ſingular ſpars which have reached my obſervation; but the general ſhape of our figured ſpars is hexagonal; and theſe hexagons either conſiſt of a ſhaft or column, and a point with the ſame number of ſides correſpondent to the column, or are only Fig. 77.
points, that is, pyramidal hexagons ſtuck on at their baſe, upon the ſurface of their ſtony beds. Fig. 78.

In a thin cake or lump in my poſſeſſion, one half of the ſpar ſhoots from one ſide, the other half from the other; and ſo incruſts the planes of both ſides with hexagonal *apices*. Fig. 79.

Here the ſpar (as appears by the tendency of it's fibres, when the interior texture is examined) ſtruggled to form ſtems or ſtalks to theſe *cuſpides*; but either the effort was not vigorous enough, or, through the impurities of the juice, the ſparry or cryſtalline principles had no room to extend or protrude themſelves into the ſhape they ſeem inclinable unto; ſo they lie blended, and their lineaments ſcarcely to be diſtinguiſhed from the general maſs.

Some ſpars riſe out of the general ſurface, into large orbicular bliſters, thick ſet with hexagonal points diverging, as rays from a center. Fig 80.

Theſe bliſters or protuberances are in other ſpars ſubdivided into numbers of other ſmall, orbicular excreſcencies, and the *cuſpides* very ſmall, but, like the pointed ſhoots of moſt *Corniſh* ſpars, hexagonal. Fig. 81.

At

At the root, or where they join to the rock, theſe globular maſſes ſhew in what direction the juice exerted itſelf, ſpringing commonly, as from one general center, and extending itſelf equably on every ſide. When the juice is ſimple, and of one ſort only, the rays are continued
Fig. 82. from the center to the extremities. But when the juice is of two or more different mixtures and impregnations (which will generally appear from the different colours and degrees of tranſparency), then the effort is various and ſucceſſive, protruding the juice according, and in proportion to the different activity of the *ſtamina*, of which it conſiſts; and in both theſe caſes I find the coarſeſt and moſt terrene part of the ſparry lump next the center, and the moſt tranſparent and pureſt ſhot forth, to form the pyramidal *cuſpides* of the circumference. Though the effort is various and multiple, the ſeveral juices preſerve a paralleliſm to each other, and to the extremity, each juice proceeding no farther than it's own impregnation would carry it; and therefore ſettling in parallel liſts or lines behind each other, and their angles leſs and leſs perfect, that is, becoming more obtuſe, till you come to the rock, or lifeleſs lump of ſpar, which the effort had no power to move, and thro' which the impregnated and purer juices eſcaped, according to their degree of mobility: the baſe of one of theſe orbicular lumps, which has ſeven diſtinct liſts or fillets one within the other, beſides leſſer liſts,
Fig. 83. marked (*h*), will explain what is here ſuggeſted †.

Theſe hexagonal points do not always ſit cloſe to the body of the rock, but are as often found mounted upon columnar ſhoots of the ſame number of ſides; and theſe are what are commonly called *Corniſh Diamonds:* they are generally found larger ſomewhat at the baſe, where they faſten on the rock, than at the top, where they ſupport the *cuſpis*.

Fig. 84. Some of theſe ſhoots have alſo hexagonal points at each end, and are
Fig. 85. ſometimes found ſingle, that is, detached, and without a root, as the Naturaliſts ſay; but I have them alſo in lumps fixed ſide by ſide, but
Fig. 86. in no parallel direction (13).

This is the general and moſt common appearance of our *Corniſh* figured ſpars and cryſtals, *viz.* either hexagonal points on the rock, or common ſpar, or fixed on ſhoots or columns of the ſame figure; but we muſt not imagine, that ſpar aſſumes no other ſhape, but what has been here mentioned. There are alſo trigonal and cubical ſpars; but of theſe ſorts I have not yet ſeen any in *Cornwall*; however, as our

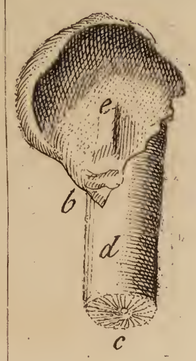
† *a*. The cleareſt cryſtal.
b. Sup-pellucid, inclining to purple.
c. Flock-white, not pellucid.
d. Large fillet of purpled ſpar.
e. Flock-white.
f. Small fillet of purpled ſpar.
g. Flock-white.
h. Liſts of ſpar leſs diſtinct.
i. That ſide on which the effort was faint, and the ſhoots ſcarce perceivable.

(13) Theſe ſhoots are not always ſtrait, but are found ſometimes bent or crooked; but as this deformity is owing to ſome accidental interpoſing force or obſtruction, during the time of forcing, it will not, I apprehend, make the body of a different ſpecies, as long as all the other common properties are continued.

obſervations

Fig. 61.

Fig. 64.



C

A

B

Fig. 63.

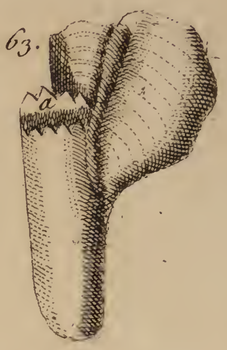


Fig. 67.

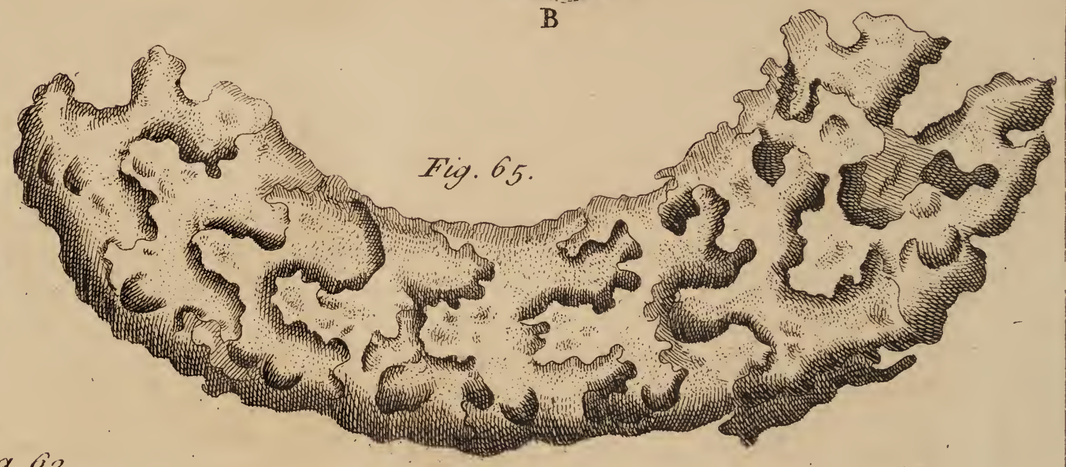


Fig. 65.

Fig. 62.

Fig. 66.

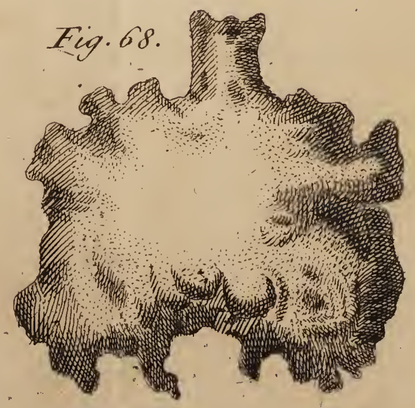


Fig. 68.

obſervations in this inquiſitive age are daily growing more extenſive, it is very probable, that new and undeſcribed ſhapes of ſpars may often fall under our notice: what I have here mentioned ſeem moſt worth notice; but to pretend to number them all, would be very extravagant; for they are varying every day upon our hands, and new mines throw forth new forms, according to the different combinations of their ſolids, and the impregnation of their waters.

Sect. 4. *Whence the hexagonal ſhape of Spars.*

It has been obſerved before, (*Sect.* 3.) that water and the ſparry juice, conſidered ſimply, without any other aid, will produce only the irregular, ſhapeleſs maſſes of ſpar, incapable of any activity, or ſtruggle towards ſhape and figure, and determined only by the common principle of gravitation, to that poſition in which we find it: we muſt call in therefore the aſſiſtance of ſome other principle, to account for the rectilineal regular phænomena of theſe ſparry productions.

As the general appearance of our *Corniſh* ſpars is hexagonal and uniform, there muſt be ſome one general principle to which this great uniformity is to be referred. If theſe ſpars owed their form to any metallic principle, that metal would be found, when the fluor was thrown off by fire: but the contrary is apparent, the cleareſt ſpars yielding no metal. And, indeed, it may here be obſerved, that when the ſtony juice meets and coaleſces with any metallic particles, that juice ſhoots not into it's natural form, but by tin is forced into priſms, and various *ſpeculums*; by lead, into cubes; and by other metals into other forms: ſo that it never retains it's hexagonal ſhape, but when free of metals. As therefore there is no metal in our regularly-figured ſpars, we muſt have recourſe to another origination; and ſalt, as I take it, is moſt likely to be that active principle, by whoſe force the fluid in which it is mixed, be it pure water, or lapideous juice, is made to ſhoot forth into regular rectilineal maſſes, agreeable to the original ſhape and figuration in which theſe ſalts were firſt created. 'Tis by the force of ſalts that liquid bodies are thrown into all the geometrical planes, angles, and more compounded ſhapes, the variety of which is no leſs ſurprizing, than the conſtancy and uniformity of each particular ſpecies; the ſame ſalt ſhooting ſtill into the ſame figure (as is plain from all artificial cryſtallizations), when not ſtreightened in room, or otherwiſe determined by heterogeneous mixtures. To produce ſalt from any liquid body, two things are requiſite: firſt, that the redundant liquor, in which the ſaline particles are kept too much diſperſed, and too remote to attract each other, be diſcharged (which is uſually performed by evaporation), and that the remainder be expoſed to a colder air. This ſimple plain proceſs will produce all the varieties of cryſtallization; the ſalts contained will ſhoot into their peculiar forms, pointing forth their darts, regular planes, or ſpires, into ſuch figures as are proper either to their native or compounded ſalts. From this eaſy and inconteſtable procedure of liquids into figured and ſolid bodies (to which nothing more is required than heat and cold), may it not appear probable, that ſome-

thing like this has happened, and does ſtill happen, among our ſpar-loads in the mine? For inſtance: when the juice of ſpar, impregnated ſtrongly with ſalts, which have been from time to time imbibed, is ſufficiently drained from the water (which not only collected the ſparry maſs, but kept it in a fluid ſtate), either by natural heat, ſo common in mines, or by the water's running off into crevices, where the ſtiffer ſtone-juice cannot follow it; in other words, when the water deſerts the ſpar; the ſpar, as ſoon as a colder air ſucceeds (14), ſhoots, and is protruded into figures by the ſalt which it contains (15); and thus it happens that we have ſuch figured bodies from the ſpar, which, without thoſe ſalts, would ſhew us no ſuch ſhoots as we call *Corniſh* Diamonds, but fix quietly into ſheets, and even plates, or drop down wherever it's own weight would carry it.

What ſort of ſalt it is, which inclines ſpar to this hexagonal form, is the next thing to be inquired into; and moſt probably will appear to be that of nitre, if we conſider that the reſemblance betwixt the figure of what we call *Corniſh* Diamonds, and that of the pure unmixed nitre, is ſo great, that no two things can be more exactly alike. "The known "figure of nitre, ſays *Grew* (*Coſmol. p.* 15.) is a ſexangular priſm." "Particulas nitri *Liſterus* deprehendit ſexangulas, tenues, longas, late-"ribus parallelogrammis, & ex altera parte in pyramidale acumen "deſinentes." *Phyſ. Cler. 8vo, tert. Edit. p.* 150. This exact reſemblance is ſufficient to make us conjecture, that theſe ſparry productions may owe their general figure to a nitrous ſalt, which exerted itſelf at the time when the juice of ſpar became ſtone; and I ſhall endeavour to ſupport this conjecture only by one authority, which is that of the curious, and, in the Studies of Natural Hiſtory, indefatigable *Linnæus*, which he favoured me with in anſwer to ſome queries, jointly with the opinion of the preſent Dr *John Fred. Gronovius* of *Leyden*. "The "origine of thoſe cryſtals (16) is a moſt intricate thing; but you may "conclude—*quod omnis cryſtallizatio a ſale, quod cryſtalli gaudent figura* "*nitri, quodque omnes generentur in cavo: hi (viz. cryſtalli) quo magis* "*ſimplices, eo magis puri & pellucidi: hinc nitro originem debent, quem-*"*admodum gemmæ iſtæ, quæ priſmaticam nitri figuram exhibent.*"

As nitre may be reaſonably conjectured to give the ordinary and general hexagonal figure to cryſtals, it may be as juſtly inferred, that when they depart from this uniformity, it is owing to ſome mineral,

(14) "*Cryſtallus eſt ſuccus, quem frigus intra terram conglutinavit.*" Agric. p. 282.

(15) Mr *Boyle*'s opinion is, that ſuch ſtones, (*viz.* Spars and Cryſtals) were originally in a fluid ſtate; that the figure of them is determinate and geometrical, like the cryſtals produced by alum, nitre, vitriol, in water; and their texture like the congelations of ſalt produced in cryſtallization by cold.

Grew (*Coſmol. p.* 14.) after talking of the regularity of forms, and the ſalts of bodies, proceeds thus: "Arguing (ſays he) that the atoms of the lapidific, as well as of the ſaline "principle, being regular, do therefore concur in producing regular ſtones."

(16) *Corniſh* Diamonds, ſent to Dr *Gronovius* from *Cornwall*.

earthy,

earthy, or metallic mixture, ſome heterogeneous ſalt, which impedes the nitre in it's ſhooting, and turns it into trigonal, cubical, conic, or other unuſual figures.

The next thing to be conſidered, is the cauſe to which the different ſize in which theſe bodies do appear may be owing. Some cryſtals are ſaid to be a cubit high: *Livia Auguſta* dedicated one in the *Capitol* of fifty pounds weight (17); and Dr *Iſaac Lawſon*, late phyſician to the army in *Flanders*, informed me, that he ſaw a cryſtal in a foreign mine, with it's edges well preſerved, which he believed might weigh about 200 pounds. Dr *Woodward* (18) reckons, among his *Corniſh* diamonds, a ſingle column or ſhoot very large, if it be three inches in length, and 1½ inch in diameter near the baſe. The largeſt I have yet ſeen in *Cornwall* weighs ſomewhat more than 3 pounds, is about 10 inches in girt at the largeſt end, and more than ſeven high; from which ſize there are of all degrees, down to the bigneſs of a ſmall pin. As the ſize of this laſt mentioned is very unuſual, I have given a drawing of it. Sect. 5. *Of their ſize.* Fig. 87.

The largeſt proceed out of a large courſe or load of ſpar; but the ſmalleſt of all from ſmall bits or lumps of ſpar; and the ſmall pyramidal *apices* are generally ſtuck on upon the ſide of the large ones, ſometimes in diſtant ſpangles, other times in thin and broken incruſtations. Now the cauſe of theſe different ſizes ſeems to be this: wherever great maſſes of the ſparry juice have happened at the ſame time to be in a ſtate of fluidity, the exuberant water drained off ſuddenly, and conſequently left cavity ſufficient for the ſalts to extend themſelves, there the great quantities of nitrous ſalts mixed with the lapideous juice incline it to ſhoot vigorouſly, and form large cryſtallizations; and from proportionably leſſer maſſes, whilſt they are indurating, proceed leſſer diamonds. If the whole maſs be impregnated with nitrous ſalts, the whole ſurface of the rock ſhall riſe into points or ſpires (19), according as the maſs is more free or more ſtubborn to comply with the agitation. If the nitrous ſalts are not intimately mixed, but ſwim in clouds and bunches, thoſe lumps only, where the nitre is, ſhall be ſhot, and the reſt be plain. This is the caſe when the ſparry juice gets into it's proper *nidus*, or reſts in it's fiſſure. But where ſmall ſcattered quantities of this ſtony juice circulate in the ſubterraneous waters, ſome trickle down the ſides of ſtones and fiſſures, and already-formed diamonds, and, ſticking in little globules, form a cruſt by juxta-poſition, whoſe points are ſexangular, their columns ſhort, ſometimes crooked and unfiniſhed. At other times water, charged with theſe ſparry juices, falling from higher into lower parts of the mine, cannot but be daſhed and diſperſed about the cavity in all directions; and thus it is, perhaps,

(17) *Pliny, lib.* 37. *c.* 2. Sir *Hans Sloane* has one block of cryſtal which weighs between 40 and 50 pounds, and another about 20, quite clear and regular. *C. M.*

(18) *Catal.* 158. *f.* 98.

(19) See *Fig.* 72, 75, 79, 82.

that the diſtant ſpangles, like the dew or miſt that riſes from a caſcade ſprinkled on the ſurfaces of all bodies in their reach, are there congealed, and ſhot by their ſalts.

Sect. 6. *Of their colour.*

Spars are of different colours, and different degrees of tranſparency; ſome yellow, ſome reddiſh, brown, green, purple, black, ſome of a cloudy fleecy white, ſome freckled with little ſpecks of various colours and magnitudes, and others of a water not inferior to the pureſt cryſtals. The yellow is ſuppoſed to be indebted for it's tinge to ſulphur and iron, or lead, or both; the red to iron, and perhaps *goſſan*, that general companion of copper; green, to the ſolutions or ruſt of copper; copper will alſo probably impart it's purple (for of that colour we find ſome of our moſt beautiful copper ores) to the juices near it: black may poſſibly be indebted to copper alſo of like colour, to tin, or the particles of coal; but the moſt tranſparent owe that advantage to the purity and ſimplicity of the juices of which they are formed. What this purity is owing to, cannot be ſo eaſily determined. Some think to percolation, or ſtraining through the pores of other bodies, the lapideous juice depoſiting the ſediment and impurities, which it may have contracted in it's paſſage. Now, it is not impoſſible but that the water, and that liquid ſpar, of which theſe bodies are principally formed, paſſing by their own weight through a ſoft, porous, ſandy ſtone in the *Oxford*, *Bath*, and other-like quarries, may undergo a change for the better, and acquire a greater degree of tranſparency; but it cannot be ſo with our ſpar, on which we find the cryſtals above-mentioned: for, beſides that theſe cryſtals are found on both ſides the ſtone, (which, in the procedure of percolation, could never happen), and in very large ſhoots, our ſpar will no more tranſpire or exude than glaſs, it is of ſuch conſiſtency and hardneſs: ſo that whatever filtration has happened to theſe cryſtals, muſt have befallen them during a former percolation, before they reſted in their preſent beds, not from any ſweating through that bed in which we find them, as Dr *Plott* imagines (20).

Fig. 79.

Cryſtals therefore, it is certain, owe their tranſparency and purity to the ſimple ſtate of the juices that form them; but to what that ſtate and condition is owing is uncertain. Whether it may be to ſome purifying menſtruum or ſpirit, that precipitates every kind of ſediment, I do not preſume to ſay: I ſhall only obſerve, that in *Cornwall* the cleareſt diamonds are for the moſt part found in a dry, lax, ſandy ſoil, where no dirty or dark-coloured loam, mineral, or opake ſtones prevail: ſo we may conclude in general, that if, during the fluidity of theſe bodies, no metallic or mineral fume, no duſt, clay, or ſand, was imbibed, the water and lapideous juice make up a clear pellucid maſs. If the caſe was otherwiſe, whatever impurities the waters contracted, and had not at the time of forming diſcharged, are ſtill to be ſeen in the ſtone.

(20) *Oxfordſh.* p. 98. and *Ramundus in Alonzo Barba*, p. 36.

As

As to the hardneſs of our *Corniſh* cryſtals, all I have to obſerve is, that they cut well into ſeals, when they have no flaws: their natural points alſo will cut glaſs; but not freely or deep; in which particular they fall much ſhort of the true diamond. Sect. 7. *Of their hardneſs.*

Of this I ſhall not pretend to aſſign any other reaſon, than that the true diamond ſeems to have more lapideous juice included, and more intimately and congenially united under an equal ſurface, than any other body in the world. It has alſo very little ſalt in it, as Dr *Grew* obſerves (*Coſmol. p.* 14.); and his opinion is confirmed by it's being found in ſuch ſmall maſſes; and by it's great weight it can have little water; both which obſervations are ſupported by it's great reſiſtance, and almoſt immutability in fire: ſo that the true diamond has little ſalt, and little water, conſiſting almoſt entirely of ſtony juice concreted; to which properties it's great hardneſs may therefore be attributed: whereas in our diamonds there is much ſalt, and much water (comparatively ſpeaking); which two ingredients, mixed with the lapideous juice, may incline thoſe bodies to be more friable and tender, and deprive them of that hardneſs, which a leſs-reduced lapidific juice would certainly have had. This ſeems to me the real cauſe of the true diamond's hardneſs, and of our falſe ones falling as much ſhort of it in this particular, as in luſtre: but I muſt acknowledge, that, for want of ſufficient experiments relating to both, I cannot decide peremptorily. However, by weighing the *Corniſh* diamonds in water, I find they are generally to the weight of our common water, at a medium, as $10\frac{1}{2}$ is to 4; and I apprehend, that if they had more lapideous particles, they would weigh more, as they find the true diamond really does *. I find alſo the cleareſt and brighteſt *Corniſh* diamonds weigh much heavier than the other which are more ſhady and opake. That they have much ſalt alſo, may be concluded from their being projected ſometimes into ſuch large, regular, hexagonal columns. *Conjectures relating to the properties of the true Diamond.*

There are ſome little varieties in the texture of our *Corniſh* diamonds, which are ſometimes to be obſerved in their broken ſides and edges; but always, and more diſtinctly, in their baſe. Some are uniform, of one colour and tranſparency throughout; ſome have hexagonal ſheaths deſcribed one within another, as in *Fig.* 88. In the firſt caſe, the ſhotten juice was of one and the ſame nature and conſiſtency, equally impregnated, and the production of one effort; in the latter caſe, where the ſheaths are one within another, it is alſo the product of one effort or ſhoot, the concretion of one and the ſame time, as the tremulous undulations upon the ſurface of water, on throwing in a ſtone, are all the offſpring of one force, tho' the firſt be ſtrongeſt, and the reſt gradually fainter and leſs diſtinct: but the juices being differently mixed, gave way to the effort in proportion to their ſenſibility of the impreg- Sect. 8. *Of the texture of Corniſh Diamonds.* Fig. 88.

* The weight of *Cryſtals* to water is as $2\frac{1}{2}$ to 1. of *Diamonds* as $3\frac{1}{2}$ to 1. See theſe Tranſ. N°. 488. p. 451. *C. M.*

nation,

nation, the moſt agile flying off to the greateſt diſtance from the center, and the moſt mixed, coarſeſt, and moſt opake, remaining neareſt the center (21). That this is the true proceſs, and that thoſe different ſheaths are the produce of one effort or birth, and not formed at different times, and in ſucceſſion, as *Steno* and *Aldrovandus*, and ſome others, ſay (22), is plain, I think, from the two following obſervations. If the juice which forms theſe ſheaths was impregnated and ſhot at different times, it would not form in ſheaths round what appears to be the central or maſter-ſhoot, but would project itſelf into it's natural hexaedral figure. If it were not impregnated, but mere ſpar only, without nitre, or any other active principle, it would form itſelf, as the ſtalactites, in a pendulous undulating direction, drop or wave upon wave; and in this caſe theſe ſheaths could never ſurround, in that neat and exact manner, the central ſhoot; but would be found only on the under part of that ſhoot, where it's own gravity would inevitably carry it: for it would be obſerved immediately, that theſe diamonds in the mine point forth in all directions; which muſt conſequently prevent every unimpregnated juice from hardening into a regular uniform ſheath.

It is alſo to be noted, that theſe ſheaths are often found broken and interrupted: but this cannot be owing to their being *gemmæ inchoatæ & non perfectæ* (23), for the above-mentioned reaſons, but to the inſufficiency and want of juice, or to ſome accidental impediments of ſtone, earth, or ſand, which make thoſe breaks, and prevent it from forming a complete ſheath.

Sect. 9 *Of their direction in the* Mine.

Not long ſince I went with a friend or two into a mine called *Pillion Erth*, in the pariſh of *St Juſt*, on purpoſe to ſurvey the *Corniſh* cryſtals in their natural ſituation. The cave, to which we were introduced, was not much larger than a common baker's oven, and much of that figure. We had two candles with us, by means of which we ſaw the roof, which might, in the middle, be about five feet high from the floor; in the other parts not ſo much. The roof was the moſt ſurprizing piece of fretwork imaginable, and conſiſted intirely of ſpar ſhot into *Corniſh* diamonds, of which the large one (*Fig.* 87.) was a part. I could not diſcern any coveting a poſition exactly perpendicular to the horizon; but in every indifferent direction they pointed forth very plentifully of ſeveral ſizes; ſometimes in groupes and cluſters, ſometimes ſingle, now croſſing each other, and now ſtanding by each other with parallel ſides: ſome were ſmooth, ſhining and clear, others rough and opake; ſome veined with red, like porphyry; others ſpeckled thick with the ſmalleſt ſpots of deep purple, and a bluiſh caſt: but the fineſt of all were thoſe which had innumerable little diamonds or ſparks (of

Fig. 87.

(21) See *Fig.* 83. and it's deſcription, *p.* 648.
(22) See *Plott's Oxf.* p. 98.
(23) As *Plott, ibid. ut ſupra.*

the

Fig. 69.

Fig. 70.

Fig. 71.

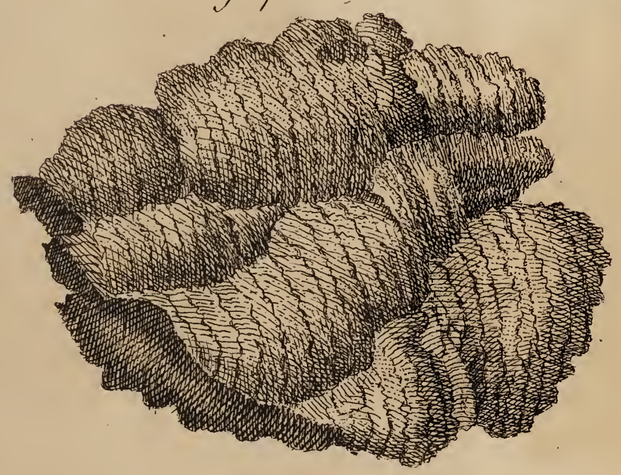


Fig. 72.

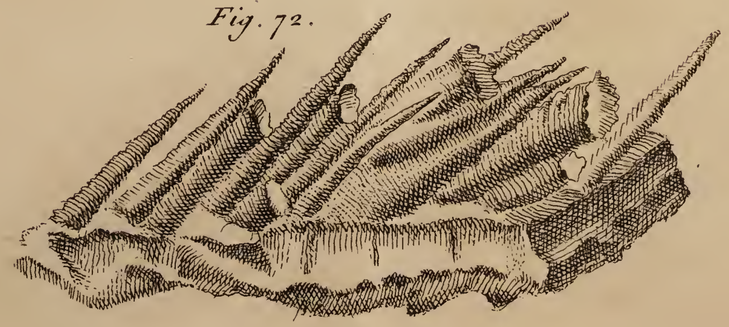


Fig. 75.

Fig. 76. & a Base of Fig. 75.

Fig. 77.

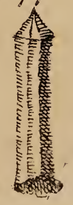


Fig. 78.

Fig. 79.

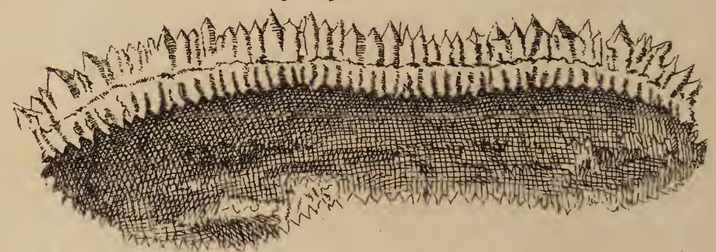
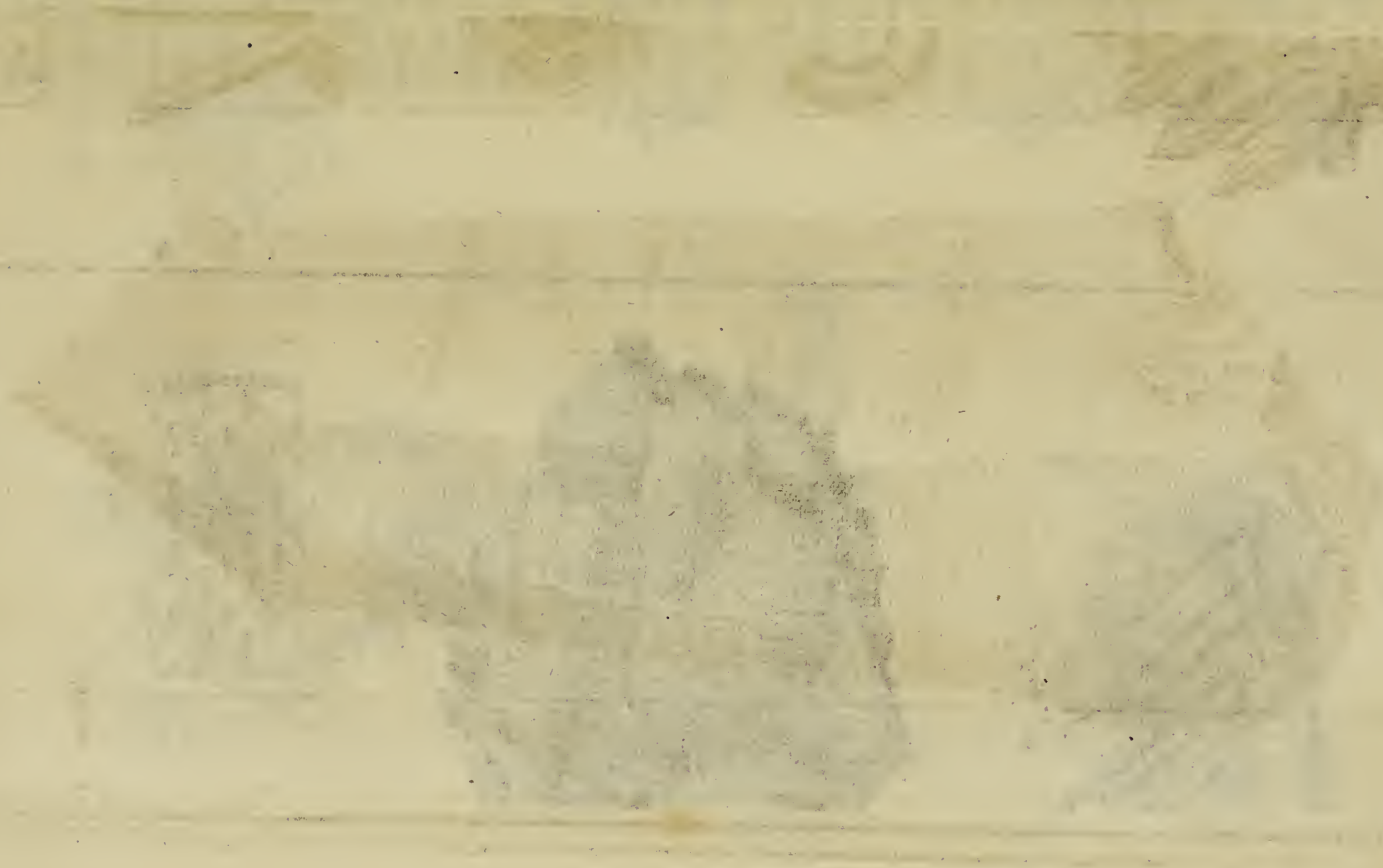


Fig. 80.

Fig. 81.



Pla. XV. *Vol.* X. *Part* II. *Pag.* 654.

Fig. 82.

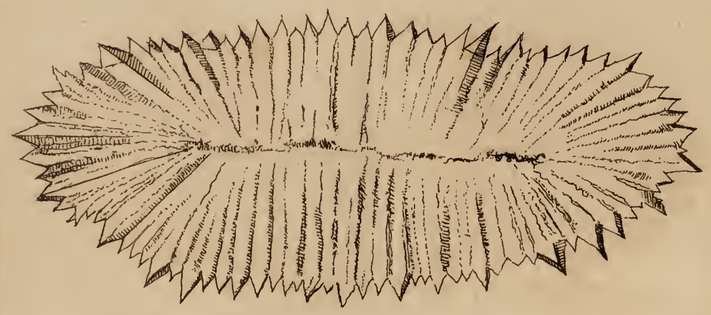


Fig. 83.

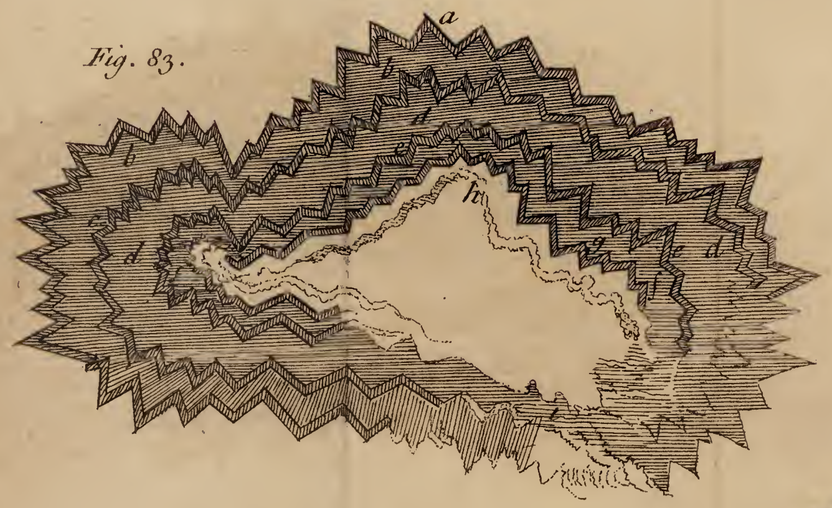


Fig. 86.

Fig. 85.

Fig. 84.

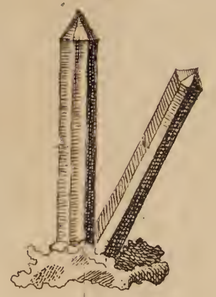


Fig. 88.

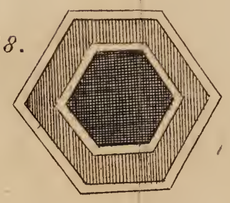


Fig. 89.

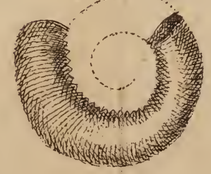


Fig. 90.

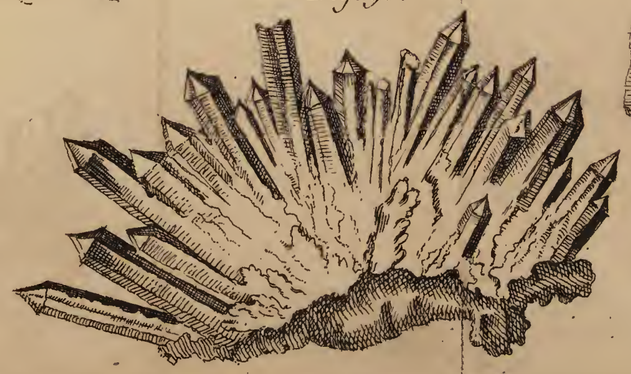
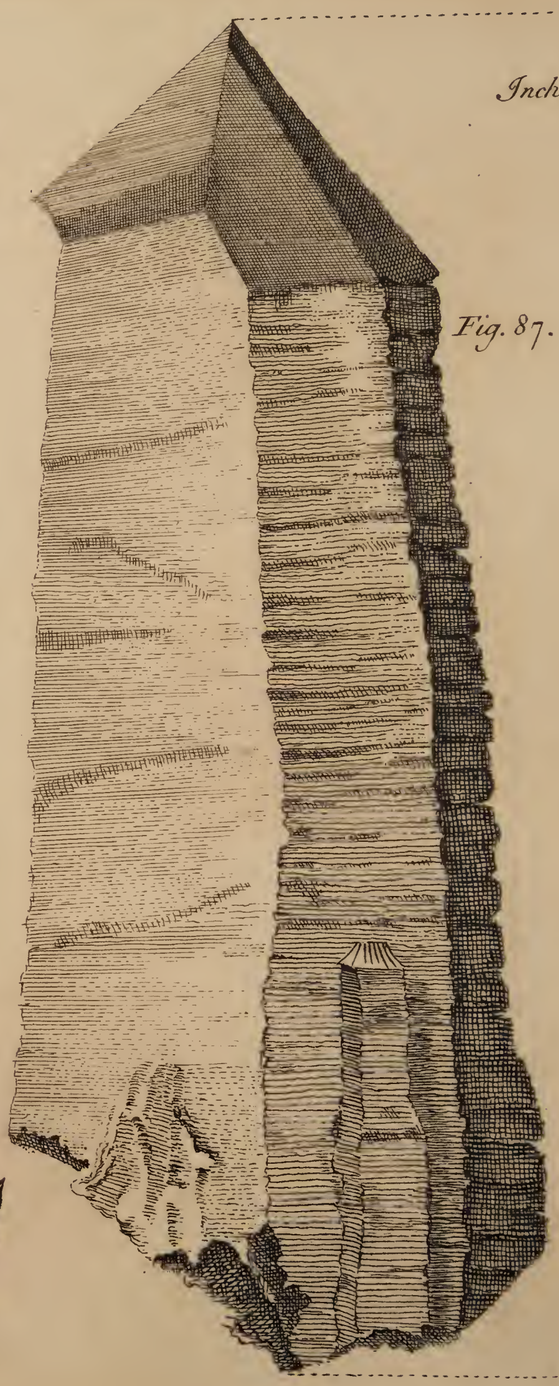


Fig. 87.



the cleareſt water) ſtuck upon their ſides, and, by means of the candle, had a luſtre not to be conceived. We gazed here in this incommodious, but beautiful little cave, till we could no longer hold up our heads; and then crept out as we came in, upon our knees.

Now, among the great numbers of ſparry productions which I ſaw in this mine, I could not obſerve, but that they pointed indifferently in all manner of directions; which I ſuppoſe owing to the great unevenneſs of the ſurface on which the baſes of theſe little columns were fixed; and for the ſame reaſon I doubt not, but in all concave beds the points converge, as in all convex they turn the contrary way, and diverge, as from a center: ſo that the natural cauſe of theſe different directions is probably no other than the accidental form of that general maſs from whence theſe ſhoots proceed. I would ſay, that as the figure and regularity of the lapidific impregnated matter happens to be when drained of all ſuperfluous moiſture, and by the cold, heat, or dryth, diſpoſed to ſhoot, ſo will the tendency of the ſhoots, both column and point, be. If the concave part (for inſtance) of the *voluta* of the *cornu-ammonis* be ſet with cryſtals, their points muſt tend inwards to the center, where the tail ends, being thereto compelled by the regular contour of the ſhell; and indeed they are ſo in fact, as ſee *Fig.* 89. But if the cryſtal juice chance to fix on, or proceed from a convex bed (to which it's *nidus*, and other cauſes, may contribute), or be itſelf an orbicular lump, and equally impregnated on all ſides; then will the fibres ſpring as from one common center, and at their extremities point forth their *cuſpides* in a circular figure, as in *Fig.* 90.* Fig. 89. Fig. 90.

If there be a thin plate of ſpar equally expoſed to cold on each ſide, and having equal room and force to ſhoot, it will throw forth it's points on each ſide; and the line from whence the fibres began to ſpring ſhall be exactly in the middle of ſuch plate of ſpar, and the figures ſhall cut the ſaid line at right angles, as in *Fig.* 79. whence it ſeems reaſonable to conclude, that the direction in which theſe ſparry productions ſhoot forth is generally rectangular, or very near it, to the beds or maſſes from which they proceed; and that all the ſeeming confuſion in pointings of the *Corniſh* diamonds in *Pillion Erth*, was owing to the great variety of planes and ſurfaces, into which that large body of ſpar was hardening, when theſe diamonds ſhot from it's extremities.

XXVII. 1. The curious foſſil I now exhibit to the *Society*, is as rare as it's figure is elegant; having been mentioned by none of our own writers who treat on foſſils, and but very imperfectly deſcribed by foreign Lithographiſts †. *A letter from the Rev.* Char. Lyttelton, *L. L. D. and F. R. S. Dean of* Exeter *to*

I diſcovered

* See *Fig.* 76.

† I ſuppoſe the *Dean* means Dr *Bruckmann*, and the late Mr *Linck*, an eminent Apothecary at *Leipſic*: for Dr *Bruckmann*, in his *Centuria Epiſt. Itinerar. Wolffenbuttl.* 1742. 4to, Epiſt. XXIII. has given ſeveral figures of petrifactions, very much reſembling theſe

Dudley.

the Pref *concerning a non-descript* petrified insect. N°. 496. p. 598. Nov. &c 1750. *Read* Dec. 20. 1750 Fig. 91, 92, 93, 94.

I discovered a single specimen of it (*Fig.* 91, 92, and 93.) last yea in the limestone pits at *Dudley* in *Worcestershire*; and very lately a larg mass of limestone (*Fig.* 94.) full of them in the same place; both which are now submitted to the inspection of this Learned Body, who are best able to determine to what class of the animal kingdom it properly belongs.

Extract of a letter from the same; to C. Mortimer, *Secret. R. S.* Ibid. p. 600.

2. The Rev. Dr *Shaw*, of *Oxford*, has procured a specimen of the extended *eruca*. As the Fossilists differed in their opinion of this *Dudley* fossil, some pronouncing it an *eruca*, others a *bivalve*, I thought it best to leave the reader to judge for himself from the engravings; but, as we are now able to add a specimen of this fossil in an extended posture, there is a better pretence to call it an *eruca*.

Some further account of the before mentioned Dudley Fossil; *by the* Editor *of these* Transactions. Ibid.

3. The Rev. Dr *Pocock*, F. R. S. was so obliging as to send several specimens of this fossil to the *Pres.* who put them into my hands, and desired me to draw up an account of them to be annexed to the preceding paper.

The first specimen is a mass of stone containing the face and eyes, with some rudiments of legs on the sides; but the back is intirely broken away. Another specimen contains the head only: a third, the head, and part of the back, but greatly distorted. But the most beautiful and complete are the two which I caused to be drawn and engraven in *Fig.* 98, 99, 100, 101.

Fig. 98, 99, 100, 101.

At *Fig.* 98. is one of these insects completely extended at it's whole length; wherein it appears, that the head is covered with a shell or crust consisting of 3 parts; the middle part is broad and round, *a.* which I shall therefore call the nose: the two side pieces are of a triangular form, *b. b.* in each of which is situated a large protuberant eye, *c. c.* The anterior part of the whole is encompassed by a round border, *d. d. d.* which looks like an upper lip; tho' I do not take it to be so; but that the mouth is situated lower down, as in the crab-kind, and does not appear in any of the specimens I have yet seen. On each side the crown of the head, towards the back part of it, are two small knobs, *e. e.* At *f. f.* in *Fig.* 99 appear some traces of feet, which seem to lie under the belly: but, as the belly, or under side, was not distinct, not being cleared from it's stony and earthy matter, I could not discern any other legs.

Dudley Fossils; the first was found at *Steme*, a village in the neighbourhood of *Paderborn*, given him by Dr *Kœnig*, which he took for a sort of *polypus marinus*; he says it is an animal unknown to him, but he gives those figures of it, in hopes that some curious persons, who live near the sea, may light upon some animal resembling this. The body of this stone, he says, has, on each side three striated lobes, and three pointed *appendices* beneath; it's inner substance is white, being *selenites*, or white spar; it's colour on the outside is every where brown. His friend *Linck* had sent him specimens of these stones 6 years before, some modelled in wax, others engraven upon copper. *C. M.*

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Pla. XVI. *Vol.* X. *Part* II. *Pag.* 656.

Fig. 91.

Fig. 92.

Fig. 93.

Fig. 95.

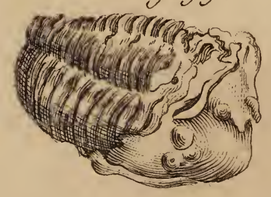

Fig. 96.

Fig. 97.

Fig. 98.

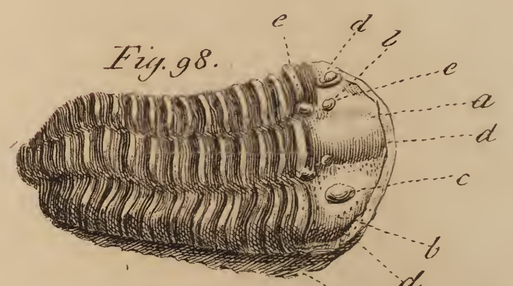

Fig. 99.

Fig. 100.

Fig. 101.

Fig. 94.

It is moſt likely the whole back of this creature was, when alive, covered with a caſe, or undivided *elytrum*, as is the *Scolopendra aquatica ſcutata*, deſcribed by M. *Klein*, of *Dantzick* * ; and afterwards by the Rev. Mr *Littleton Brown*. M. *Klein* ſays, the caſe was whole ; and that he was forced to ſlit it open to ſhew the back underneath ; when it appears, that the body was trilobated, as in *Fig.* 78. The caſe, being very thin and tender, may probably have been broken off at the death of the animal, before it's being petrified.

Mr *Brown* does not mention in his inſect the property of rolling itſelf up, which this certainly had ; as appears by ſeveral of the figures, as *Fig.* 91, 92, 93, 96, 100, and 101, which are intirely rolled up ; and as is more particularly repreſented by *Fig.* 100 and 101, in which it appears, that the tail is turned up under the belly quite to the mouth ; and at *Fig.* 97. the creature ſeems but half rolled up.

I have conſulted all the books I could meet with, which give figures of inſects and cruſtaceous animals in their natural and petrified ſtates ; and find none reſemble this *Dudley* foſſil ſo near as M. *Klein*'s inſect ; therefore I ſhall, till we get more information, call it, *Scolopendræ aquaticæ ſcutatæ affine animal petrifactum.*

XXVIII. This work conſiſts of 295 pages in 8vo, excluſive of the preface, and of 6 copper-plates, exhibiting different views of ſalt-houſes, inſtruments, *&c.* neceſſary to the preparation of ſalt. It is enriched likewiſe with notes of great importance to the work, not only of the author, but alſo from the *Philoſ. Tranſ. Medical Eſſays*, *Mem. of the R. Acad. of Sc. at Paris*, *Pliny*, *Agricola*, *Alonſo Barba*, *Ramuſio*, *Boyle*, *Hoffman*, *Liſter*, *Herrera*, *Dampier*, *Baccius*, *Pomet*, *Marſilli*, *Plott*, *Scheuchzer*, *Hales*, *Raſtel*, *Leigh*, *Boerhaave*, *Shaw*, and others.

Amongſt the vulgar arts, that of preparing ſea-ſalt for the uſes of mankind hath been thought worthy the notice of many great and learned men, as well antient as modern. Thus many things relating to this art are recorded by *Cato* and *Pliny*, *Agricola* and *Hoffman*, to whom our author is much indebted for thoſe memoirs that have been tranſmitted to us, relating to it's hiſtory. Had thoſe great men been as diligent in improving this art, as they were in recording the improvements made therein by others, there would not now have been occaſion to remark, that, after the practice of ſo many ages, an art ſo ſimple, and withal ſo neceſſary, hath not yet been brought to any degree of perfection.

That this art was capable of great improvements, eſpecially as practiſed in *Great Britain*, was the ſentiment of this *Society* ſoon after it's inſtitution ; at which time the members thereof were very intent upon bringing it to a greater perfection ; as may be gathered from the inquiries and ſuggeſtions of Dr *Beal*, and the hiſtories of ſeveral methods of making ſalt, which then were publiſhed by the *Society*. And although the *Engliſh* have, ſince that time, conſiderably improved their method

An account of a treatiſe by Wm. Brownrigg, *M. D. F. R. S intituled*, "The "Art of making common Salt, "as now "practiſed in "moſt parts "of the "world ; "with ſeveral Improvements "propoſed in "that Art, "for the Uſe "of the *Britiſh* Dominions ;" *abſtracted by* W. Watſon, *F. R. S.* N°. 487. p. 351. April &c. 1748. *Read* June 15. 1748.

* See Vol. IX. Part iii. § iv.

of boiling ſalt; yet this art is ſtill practiſed with greater ſkill and ſucceſs by the *Dutch*, as the ſuperior goodneſs of the fiſh, cured with their ſalt, doth ſufficiently prove.

The Commons of *Great Britain*, having taken into conſideration the great importance of this art, judged ſome improvements propoſed therein worthy their regard and encouragement; well knowing, that, could this be brought to the ſame perfection in *Britain* as in ſome neighbouring countries, large ſums of money might be ſaved in the nation, which are now paid to the *French* and others; it's fiſheries improved, and it's navies and commerce, and many of it's richeſt colonies, would no longer depend upon it's enemies for one of thoſe neceſſaries, without which they cannot be ſupported.

Theſe conſiderations have induced our author to give a brief account of the various methods of making ſalt, which are now uſed in *Great Britain*, and in other countries, where this art is practiſed with more ſucceſs; and alſo to attempt ſeveral further improvements for the uſe of the *Britiſh* dominions. How far he has ſucceeded in theſe attempts, will beſt appear, if the public ſhall think the following propoſals ſo far worthy their attention, as to merit a fair and impartial trial. The principal concluſions, deduced from a variety of obſervations and experiments, are as follows: 1. That, by the methods here propoſed, an excellent bay-ſalt may be made in *Britain* in very large quantities, ſo as to be afforded cheaper than at the prices paid for foreign ſalt; and that the *Britiſh* colonies in *America* may very commodiouſly be ſupplied with bay-ſalt of their own manufacture, without having recourſe for it to the *French*, *Spaniards*, and *Portugueſe*. 2. That, by the methods here propoſed, an excellent kind of refined white ſalt may be made in *Britain*, as well from ſea-water and rock-ſalt, as from natural brine, in any quantity wanted, ſo as to be afforded cheaper than foreign bay-ſalt; and which will alſo be better for curing fiſh, fleſh, and other proviſions.

In forming theſe concluſions, an impartial regard has been had to truth, without attending to the private advantage of any particular ſet of men. The ſenſe of this, together with a deſire of promoting the publick advantage, has induced our author to communicate the following ſheets at this time, although by deferring the publication ſome time longer he might have made them poſſibly more accurate; becauſe, beſides other conſiderations of no ſmall import, an opinion has prevailed, that the eſtabliſhing of fiſheries in the north of *Scotland* would be the beſt means of affording an uſeful employment to more uncivilized inhabitants of that part of the kingdom, for carrying on of which they are moſt commodiouſly ſituated.

What Mr *Lowndes* * hath lately done towards the improvement of brine-ſalt, may, perhaps by ſome, be thought to ſuperſede the neceſſity

* Mr *Lowndes*'s proceſs is inſerted in this Work. See *p.* 104, *&c. ſeq.*

of

of further attempts for improving and extending our ſalt manufacture. Dr *Brownrigg* is very far from depreciating the endeavours of that gentleman, which have met with parliamentary encouragement; and had his diſcovery appeared to the doctor ſufficiently complete and extenſive, he would not have given the public and himſelf this trouble. He makes no doubt but that the ſpecimen of ſalt, which Mr *Lowndes* exhibited before the College of Phyſicians, was a ſtrong and pure ſalt, ſince ſuch it appeared to that moſt learned Body. Whether the alum mixed with it (agreeable to the antient practice of the *Cheſhire* ſalt-boilers) contributed any thing to it's goodneſs, is more properly conſidered hereafter. It is only neceſſary here to obſerve, in juſtification of the preſent undertaking, that Mr *Lowndes*'s method of making ſalt for curing proviſions, doth not appear to be the beſt that may be put in practice; ſince our author hopes to ſhew, that, by other methods, a purer and a ſtronger ſalt may be made, and at a leſs expence. Neither is his method ſo general and extenſive as ſeems to be required for the public good; ſince Mr *Lowndes* confines it almoſt intirely to boiled brine-ſalt; and hath given no directions concerning the preparation of bay-ſalt. He indeed propoſes to meliorate the *Britiſh* ſea-ſalt, but ſeems to deſpair of preparing a ſalt either from ſea-water, or *Engliſh* rock-ſalt, fit for the uſes of the navy or fiſheries; although the *Dutch* ſalt, which is the ſtrongeſt and pureſt boiled ſalt now made, is entirely a marine ſalt, and even the brine, of which Mr *Lowndes* makes his ſalt, is only a ſolution of the *Engliſh* rock-ſalt, often in very impure water, as is well known to the Naturaliſts.

Our author, treating of ſalt in general, takes notice of the excellence and uſefulneſs thereof; and that it hath pleaſed the *Author* of Nature to provide mankind therewith in ſuch abundance, that there are few countries which do not afford vaſt quantities of rock or foſſil ſalt. Mines of it have been long diſcovered and wrought in *England*, *Spain*, *Italy*, *Germany*, *Hungary*, *Poland*, and other countries in *Europe*. Moreover the ſea affords ſuch vaſt plenty thereof, that all mankind might thence be ſupplied with quantities ſufficient for their occaſions. There are alſo innumerable ſprings, ponds, lakes, and rivers impregnated with common ſalt, from which the inhabitants of many countries are plentifully ſupplied herewith.

In ſome countries, which are remote from the ſea, and have little commerce, and which are not bleſſed with mines of ſalt, or ſalt waters, the neceſſities of the inhabitants have forced them to invent a method of extracting their common ſalt from the aſhes of vegetables.

In ſhort, this ſalt is diſperſed all over nature; it is treaſured up in the bowels of the earth; it impregnates the ocean; it deſcends in * rains; it fertilizes the ſoil; it ariſes in vegetables; and from them is conveyed into animals; ſo that it may well be eſteemed the univerſal condiment of nature.

* See *Boyle* on the ſaltneſs of the ſea.

Naturaliſts, obſerving the great variety of forms under which this ſalt appears, have thought fit to rank the ſeveral kinds of it under certain general claſſes, diſtinguiſhing it moſt uſually into rock or foſſil ſalt, ſea-ſalt, and brine or fountain-ſalt: to which may be added others of thoſe muriatic ſalts, which are found in vegetable or animal ſubſtances. Theſe ſeveral kinds of common ſalt often differ from each other in their outward form and appearance, or in ſuch accidental properties as they derive from the heterogeneous ſubſtances with which they are mixed; but, when perfectly pure, they have all the ſame qualities; ſo that Chemiſts, by the exacteſt inquiries, have not been able to diſcover any eſſential difference between them. In this our author agrees with the celebrated * *Hoffman*. Leaving therefore theſe diviſions to thoſe whom they may concern, it may for the preſent purpoſe be more proper to diſtinguiſh common ſalt after a different manner into the three following kinds; *viz.* into rock or native ſalt, bay-ſalt, and white ſalt.

By rock-ſalt †, or native ſalt, is underſtood all ſalt dug out of the earth, which hath not undergone any artificial preparation.

Under the title of bay-ſalt may be ranked all kinds of common ſalt extracted from the water, wherein it is diſſolved by means of the ſun's heat, and the operation of the air; whether the water, from which it is extracted, be ſea-water, or natural brine drawn from wells and ſprings, or ſalt water ſtagnating in ponds and lakes.

Under the title of white ſalt, or boiled ſalt, may be included all kinds of common ſalt extracted by coction from the water wherein it was diſſolved; whether this water be ſea-water, or the ſalt water of wells, fountains, lakes, or rivers; or water of any ſort impregnated with rock-ſalt, or other kinds of common ſalt.

The firſt of theſe kinds of ſalt is in ſeveral countries found ſo pure, that it ſerves for moſt domeſtic uſes, without any previous preparation, triture excepted. But the *Engliſh* foſſil ſalt is unfit for the uſes of the kitchen, until by ſolution and coction it is freed from ſeveral impurities, and reduced to white ſalt. The *Britiſh* white ſalt alſo is not ſo proper as ſeveral kinds of bay-ſalt for curing fiſh, and ſuch fleſh-meats as are intended for ſea proviſions, or for exportation into hot countries. So that, for theſe purpoſes, we are obliged, either wholly or in part, to

* *Hoffman* de ſalinibus *Hallenſ.* cap. viii.

Ut igitur noſtra hac de re innoteſcat ſententia, hanc interponimus; ſicuti in tota univerſi hujus orbis compage, una tantum eſt aqua, unus per fermentationem paratus ſpiritus ardens, unus Mercurius, unum volatile ſal, unum acidum nitroſum ac vitriolicum ſal; ita, pari ratione unum idemque ſal commune eſt. Sed quum plures alienæ, terreæ, lapidoſæ, ſulphureæ, calcariæ minerales ac pingues particulæ cum hiſce corporibus connubium ineant, diverſa exinde emergit eorum indoles; & ſal commune idem ſemper obtineret ingenium, ſiquis pingues terreas, calcareaſque partes ab illo artificioſe ſegregaret.

† By *Rock-Salt*, or *Sal Rupium*, the antient *Chemiſts* mean ſalt adhering to the rocks above the high water mark, being there lodged by the ſpray of the ſea, evaporated by the heat of the ſun; which is the pureſt ſalt of all for chemical uſes, and is to be had off the rocks of *Sicily*, and ſeveral iſlands in the *Weſt Indies*. *C. Mortimer.*

uſe

uſe bay-ſalt, which we purchaſe in *France*, *Spain*, and other foreign countries. To remedy theſe inconveniencies this treatiſe was written, in order to ſhew how the ſubjects of *Great Britain* may be ſupplied with ſalt of their own manufacture, fit and ſufficient for all their occaſions.

In order that the methods here propoſed might be better underſtood, and that the reaſonableneſs of them might more fully appear, the author thought it neceſſary to premiſe a brief account of the ſeveral ways of preparing bay-ſalt, as well as white ſalt, as far as they came to his knowledge. From this hiſtory may be formed a judgment, how far the methods now in uſe are proper, in what deficient, where erroneous, and how they may be improved.

Bay-ſalt in general may be divided into two kinds. Firſt, bay-ſalt, drawn from ſea-water, as is practiſed in *France*, *Spain*, *Portugal*, and many other countries. Secondly, bay-ſalt extracted from ſalt ſprings, ponds, and lakes; as at *Cape de Verd* iſlands, *Tortuga*, and other places. Of theſe the firſt is imported in large quantities into *Great Britain* and *Ireland*: our *American* colonies, in times of peace, are chiefly ſupplied with the latter; but in time of war they have large quantities of bay-ſalt from *Liſbon*, and other parts of *Portugal*.

Bay-ſalt is prepared in a manner the moſt ſimple and eaſy, when the water of ponds and lakes impregnated with ſalt is totally exhaled by the force of the ſun and air, and the ſalt is left concreted into a hard cruſt at the bottom of the lake or pond. Of ſalt thus prepared we have inſtances in many parts of the world, as in the *Podolian* deſert near the river *Boryſthenes*, on the *Ruſſian* frontiers towards *Crim Tartary*, in the kingdom of *Algiers*, and in other parts of the world.

Bay-ſalt is alſo drawn from the brine of ponds and lakes, and our author gives us an account of the preparing it it in this manner in the *Cape de Verd* Iſlands. This account was collected chiefly from the relations of ſeveral perſons of credit, who themſelves aſſiſted in making ſalt in theſe iſlands. He alſo takes notice of the bay-ſalt made at *Tortugas*, and other places in *America*. He deſcribes likewiſe the manner of making marine bay-ſalt in *France*, and other parts of *Europe*. For the particulars of theſe operations I muſt refer you to the work itſelf; and only take notice, that every kind of bay-ſalt is prepared without artificial heat, and by only expoſing the brine under a large ſurface to the action of the ſun and air, by which, in proportion to the ſtrength of the brine, and to the different temperature of climate and ſeaſon, the ſalt cryſtallizes into what we call bay-ſalt, and comes under different appearances to us from different places, which ariſe principally from the cleanlineſs and care of the artiſt.

Our author, when treating of white ſalt in general, acquaints us, that although ſalt is made, in warm climates, with the greateſt eaſe, and at the leaſt expence, by the heat of the ſun, after the methods already deſcribed; yet, in ſeveral countries, where bay-ſalt might be conveniently made, they prepare all their ſalt by culinary fires. Thus in *Auſtria*, *Bavaria*,

Bavaria, and many other parts of *Germany*, and alſo in *Hungary*, and even in ſome parts of *Italy*, they conſtantly boil the water of their ſalt ſprings into white ſalt. But in other parts of *Europe*, as in *Britain*, and in the northern parts of *France* and *Germany*, an erroneous opinion long prevailed, that the heat of the ſun was not there ſufficiently intenſe, even in the ſummer ſeaſon, to reduce ſea-water, or brine, into bay-ſalt. And all arguments would probably have been inſufficient to remove this prejudice for the *Engliſh*, had not the contrary been fully proved by experiments, which were firſt accidentally made in *Hampſhire*. However, the method of making ſalt by coction will probably ſtill continue to be practiſed in *Britain*; as the ſalt ſo prepared is for ſeveral uſes preferable to bay-ſalt; and when prepared after a particular manner, is preferable to common bay-ſalt, even for curing proviſions, as the practice of the *Hollanders* ſufficiently teſtifies: ſo that the due and right preparation of white ſalt ſeems very deſerving of the notice and regard of the public.

White ſalt, as it is prepared from various ſaline liquors, may therefore be diſtinguiſhed into the following kinds:

1. Marine boiled ſalt, which is extracted from ſea-water by coction. 2. Brine or fountain-ſalt, prepared by coction from natural brine, whether of ponds or fountains. 3. That prepared from ſea-water, or any other kind of ſalt-water, firſt heightened into a ſtrong brine by the heat of the ſun, and the operation of the air. 4. That prepared from a ſtrong brine or *lixivium* drawn from earths, ſands, or ſtones impregnated with common ſalt. 5. Refined rock-ſalt, which is boiled from a ſolution of foſſil ſalt in ſea-water, or any other kind of ſalt-water, or pure water. 6. Laſtly, ſalt upon ſalt, which is bay-ſalt diſſolved in ſea-water, or any other ſalt water, and with it boiled into white ſalt; and under theſe heads may be ranked the ſeveral kinds of boiled ſalt now in uſe. Our author has given us an exact hiſtory of the manner of preparing theſe different kinds of ſalt, as practiſed in different places, with miſcellaneous obſervations and cautions relating to their reſpective proceſſes, for which in the general I muſt refer you to the work itſelf: but the making ſalt upon ſalt deſerves more particular attention; as the author, being under no tie of ſecrecy, has revealed to us the method of making in *Holland* and *Zealand* that ſtrong and pure kind of ſalt, with which they cure herrings, and all other proviſions for long keeping; which gives the *Dutch* a great advantage over all other nations in the herring-fiſhery; ſince fiſh preſerved with this ſalt look much cleaner and fairer than thoſe that are cured with bay-ſalt, and keep much better than thoſe preſerved with any other kind of white ſalt.

From the proceſs whereby white ſalt is made from ſea-water by coction, it appears, that ſea-water, beſides common ſalt, contains ſeveral other ingredients; ſome of which are ſeparated before the common ſalt falls, and others remain in the bittern, after all the ſalt is extracted. Our author has given a full and circumſtantial account of theſe in an expreſs

expreſs chapter, under the appellation of memoirs for an analyſis of ſea-water.

The ſalt-boilers, and particularly thoſe who prepare brine-ſalt, have long been accuſtomed to make uſe of various ſubſtances, which they call additions or ſeaſonings, and mix them with the brine while it is boiling, either when they firſt obſerve the ſalt begin to form, or elſe afterwards during the time of granulation. Theſe additions they uſe for various purpoſes. Firſt, to make the ſalt grain better, or more quickly form into cryſtals. Secondly, to make it of a ſmall fine grain. Thirdly, to make it of a large firm and hard grain, and leſs apt to imbibe the moiſture of the air. Fourthly, to render it more pure. And laſtly, to make it ſtronger, and fitter for preſerving proviſions.

Theſe additions, moſt commonly uſed to anſwer the above-mentioned purpoſes, are wheat-flour, reſin, butter, tallow, new ale, ſtale beer, bottoms or lees of ale and beer, wine-lees and alum. Wheat-flour and reſin are uſed for the property they poſſeſs of making the ſalt a ſmall grain. Butter, tallow, and other unctuous bodies are commonly applied, as they are ſaid to make the brine cryſtallize more readily; for which end ſome ſalt boilers more particularly prefer the fat of dogs: but others have little to plead for their uſing theſe ſubſtances, but immemorial cuſtom: how far they have the effects aſcribed to them can only be determined by experiments, as ſeveral boilers, who formerly uſed them, now find they can make as good ſalt without them. Wine-lees, new ale, ſtale ale, the lees of ale and beer are now generally rejected by the marine ſalt boilers; except in the Weſt of *England*, where the briners, who uſe them, affirm that they raiſe a large grain, and make their ſalt more hard and firm, and ſome ſay that they make it cryſtallize more readily. *Hoffman* prefers the ſtrongeſt ale; and *Plott* aſſures us, that it makes the ſalt of a larger or ſmaller grain, according to the degree of it's ſtaleneſs. The only good effects that fermented liquors can have as an addition, are probably owing to their acid ſpirit, which may correct the alcaline ſalts of the brine, and ſo render the common ſalt more dry and hard, and leſs apt to diſſolve in moiſt air. If therefore it ſhould be thought neceſſary to uſe any of theſe additions, in order to correct the alcaline quality of the brine, ſtale ale, or *Rheniſh* wine *, ought to be choſen, as new ale contains but little acid.

Alum is an addition long known and uſed in *Cheſhire*, together with butter, to make the ſalt precipitate from ſome ſorts of brine, as we are aſſured by Dr *Leigh* in his Natural Hiſtory of *Lancaſhire*, *Cheſhire*, &c. who firſt taught the *Cheſhire* ſalt-boilers the art of refining rock-ſalt. As the bad properties of their ſalt proceeded from hard boiling, they found every method ineffectual, until they had recourſe to a more mild and gentle heat. And as alum hath been long diſuſed amongſt them, it is not likely, that they found any extraordinary benefit from it; other-

* Why not Malt-Vinegar? *C. M.*

wiſe

wiſe they would ſcarce have neglected it, and continued the uſe of butter. However Mr *Lowndes* hath lately endeavoured to revive it's uſe; aſſerting, that brine-ſalt hath evermore two main defects, flakyneſs and ſoftneſs; and to remedy theſe imperfections, he tried alum, which fully anſwered every thing he propoſed; for it reſtored the ſalt to it's natural cubical ſhoot, and gave it a proper hardneſs; nor had it any bad effect whatever. But our author is of opinion, that whoever conſiders the nature of alum, will ſcarce expect ſuch extraordinary effects from it. Neither does it here ſeem wanted; for the grains of common ſalt will always be ſufficiently hard, and of their natural figure, large ſize, and no ways diſpoſed to run by the moiſture of the air, if formed by a gentle heat, and perfectly free from heterogeneous mixtures: ſo that the goodneſs of Mr *Lowndes*'s ſalt does not ſeem owing to the alum, with which it is mixed, but chiefly to the gentle heat uſed in it's preparation.

The *Dutch*, who have long ſhewn the greateſt ſkill and dexterity in the art of boiling ſalt, make uſe of another addition, which they eſteem the greateſt ſecret of their art. This is whey, kept ſeveral years 'till it is extreamly acid; now firſt revealed by our author to the *Britiſh* ſalt-boilers, but long held in great eſteem by the *Dutch*, for the good effects it hath upon their ſalt; which it renders ſtronger, more durable, and fitter to preſerve herrings, and other proviſions.

Bay-ſalt, as well as white ſalt, is of different kinds, and poſſeſſed of different qualities: with the different kinds of theſe proviſions muſt be cured, according to the uſes for which they are deſigned. The *Dutch* indeed uſe no ſalt for curing proviſions, beſides their own refined ſalt. With it they can preſerve fleſh and fiſh of all kinds as well as with the ſtrongeſt bay-ſalt; and chuſe to be at the expence of refining bay-ſalt, rather than to defile their proviſions with the dirt and other impurities, with which it commonly abounds.

Salt, eſteemed the beſt for curing proviſions, and for preſerving them the longeſt time, is that which is the ſtrongeſt and the pureſt. This may be known by the following characteriſtics; *viz.* it is uſually concreted into large grains or cryſtals, which are firm and hard, and in reſpect to thoſe of other kinds of common ſalt, the moſt ſolid and ponderous; it is not diſpoſed to grow moiſt in a moderately dry air, to which it has been expoſed a conſiderable time; it's colour is white, and ſomewhat diaphanous; it hath no ſmell; it's taſte is truly muriatic, and more ſharp and pungent than that of other kinds of common ſalt. It has, beſides theſe, ſeveral other diſtinguiſhing properties mentioned by our author. The ſalts, which approach neareſt to this degree of perfection, are the beſt kinds of bay-ſalt, and the ſtrong *Dutch* refined ſalt; but moſt of the ſalt now made for ſale is very far from anſwering to theſe characteriſtics.

Having related the various methods of preparing ſalts that now are in uſe, as far as they are come to our author's knowledge, it appears, that

that this art is not brought to ſuch perfection in the *Britiſh* dominions as in ſeveral other countries, the ſalt there prepared being unfit for preſerving many kinds of proviſions. It remains now to ſhew, that this want of a ſtrong ſalt of *Britiſh* manufacture proceeds not from any defect in nature, but of art; and that, if proper ſkill and induſtry be uſed in the *Britiſh* dominions, and due encouragement there given by the legiſlature, ſuch improvements may be made in this art, that not only *Great Britain*, but *Ireland* alſo, and the *Britiſh* colonies in *America*, may be ſupplied with ſalt of their own manufacture, proper for curing all kinds of proviſions, in quantity ſufficient for all their occaſions, in quality equal, if not ſuperior, to any foreign ſalt now made, and at a moderate price. Theſe are truths, which the author hopes will appear evident from the facts and reaſonings contained under the following poſitions:

The quantity of water which annually falls in rain, ſnow, and hail, is very different in different parts of *Great Britain*; there commonly falling almoſt double the quantity on the weſtern coaſts, that falls on the eaſtern coaſts of that iſland. *Lemma* I.

The quantity of rain which falls in *Lancaſhire*, during the four hotteſt months of the year, *viz. May*, *June*, *July*, and *Auguſt*, doth not at a medium amount to more than $\frac{1}{3}$ of the quantity of water, which falls in rain, ſnows, and hail, during the whole year. *Lemma* II.

The water which aſcends in vapours from the ſea very greatly exceeds that which deſcends thereon in rain and other aqueous meteors: but the quantity of water, which uſually exhales from a given part of the ocean in a given time, cannot with any exactneſs be determined. *Lemma* III.

The quantity of water which commonly exhales in *Great Britain* from ſhallow ponds during the 4 hotteſt months of the year, greatly exceeds the quantity of rain which commonly falls on the ſurface of thoſe ponds during the ſaid months. *Lemma* IV.

From theſe *lemmata*, which the author has ſupported by the obſervations, not only of himſelf, but of other learned men, are deduced the following propoſitions:

In ſeveral parts of *England* large quantities of bay-ſalt may be extracted from ſea-water during the hotteſt months of the year, by receiving the ſalt-water into ponds, and ſuffering it's aqueous parts thence to exhale by the heat of the ſun, and the operation of the air and winds. *Prop.* I.

In ſeveral parts of *England* large quantities of bay-ſalt may very commodiouſly be extracted from ſea-water, after the ſame manner that is practiſed in *France*, and in other parts of *Europe*. *Prop.* II.

Bay-ſalt may be extracted in *England* from ſea-water in larger quantities, and with more certainty, than by the foregoing method, if care be taken to preſerve the brine contained in the ſalt-pits from being dilu- *Prop.* III.

ted with rains, and to promote the evaporation of the water by ſeveral artificial means, which may eaſily be put in practice.

Prop. IV. In ſeveral parts of *England* large quantities of excellent bay-ſalt may with great eaſe be made from the natural brine of ſalt ſprings, and alſo from rock-ſalt diſſolved in weak brine or ſea-water.

Prop. V. Bay-ſalt may be prepared in *England* by the foregoing methods at a very moderate expence, equal in goodneſs to the beſt foreign bay-ſalt, and in quantity ſufficient for the conſumption of all the *Britiſh* dominions.

Prop. VI. In ſeveral of the *Britiſh* colonies in *America*, bay-ſalt might, with little expence and trouble, be prepared from ſea-water, in quantities ſufficient to ſupply the *American* fiſheries, and all other occaſions of thoſe colonies, ſo as to become a conſiderable branch of their trade.

The author has ſupported all theſe propoſitions with great ingenuity; but I cannot paſs over in ſilence the artificial means to promote the evaporation of ſea-water, mentioned in *Prop.* III. as well as to preſerve the brine contained in the ſalt-pits from being diluted with rains. I therefore ſhall lay before you a ſhort account of theſe.

It will be proper, ſays he, to make all the ſalt-pits of the marſh in one long row extended from E. to W. and for each pit to make covers of thin boards, or rather of coarſe canvas, or ſail-cloth, ſtretched on frames of wood and painted white. Theſe covers muſt all be fixed with hinges to ſtrong poſts and beams on the N. ſide of the pits; ſo that they may be let down and drawn up with cords and pulleys, or by ſome other contrivance, ſomewhat like draw-bridges. Theſe covers thus fixed may be let down over the pits like a ſhed or penthouſe in rainy weather; and in dry weather may be erected almoſt to a perpendicular, but inclining a little towards the S. ſo as to form a wall with a S. aſpect. Thus theſe may ſerve a double purpoſe, as coverings for the pits in wet weather, and as reflectors of the ſun's heat upon them in dry weather, and thus greatly promote the evaporation of the aqueous parts of the brine. The hinges on which the reflectors turn may be fixed about 8 or 10 inches from the ground; by which means, when the reflectors ſtand upright, there will be an opening left beneath them, through which the air will continually flow in a briſk current, and greatly increaſe the evaporation of the water.

After having gone through that part of Dr *Brownrigg*'s work, which relates to bay-ſalt, we proceed to the methods that gentleman propoſes for preparing and improving white ſalt, which, if brought into uſe, may probably be of advantage not only to private undertakers, but alſo to the public. For it appears, that two very different kinds of white ſalt are required; the one for the uſe of the table, and the other as a condiment for proviſions. It's whiteneſs, dryneſs, and the ſmallneſs of it's grain, are the properties which chiefly recommend the firſt kind; and it's great ſtrength and purity the latter. It is this ſtrong and pure

kind

kind of white ſalt, which is wanted in the *Britiſh* dominions; and it is therefore our author's principal deſign here to conſider how this defect may be ſupplied; although at the ſame time inſtructions are given how to prepare table ſalt, not only better in quality, but alſo at a leſs expence than it is now prepared by the common methods.

In the common proceſſes for making white ſalt, the ſalt is deprived of a conſiderable part of it's acid ſpirit, by the violent boiling uſed in it's preparation. *Lemma* I.

Moſt kinds of white ſalt are rendered impure by the mixture of various heterogeneous ſubſtances. *Lemma* II.

White ſalt, by the violent coction commonly uſed in it's preparation, is rendered leſs fit for preſerving fiſh, fleſh, and other proviſions, than it would be if prepared with a more gentle heat. *Lemma* III.

The heterogeneous ſubſtances which are commonly mixed with white ſalt, render it leſs proper for preſerving proviſions, than it would be if ſeparated from them. *Lemma* IV.

After having fully conſidered the foregoing, our author gives a method of preparing a kind of white ſalt proper for curing fiſh, fleſh, and other proviſions; likewiſe a method of refining ſalt; but for theſe I muſt refer you to the work itſelf, as well as for the tables, wherein the ſeveral expences attending theſe operations are minutely conſidered.

Moſt of the facts referred to in theſe diſquiſitions are ſuch, as the conſtant practice of thoſe who make ſalt ſufficiently warrants us to rely upon for true and certain; or elſe, they are the obſervations of judicious ſalt-officers, daily converſant in theſe matters, or of curious and inquiſitive navigators, merchants, travellers, and Naturaliſts; or, laſtly, the experiments of many learned Phyſicians, Chemiſts, and Philoſophers: the truth of which ſeveral facts, though many of them have long been publiſhed, hath never been called in queſtion. So that theſe obſervations and experiments may probably be more relied on by the public, than if they had only been made by our author; ſince they have the teſtimony of many ſkilful and unprejudiced perſons, who could have no notion of the uſes to which they have been here applied. If therefore the arguments founded upon thoſe facts ſhould be eſteemed any-ways reaſonable and ſatisfactory, the author preſumes to remark, that it might not be unworthy the wiſdom of the *Britiſh* Legiſlature to direct a more full inquiry to be made into a matter of this importance, and to order proper works to be erected for making bay-ſalt, and for making and refining white ſalt, and to put thoſe works under the management of able and judicious perſons, to make exact and accurate trials, in order to diſcover the beſt and cheapeſt methods of doing them. And the methods, which ſhould be moſt approved of, might for the general good be made public, and eſtabliſhed by law as a common ſtandard, to which all thoſe who make ſalt in the *Britiſh* dominions ſhould be obliged to conform.

Of the Salt-mines near Cracau ; *by* James Mounſey, *M. D. Phyſ. to the* Czarina's *army*, N°. 493. p. 219. Oct. &c. 1749. *Read* Nov. 23. 1749.

XXIX. Near *Cracau* in *Poland* are famous ſalt-mines, of which I ſhall give you a ſhort account, as well as of the moſt remarkable things I found there. The town is ſituated near the foot of a vaſt chain of mountains, and from it, paſſing by hills and riſing grounds about two *German* miles ſouthward, I came to the mines of *Vilitzca*. Theſe are in a hill flat and irregular above, ſurrounded with hollows and vallies, and to the ſouth there is a neighbouring hill much higher. The mine has ten entries, which are provided with horſe-engines, whereof 7 are for hoiſting up the ſalt, and the reſt for drawing water from the works, and for the deſcent and aſcent of the people. I entered the mine by winding ſtairs of 484 ſteps, which brought me to the firſt ſtory. The deſcent into the ſecond is by ſtrait ſtairs of 133 ſteps. Into the lowermoſt ſtory there are no ſtairs, but 18 ladders from different floors, which make together 300 feet ; and the computed depth of the whole is about 900 feet perpendicular.

The ſeveral ſtrata of the earth are as follow.—On the ſurface is a common clayey ground, next is pure clay, and then a bed of ſoft, moiſt, black, ſlimy earth ; and below this are hills of a kind of earth without any mixture of grit or ſand. Here are firſt found particles and veins of ſalt ; and, deſcending a good way through this and ſome ſalt rocks, we enter into the firſt ſtory, where there are a great many alleys and croſs-ways (which are run out to conſiderable diſtances), and many large caverns, out of which ſalt has been cut. Here the floor, walls, and cielings, are ſolid ſalt rock. As the religion of the country is *Roman* Catholic, there are ſeveral large chapels, with altars adorned with columns, crucifixes, ſtatues of ſaints, and other ornaments in that way, hewed out of the ſalt rock, and well wrought in different orders of architecture. Some of theſe, which are of the purer ſalt, and not much ſmoked with the torches that the workmen uſe in the mine, have a very beautiful effect. In ſome places the ſides of the alleys, and ſome of the great vaults, are lined and doubled with timbers, where they thought the pillars of earth or ſalt left for ſupporting the ſuperior weight might prove too weak. I obſerved in one place, that a ſinking of the earth ſome years ago had cruſhed ſome of the baulks almoſt flat, and made a rent in the ſalt-rock on the other ſide, about 9 inches wide.

Notwithſtanding there is no remembrance or tradition of any remarkable accident by the falling-in of theſe mines, yet they have lately diſcovered a wooden houſe, which muſt have been ſwallowed in very long ago. Theſe mines were on fire in the year 1644. but this accident muſt have happened long before that time ; for they have a plan of theſe works, taken about 200 years ago, with remarks of every thing that was curious in all three contignations ; but no mention is made of this houſe, nor is there any-thing in the regiſters of theſe works that ſhews it to have ſunk in ſince. The wall of this houſe is ſeen at the ſide of one of the croſs-ways : they have found plates, ſpoons, and ſome other

things

things of metal; but they make no farther ſearch, as the pains would exceed the profit: ſo it is left as a curioſity.

Notwithſtanding the ſalt rocks are on all ſides, and the earth that is among them is full of veins and particles of ſalt, there is a ſpring of very good freſh water, which is the drink of the thirſty workmen, and of the horſes employed below-ground. This ſource comes from above; but directly over that place, on the ſurface, there is no well, nor ſpringy ground, only it is hollow.

They find in theſe mines alabaſter, *Glacies Mariæ*, *gypſum*, and ſometimes *pectines*, or ſmall ſea-ſhells: but the moſt remarkable thing of all is, in the middle of a vaſt ſalt rock, a large tree is found, with all it's branches incaſed in it, lying horizontally. I ſend you a piece of it, which I hewed out of the rock myſelf. It ſeems to be a beech-tree, of which there grow plenty in theſe countries at preſent.

From the upper ſtory the rocks grow broader like cones, and the deeper they go, the ſalt is always finer, and leſs mixed with earth: but it is not yet known how deep they run. They do not however find it turns ſo much to account to work the lowermoſt ſtory, though it is all pure rock, the hoiſting being more expenſive than the running out croſs-ways, and working the upper ſtories. The rocks have roots or veins, which ſhoot into the earth on all ſides, ſome in ſtrait lines, others in zigzag, even to the diſtance of 70 feet; whereby the miners are often directed to the body of the rock. Theſe veins are very white and clear, yet they make no uſe of what is found in them, being impure, and mixed with other ſalts: it likewiſe diſſolves much eaſier than the true ſalt. *Sal gemmæ* is found in veins and neſts in ſeveral places of the mines, but 'tis often very troubleſome to hew, and get it out of the other ſalt rock. Here are no wells of *naphtha*, but there are ſome cavities where the air is ſo inflammable, that ſome, by going raſhly into ſuch places with a light, have been damaged by the fire, and even run the riſque of their lives. This only happens in places where the air has no free admiſſion; for in all the main ſtreets and croſs-ways there is a conſiderable draught of air through the ten entries: and, in the winter, while it is a ſtrong froſt, and quite calm weather, there reigns a very ſtrong wind in the mines: but ſtormy weather makes no alteration; and in the ſummer there never happen any ſuch changes.

From the ſeveral ways are entries into the chambers or vaults where they work. They hew the ſides of the wall into large ſquare columns, the height of the room, and about 2 or 3 feet thick. By driving their wedges in behind theſe pillars, they make them rend from the rock, and their fall on the floor makes a very great noiſe. The workmen are ſo accuſtomed, that by the ſound of their blows they know the inſtant it is going to fall, and get out of the way accordingly. Sometimes they hew the like pieces from the floors. Theſe pillars are again hewed into blocks, from 3 to 6 feet long, according to their thickneſs. They are drawn up, and tranſported in ſuch pieces, and the ſmall in barrels.

The

The quantity of ſalt dug here yearly, comes to about 120,000 centeners of *Vienna:* and the whole expences for officers, workmen, materials, *&c.* amount to about 100,000 dollars. The number of workmen of all ſorts make about 600 perſons: they are very healthy and long-lived, not ſubject to the ſcurvy, or any particular diſtempers. The officers on the contrary are very ſubject to diſeaſes of the breaſt, and conſumptions, which is probably owing to the frequent changes of air they meet with, their buſineſs obliging them to ſtir about much, both above and below ground, where the air is very different.

Of the Foſſils of Bohemia, *by the ſame.* Ibid. p. 218.

XXX. The kingdom of *Bohemia* is a fine fertile country, rich in metals and minerals of all ſorts. The frontiers all round are very high mountains: the inward parts of the country are hilly, with plains and riſing grounds intermixed, that have the appearance of being the remaining baſes and ruins of former mountains, the ſoil being a compoſition of decayed rocks mixed with ſome vegetable earth. The rocks on the higheſt mountains are an aggregate ſtone of *lapides calcarii, ſpati, quartzi, micæ, &c.* The plains are covered with the leaſt diſſolvable parts of ſuch rocks. Their fineſt cryſtals, and precious ſtones, are gathered behind the plough; many ſtill retaining the ſame figures they had received at their formation in the veins and hollows of the rocks. I found on the tops of mountains decaying rocks, which, when mixed with a little vegetable earth, made exactly the ſame ſoil with that in the riſing grounds and plains below.

There are ſeveral places in this kingdom where the mountains are wholly of *lapis ſciſſilis*, which breaks into rhomboids; and I obſerved for many miles the ſhelves of this ſtone running through different mountains in the ſame direction, facing the S. E. with an inclination of the ſhelves of about 35°. The ſoil here in the plains is clayey.

Of ſome Foſſils found in Ireland; *by Mr* James Simon. N°. 477. p. 531. Aug &c 1745. *Read* Nov. 14. 1745.

XXXI. In my little excurſions in queſt of foſſils in this country, I found, *Sept.* 13. 1745. what Naturaliſts call *lac lunæ*; but think Dr *Plott* is miſtaken, when he gives it as a criterion or ſign of good limeſtone; for the two quarries where I found it were building ſtone, but will not burn into lime. This matter or earth makes a ſtrong ebullition with vinegar and ſpirit of vitriol. Some of it was as ſoft as creamcheeſe, when I took it out of the fiſſure of the rock; the other was hard, ſome in thin cruſts, and ſome in pretty thick lumps. It never was taken notice of in this kingdom before. I alſo found, about ſix weeks ago, white native vitriol, which I take to be the *capillaris* ſort; but as we have no Naturaliſt here, nor collection of foſſils, or any other natural curioſities (though in great plenty in this kingdom), it is hard for me to give names to ſuch as I have (about 800 articles) or do diſcover daily.

XXXII.

XXXII. What *ſpelter* is I don't well know, nor what uſes are already made of it; but I believe it was never yet applied to ſo large a work as the cylinder of a fire-engine, till Mr *Ford*, of *Colebrook-Dale* in *Shropſhire*, did it with ſucceſs: it run eaſier, and caſt as true as braſs, and bored full as well, or better, when it had been warmed a little: while cold, it is as brittle as glaſs, but the warmth of my hand ſoon made it ſo pliant, that I could wrap a ſhaving of it round my finger like a bit of paper. This metal never ruſts, and therefore works better than iron; the ruſt of which, upon the leaſt intermiſſion of working, reſiſts the motion of the *piſton*.

A letter from the Rev. Mr Maſon, Woodwardian *Prof. at* Cambridge, *and F. R. S. to the* Pr. *concerning* Spelter, *and melting* Iron *with Pit-coal.* N°. 482. p. 370. Jan. &c. 1746-7. *Read* Jan. 22. 1746-7.

Several attempts have been made to run *iron ore* with pit-coal; I imagine it hath not ſucceeded any-where, becauſe we have had no account of it's being practiſed; but I find that Mr *Ford*, from iron ore and coal, both got in the ſame dale, makes iron brittle or tough, as he pleaſes; there being cannon thus caſt ſo ſoft as to bear turning like wrought iron.

XXXIII. 1. I take the freedom to incloſe to you an account of a ſemi-metal called *Platina di Pinto*; which, ſo far as I know, hath not been taken notice of by any writer on minerals. Mr *Hill*, who is one of the moſt modern, makes no mention of it. Preſuming therefore that the ſubject is new, I requeſt the favour of you to lay this account before the *R. S.* to be by them read and publiſhed, if they think it deſerving thoſe honours. I ſhould ſooner have publiſhed this account, but waited, in hopes of finding leiſure to make further experiments on this body with ſulphureous and other cements; alſo with Mercury, and ſeveral corroſive *menſtrua*. But theſe experiments I ſhall now defer, until I learn how the above is received. The experiments which I have related were ſeveral of them made by a friend, whoſe exactneſs in performing them, and veracity in relating them, I can rely on: however, for greater certainty, I ſhall myſelf repeat them.

Several Papers *concerning a new* Semi Metal, *called* Platina; *communicated to the* Royal Society *by Mr* Wm Watſon, *F. R. S* N°. 496. p. 584. Nov. &c. 1750. *Read* Dec. 13. 1750. *Extract of a letter from* Will. Brownrigg, *M. D. F. R. S. to* Wm. Watſon, *F. R. S. Dated* Whitehaven, Dec. 5, 1750.

2. Although the hiſtory of minerals, and other foſſil ſubſtances, hath been diligently cultivated, eſpecially by the Moderns; yet it muſt be acknowledged, that, among the vaſt variety of bodies which are the objects of that ſcience, there ſtill remains room for new inquiries.

Memoirs of a Semi-metal called Platina di Pinto, *found in the* Spaniſh Weſt Indies, Ibid. p. 585.

No wonder that, among the great, and almoſt inexhauſtible varieties of ſalts, ores, and other concretes, new appearances, and mixtures before unknown, ſhould daily be diſcovered: but that, among bodies of a more ſimple nature, and particularly among the metalline tribe, ſeveral diſtinct ſpecies ſhould ſtill remain almoſt wholly unknown to Naturaliſts, will doubtleſs appear more ſtrange and extraordinary.

Gold is uſually eſteemed the moſt ponderous of bodies; and yet I have ſeen, in the poſſeſſion of the late Profeſſor *s'Graveſande*, a metalline ſubſtance, brought from the *Eaſt-Indies*, that was ſpecifically heavier

vier than gold, by at leaſt $\frac{1}{20}$ part. Mercury, next to gold, is commonly ſaid to be the heavieſt body; yet Mercury is greatly exceeded in ſpecific gravity by a ſemi-metal brought from the *Weſt-Indies*, whereof I have now the honour to preſent ſpecimens to the *Royal Society*. And this ſemi-metal ſeems more particularly to deſerve our attention, as it is endued with ſome very ſingular qualities, which plainly demonſtrate that certain general theorems, tho' long eſtabliſhed, and univerſally received by the Metallurgiſts, yet do not hold true in all caſes, and ought not to be admitted into their arts, without proper limitations and reſtrictions. For inſtance, *That gold and ſilver may be purified from all heterogeneous ſubſtances by coppellation*, is a propoſition that all aſſayers and refiners have long thought true and undeniable; yet this propoſition ought not to be received by thoſe artificers, without an exception to the ſemi-metal here treated of; ſince, like thoſe nobler metals, it reſiſts the power of fire, and the deſtructive force of lead in that operation.

This ſemi-metal was firſt preſented to me about nine years ago, by Mr *Charles Wood*, a ſkilful and inquiſitive Metallurgiſt, who met with it in *Jamaica*, whither it had been brought from *Carthagena* in *New Spain*. And the ſame gentleman hath ſince gratified my curioſity, by making further inquiries concerning this body. It is found in conſiderable quantities in the *Spaniſh Weſt Indies* (in what part I could not learn) and is there known by the name of *platina di pinto*. The *Spaniards* probably call it *platina*, from the reſemblance in colour that it bears to ſilver. It is bright and ſhining, and of an uniform texture; it takes a fine poliſh, and is not ſubject to tarniſh or ruſt; it is extremely hard and compact; but, like Bath-metal, or caſt iron, brittle, and cannot be extended under the hammer.

The *Spaniards* do not dig it in the form of ore, but find it in duſt, or ſmall grains, as herewith preſented to the *Royal Society*. Whether they gather it in a pretty pure ſtate, as brought to us, or waſh it, like gold-duſt, from among ſand, and other lighter ſubſtances, is to me unknown: however, it is ſeldom collected perfectly pure; ſince, among ſeveral parcels of it that I have ſeen, I conſtantly obſerved a large mixture of a ſhining black ſand, ſuch as is found on the ſhores of *Virginia* and *Jamaica*, which is a rich iron ore, and anſwers to the magnet. It hath alſo uſually mixed with it ſome few ſhining particles of a golden colour, which ſeem to be a ſubſtance of a different nature.

It is very probable that there is great plenty of this ſemi-metal in the *Spaniſh Weſt Indies*; ſince trinkets made of it are there very common. A gentleman of *Jamaica* bought five pounds of it at *Carthagena* for leſs than it's weight of ſilver; and it was formerly ſold for a much lower price.

When expoſed by itſelf to the fire, either in grains, or in larger pieces, it is of extreme difficult fuſion; and hath been kept for two hours in an air-furnace, in a heat that would run down caſt iron in 15 minutes: which great heat it endured without being melted or waſted; neither could

could it be brought to fuſe in this heat, by adding to it borax, and other ſaline fluxes. But the *Spaniards* have a way of melting it down, either alone, or by means of ſome flux; and caſt it into ſword-hilts, buckles, ſnuff-boxes, and other utenſils.

When expoſed to a proper degree of fire, with lead, ſilver, gold, copper, or tin, it readily melts and incorporates with theſe metals; rendering the mixture, like itſelf, extremely hard and brittle.

Having been melted in an aſſay-furnace, on a teſt with lead, and therewith expoſed to a great fire for three hours, till all the lead was wrought off, the *platina* was afterwards found remaining at the bottom of the teſt, without having ſuffered any alteration or diminution by this operation,

A piece of *platina* was put into ſtrong and pure *aqua fortis*, and therewith placed in a ſand-heat for 12 hours, the *platina*, when taken out of the *aqua fortis*, was found of the ſame weight as when put into it; being in no-wiſe diſſolved or corroded by that *menſtruum*.

It had been reported, that this ſemi-metal was ſpecifically heavier than gold; but having weighed ſeveral pieces of it hydroſtatically in a nice aſſay-balance, I found one of theſe pieces to weigh in air *gr.* $\frac{345}{8}$, and in water *gr.* $\frac{322}{8}$: ſo that it's ſpecific gravity was to that of water exactly as 15 : 1. Another piece, that ſeemed to be caſt very open and porous, I found in gravity to water only as 13.91 to 1. Although this laſt mentioned piece, could it have endured the hammer as well as gold, might probably have been reduced to a conſiderably greater degree of ſolidity than that of the firſt-mentioned ſpecimen. For the pureſt gold is ſeldom found, after fuſion, to come up to it's true ſpecific weight, until it hath been brought up to it's greateſt degree of ſolidity under the hammer.

I alſo weighed an equal mixture of gold and *platina*, which I found nearly as ponderous as gold itſelf; the ſpecific weight of this mixture being to that of water as 19 to 1.

It hath been reported, that the *Spaniards* have ſometimes been tempted to adulterate gold with *platina*, as the mixture could not be diſtinguiſhed from true gold by all the ordinary trials: but the gold thus adulterated was, upon a nicer examination, found hard and brittle, and could not be ſeparated from the *platina*, and rendered ductile and pure, either by cementation, or by the more ordinary operations with lead and antimony. In order therefore to prevent this fraud, the king of *Spain* commanded that the mines of *platina* ſhould be ſtopped up; ſo that this ſemi-metal is now much ſcarcer than formerly.

From the foregoing account it appears, that no known body approaches nearer to the nature of gold, in it's moſt eſſential properties of fixededneſs and ſolidity, than the ſemi-metal here treated of; and that it alſo bears a great reſemblance to gold in other particulars. Some Alchemiſts have thought that gold differed from other metals in nothing ſo much as in it's ſpecific gravity; and that, if they could obtain

a body that had the ſpecific weight of gold, they could eaſily give it all the other qualities of that metal. Let them try their art on this body; which, if it can be made as ductile as gold, will not eaſily be diſtinguiſhed from gold itſelf.

Upon the whole, this ſemi-metal ſeems a very ſingular body, that merits an exacter inquiry into it's nature than hath hitherto been made; ſince it is not altogether improbable, that, like the magnet, iron, antimony, mercury, and other metallic ſubſtances, it may be endowed with ſome peculiar qualities, that may render it of ſingular uſe and importance to mankind.

Specimens of Platina *preſented to the* Royal Society.

N°. 1. *Platina*, in duſt, or minute maſſes, mixed with black ſand, and other impurities, as brought from the *Spaniſh Weſt Indies*.
2. Native *platina*, ſeparated from the above-mentioned impurities.
3. *Platina* that has been fuſed.
4. Another piece of *platina*, that was part of the pummel of a ſword.

——*To the* Royal Society, *by Mr* Watſon. *dated* London, Dec. 13. 1750.

3. I beg leave to ſubjoin a few lines to Dr *Brownrigg*'s paper concerning the *Platina di Pinto*, or what is likewiſe called in *America Juan Blanco*. This ſubſtance is mentioned in no author I have met with, except by Don *Antonio d'Ulloa*, who, in the Hiſtory of his Voyage to *South America*, *Vol.* II. *Book* 6. *Chap.* 10. which I have here extracted, and tranſlated from the *Spaniſh*, when giving an account of the gold and ſilver mines in the province of *Quito*, and of the various methods of ſeparating theſe metals from other ſubſtances, with which they are combined, ſays, that, " in the territory of *Choco* . . . . there are gold " mines, in which that metal is ſo diſguiſed and enveloped with other " mineral ſubſtances, juices, and ſtones, that, for their ſeparation from " the gold, they are obliged to uſe quickſilver. Sometimes they find " mineral ſubſtances, which, from their being mixed with *platina*, " they chuſe to neglect. This *platina* is a ſtone (*piedra*) of ſuch reſi- " ſtance, that it is not eaſily broken by a blow upon an anvil. It is not " ſubdued by calcination; and it is very difficult to extract the metal it " contains even with much labour and expence."

In the before-mentioned work, *Chap.* 11. the ſame author, when ſpeaking of the remaining works of the *Indians* of old, ſays, " the *ſpe-* " *cula* wrought out of ſtones, which are found in the places of worſhip " of the *Indians*, are of two kinds, in relation to the matter of which " they are made: one of theſe is called *Piedra de Inga*, the other *Pie-* " *dra de Gallinazo*. The firſt of theſe is ſmooth, of a leaden colour, " and not tranſparent; they are uſually found wrought of a circular " figure: one of the ſurfaces is plain, and as ſmooth as though it were " made of a kind of cryſtal; the other ſurface is oval, or rather ſome- " what ſpherical, and not ſo much burniſhed as the plain one. Altho' " they vary in their ſize, they are commonly from 3 to 4 inches in " diameter; but he has ſeen one that was 1 $\frac{1}{2}$ foot in diameter. It's " principal ſurface was concave, and much augmented the ſize of ob-

" jects,

" jects, as it's poliſh was in as great perfection as though it had been " worked by a dextrous artiſt in theſe times.

" This ſtone has certain veins, or hair-like appearances, on it's ſur- " face; whereby it is rendered leſs fit for a *ſpeculum*, and is apt to " break in theſe veins in receiving any blow. Many are perſuaded, or " at leaſt ſuſpect, that the matter of theſe is a caſt compoſition; and " although there are ſome appearances of this being ſo, they are not " ſufficiently convincing. In this country there are gullies (*quebradas*) " where the mineral of them is found rough, and from whence ſome " are always taken; but theſe are not now wrought for thoſe purpoſes " for which heretofore they were employed by the *Indians*: but this is " no reaſon but that ſome of them may have been caſt, as with the " ſame material taken out of the mine, they may have been made ar- " tificially, and thereby have received a greater degree of perfection, as " well in their quality as in their figure." He ſays further, " that, " although at preſent, theſe, as well as ſeveral other things found there " are but of ſmall value, nevertheleſs they are extremely curious, and " worthy to be eſteemed, as well for their great antiquity, as for their " being the performances of thoſe barbarous people."

Some of theſe *piedras de inga* I now take the liberty of laying before the *Society*, both in their rough and in their poliſhed ſtate. They were brought hither with ſeveral other curioſities from *America*, by Don *Pedro Maldonado*, and were preſented by him to the *Preſident*, who was pleaſed to put them into my hands. They are doubtleſs of a metalline ſubſtance, and have, in my opinion, evident marks of having been fuſed and caſt. They very much reſemble, as you will ſee by comparing them, the *platina* before-mentioned: and though they are called (*piedras*) ſtones by Don *Antonio d'Ulloa*, he likewiſe gives the ſame appellation to the *platina*. I cannot therefore help recommending to ſome curious Metallurgiſt of the *Society* to make the experiment, whether or no, when the *piedras de inga* are, by a proper proceſs, diveſted of their ſtony and other heterogeneous parts, the metalline *reſiduum* will not reſemble, as well in ſpecific gravity (for which it is ſo remarkable) as in other properties, the purified *platina* now before us?

4. In *January* 1742-3. there were brought from *Jamaica*, in a man of war, ſeveral bars (as thought) of gold, conſigned from different merchants of that iſland, to their different correſpondents here, as bars of gold. Theſe bars had the ſame ſpecific gravity, or rather more than gold, and were exactly like that metal in colour, grain, *&c.* A piece of one of theſe counterfeit bars was ſent to the mint to be teſted, and it was found to be 21 carats 3 grains worſe than ſtandard. ——*by Mr* Emanuel Mendez da Coſta. *Read* Dec. 20. 1750.

5. The gentleman, whoſe experiments on *platina* I mentioned to the *Royal Society*, was Mr *Charles Wood*, who permitted me to make what uſe of them I pleaſed; and I did not pretend to have made any new diſcovery, nor to know ſo much of that body, as hath long been known to *Extract of a letter from* Wm. Brownrigg, *M. D. and F. R. S. to* Wm. Wat-

ſon, *F. R. S. containing ſome further experiments upon the* Platina. Ibid. p. 594. *dated* Whitehaven, Feb. 13. 1750. *Read* Feb. 28. 1750-51.

to the *Spaniards*. I might indeed have made uſe of his authority; but he was not ambitious of appearing in print.

The chief thing about which I had any difficulty, was what had been aſſerted of the *platina*'s reſiſting the force of lead in coppellation. This experiment I have tried therefore, by adding to *gr. xxvi.* of *platina*, 16 times it's weight of pure lead, that I had myſelf reduced from litharge. To the lead put into a coppel, and placed in a proper furnace; as ſoon as it was melted I added the *platina*, which in a ſhort time was diſſolved in the lead. After the lead was all wrought off, there remained at the bottom of the coppel a pellet of *platina*, which I found to weigh only *gr. xxi.*; ſo that, in this operation, the *platina* had loſt near $\frac{1}{5}$ of it's weight.

According therefore to this experiment, the *platina* does not wholly reſiſt the force of lead in coppellation; but, by repeated operations of that kind with larger quantities of lead, may probably all be deſtroyed: and by ſuch repeated coppellations, gold and ſilver may very likely be refined from it; although what was before aſſerted may hold pretty true, with regard to the common coppellations of the aſſayers and refiners.

Mr *Wood* ſaid, that, in his experiment, he thought the *platïna* rather gained than loſt in weight by coppellation. This might happen from ſome ſmall mixture of lead, or other metal continuing with it after it remained no longer fuſed.

From this ſingle experiment I will not be quite poſitive that lead thus conſumes ſome ſmall quantity of *platina*, ſince it is poſſible the *platina* uſed might not be pure. Beſides, in order to keep it longer in fuſion, I urged on the experiment with an uncommon degree of heat, eſpecially towards the end of the operation; although I think no great error could thence ariſe; as 3ſs of ſilver, which I coppelled at the ſame time, had loſt only *gr. ij.* in the operation.

I am told that one Mr *Ord*, formerly a factor to the *S. S. Company*, took in payment from ſome *Spaniards* gold, to the value of 500 *l.* ſterling, which being mixed with *platina*, was ſo brittle, that he could not diſpoſe of it, neither could he get it refined in *London*, ſo that it was quite uſeleſs to him: although, if no error hath been committed in the above-mentioned experiments, it might probably have been rendered pure by a much larger doſe of lead than is uſually applied for that purpoſe.

To my memoir I might have added, that, attempting to cleanſe a parcel of the native *platina* from the black ſand, wherewith it was mixed, I found that a great many of it's grains were attracted by the magnet I made uſe of for that purpoſe. This circumſtance I took notice of in a letter to Lord *Lonſdale* two years ago.

Extract of a letter from

XXXIV. As the Natural Hiſtory of *Perſia* is but little known, and the authors of the *Univerſal Hiſtory* have given no true account of the

ever-

everlaſting ſacred fire which the *Gauers* worſhip, I ſhall now ſend you a deſcription thereof, which you may depend upon, as there was a *Ruſſian* army for ſome years in the kingdom of *Dageſtan*, where that fire is; and I took down what I am going to relate from the mouths and journals of many officers that were there, and more particularly from what was communicated to me by *Archiater Fiſcher*, who received an account thereof from Dr *Lerch*, Phyſician of that army.

Dr James Mounſey, *Phyſ. of the* Czarina's *army, to* H. Baker, *F. R. S. concerning the* Everlaſting Fire *in* Perſia. N°. 487. p. 296. Apr. &c. 1748. *dated* Riga, Feb. 24. 1748. *Read* April 27. 1748.

This perpetual fire riſes out of the ground in the peninſula of *Abſcheron*, about 20 miles from *Baku*, and 3 miles from the *Caſpian* ſhore. The ground is very rocky, but has a ſhallow covering of earth over it. If a little of the ſurface be ſcraped off, and fire be applied to the hollow, it catches immediately, and burns without intermiſſion, and almoſt without conſumption; nor is ever extinguiſhed, unleſs ſome cold earth be thrown over it, by which it is eaſily put out.

There is a ſpot of ground, about two *Engliſh* miles large, which has this very wonderful property; and here is a *caravanſary*, round which are many places where the earth continually burns; but the moſt remarkable is a hole about 4 feet deep, and 14 feet in diameter. In this *caravanſary* live 12 *Indian prieſts*, and other *devotees*, who worſhip the fire, which, according to their traditions, has burnt many thouſand years. It is a very old vaulted building, and in it's walls are a great many chinks, whereto if a candle be applied, the fire catches inſtantaneouſly, and runs inſtantly wherever the chinks communicate; but it may be eaſily extinguiſhed: they have hollow places in the houſe fitted to their pots, which they boil without any other fuel; and inſtead of candles, they ſtick reeds into the ground; from the tops whereof, upon applying fire thereto, a white flame immediately comes forth, and continues to burn without conſuming the reeds, until they think proper to extinguiſh it, by putting little covers over them for that purpoſe.

They burn lime of the ſtones dug hereabouts, firſt making an hollow in the ground, and then heaping the ſtones on one another. This done, on applying fire to the hollow, a flame burſts out, and is diſperſed at once with a very great crack through the whole heap of ſtones; and after it has continued burning for 3 days, the *lime* is ready: but ſtones placed in this fire for ſetting their pots on never turn to lime; which cannot be made but by heaping them on one another. The earth and ſtone are no farther warm than where the fire reaches: and what ſeems very well worth obſervation, this flame of fire gives neither ſmoke nor ſmell, however great it be.

About an *Engliſh* mile and half from this place there are wells of *white naphtha*; which is exceedingly inflammable; and though the flame of *naphtha* affords both ſmoke and ſmell, it is highly probable the perpetual fire I have been deſcribing is owing to *naphtha*, but ſo purified, in filtering through the ſtone, that it becomes diveſted of all ſuch particles as produce ſmoke or ſmell. The ſtone and earth are grey in colour, and ſaltiſh to the taſte; and indeed much ſalt is found on this

peninſula

peninſula of *Abſcheron*. There is alſo a ſalt lake, near the ſide of which the *white naphtha* flows by 5 different ſprings. This *naphtha* is made uſe of only in the medicinal way. It is yellowiſh from the ſpring, but when diſtilled reſembles ſpirit of wine. They give it internally, for gonorrhœa's, diſorders of the breaſt, and for the ſtone; and they apply it externally in gouty caſes, contractions of the ſinews, and cramps.

Black naphtha is produced 8 or 9 miles from the perpetual fire; it is thick, and being diſtilled grows not clear but yellow. About *Baku* there is ſome of it ſo thick, that they employ it for greaſing wheels: but the beſt and greateſt plenty, is at *Balachame*, where there are above 50 ſprings, the greateſt whereof produces every day 500 *batman*, each *batman* containing ten *Ruſs* pounds, which are ſomewhat leſs than *Engliſh* weight. You hear it make a conſiderable noiſe in riſing out of the ground, though the ſpring be 20 fathom deep.

In *Baku* they have little or no other fewel to burn beſides *naphtha*, but it muſt be mixed with earth or aſhes to make it fit for uſe. The fire it makes is only good to boil with; and this inconveniency attends it, that all their food ſo boiled ſmells and taſtes of *naphtha*. For baking and roaſting they make uſe of *abrotanum*, *abſynthium*, and ſuch-like; but in general *naphtha* is their fire.

CHAP. IV.

MAGNETICKS.

Magnetical Experiments, *ſhewn before the* R. S. *by Mr* Gowin Knight, *on* Thurſday, Nov. 15. 1744. *Read the ſame day.* N°. 474. p. 161. June, &c. 1744.

I. MR *Knight*, of *Magd. Coll.* in *Oxford*, being introduced to a meeting of the *R. S.* produced, before the gentlemen there preſent, ſeveral curious artificial magnets contrived by himſelf; ſome of which conſiſted of plain bars of ſteel naked, and other of bars or blocks of the ſame ſubſtance, armed with iron after the common manner of natural loadſtones: but, as he was apprehenſive the trials he had before made of the weights theſe magnets were reſpectively capable of lifting, could hardly be repeated with ſufficient exactneſs and advantage before ſo large a company, he deſired to refer himſelf, for thoſe particulars, to what the *Preſ.* had ſeen at his lodgings on the 7th, and 13th of the ſame month.

Whereupon the Preſident acquainted the company, that he had lately been ſeveral times at Mr *Knight*'s lodgings, where he had ſeen many experiments made with his artificial magnets; and that, particularly on the days above-mentioned, he had been preſent, and had taken minutes of the following trials then made by that gentleman; by which it appeared, that,

A ſmall

A ſmall eight-cornered bar of ſteel, of the length of 3 inches, and almoſt $\frac{7}{10}$, and of the weight of about $\frac{1}{2}$ an ounce *Troy*, lifted by one of it's ends about 11 of the ſame ounces.

That anothe r plain bar of ſteel of a parallelopiped form, of the length of 5 inches $\frac{9}{10}$, the breadth of $\frac{4}{10}$, and the thickneſs of $\frac{2}{10}$ of an inch, weighing 2 ounces $8\frac{1}{2}$ pennyweight, lifted, in like manner, by one of it's ends 20 *Troy* ounces.

That a ſteel bar, almoſt of the ſame form as the laſt, but only 4 inches in length, capped or armed with iron at each end, cramped with ſilver, and weighing all together one ounce 14 pennyweight, lifted by the feet of the armour full 4 pounds *Troy*.

That a ſingle block of ſteel of a parallelopiped form, almoſt 4 inches long, 1 inch $\frac{2}{10}$ in height, and $\frac{4}{10}$ of an inch in thickneſs, armed with iron, cramped with braſs, ſuſpended by a ring of the ſame, and weighing all together 14 ounces 1 pennyweight, lifted by the feet of the armour 14 pounds $2\frac{1}{2}$ ounces *Troy* weight.

That a compound artificial magnet was alſo tried, conſiſting of 12 bars of ſteel armed; and that it was found to lift by the feet of the armour as the laſt, 23 *Troy* pounds, $2\frac{1}{2}$ ounces.

The 12 bars, compoſing this laſt magnet, were each a little more than 4 inches long, $\frac{3}{10}$ of an inch in breadth, and $\frac{16}{100}$ of the ſame in depth, weighing one with another about 25 pennyweight each. They were all placed one upon another, ſo as to make together one parallelopiped body, of the common length and breadth of the ſeveral bars, but of the height of near 2 inches, being the ſum of the reſpective thicknеſſes of all the bars taken together: and this parallelopiped body, being cramped with braſs, and fitted with an handle of the ſame metal, was armed at the 2 ends that were made up of the common extremities of all the bars, with 2 ſubſtantial pieces of iron, after the common manner of arming natural loadſtones, the whole frame weighing together about 20 *Troy* ounces.

Beſides theſe, the Preſident made alſo the following report of ſome trials he had ſeen made at the ſame time of the effects of an art Mr *Knight* is maſter of, by which he can improve or increaſe the lifting powers of natural loadſtones.

He carried with him, on *Wedn. Nov.* 7. a ſmall armed loadſtone belonging to an acquaintance, which weighed, with it's armour, 7 pennyweight 14 grains; but being reputed but of an ungenerous nature, took up, and with ſome difficulty, barely 2 ounces. Mr *Knight* took it into his ſtudy, and, returning it in about a minute, it then took up better than 4 ounces with eaſe: but, upon his ſaying, it would ſtill gain ſome more ſtrength, by remaining with him ſome time, it was left till the 13th, when it took up diſtinctly, with the ſame *apparatus* as before, 6 ounces 18 penny weights and 3 grains; ſince which time it has alſo ſeveral times been found to lift nearly the ſame quantity.

Mr *Knight*

Mr *Knight* further, at the ſame time, ſhewed the Preſident the following inſtances of his ability to invert or change the direction of the poles in natural loadſtones.

Such a ſtone belonging to Mr *Fr. Haukſbee*, weighing about 5 ounces and 14 pennyweights, of an irregular cylindrical form, with 2 of the ſides ſomewhat flatted, upon which armour had formerly been applied, had the direction of it's polarity from one of theſe flatted ſides to the other, notwithſtanding the ſtone had a diſtinct grain running at right angles to that direction. It was tried and obſerved, that one of theſe flatted ſides ſtrongly attracted the N. end, and repelled the S. and that the other attracted the S. and repelled the N. end of the magnetic needle. The end of the ſtone, attracting the S. end of the needle, was then marked, by the rubbing of a piece of ſilver upon it, as upon a touchſtone: after which Mr *Knight* carried the ſtone into his ſtudy; and, reproducing it in about a minute, ſhewed, that the poles were then directly inverted; and that the ſame end, which before attracted the S. end of the needle, now attracted the N. and repelled the S. and *vice verſâ*.

After this, Mr *Knight*, again taking the ſtone, brought it back in as ſhort a time as before, with the direction of it's polarity turned at right angles to it's former direction, and into the direction of the natural grain of the ſtone, the poles now lying in the flat ends of the cylinder; one of which, being the ſmoother end, attracted the S. end of the needle, whilſt the other, which was of a rougher texture, attracted the N. end, and repelled the S. end of the ſame: when it was alſo obſerved, that the polarity appeared ſtronger in this caſe, than either of the former.

Laſtly, Mr *Kuight*, in about the ſame time, inverted this laſt direction of the poles, keeping it ſtill parallel to the axis of the cylinder, but cauſing the ſmooth end of the ſtone to attract the N. end of the magnetic needle, and the rough end to attract the S. and repel the N. end of the ſame needle.

After this report, Mr *Knight* proceeded to ſhew, at the meeting, ſome of the ſame artificial magnets therein mentioned; and it was found, that the compound magnet, conſiſting of 12 ſteel-bars, which had, in the experiment made before the Preſident, lifted 23 pounds $2\frac{1}{2}$ ounces *Troy* weight, did here, under all the inconveniencies and diſadvantages of a crouded room, ſtill lift a weight amounting to 21 pounds and 11 of the ſame ounces.

It was alſo found, that the ſingle armed block of ſteel, which had before lifted 14 pounds and 2 ounces, did here, under the ſame diſadadvantages as the former, lift 13 pounds and 7 ounces of like *Troy* weight.

And, laſtly, Mr *Knight* produced to the company the above-mentioned natural loadſtone belonging to Mr *Haukſbee*, but with the direction of it's polarity again altered from what it was when it was laſt ſeen by the Preſident.

P. S.

P. S. Since the artificial magnets mentioned in the foregoing paper, Mr *Knight* has cauſed ſome others to be made of a leſſer ſize, but of a very great lifting power: and one of theſe, weighing without it's armour juſt an ounce, and with the armour, cramps, and rings, 1 ounce 17 pennyweights, lifted, before the Preſident of the *Society*, on *Friday July* 27, 1745. 6 pounds and 10 ounces *Troy* weight.

This magnet conſiſted of 3 plates of ſteel, each 2 inches long, $\frac{7}{100}$ of an inch in breadth, and not above $\frac{6}{100}$ of an inch in thickneſs: they were laid flat upon each other, and ſcrewed together by 2 ſmall braſs ſcrews going through the 3 plates. After which, the little parallelopiped block ſo made up, was armed with iron at the two ends, cramped together with ſilver, and fitted with a double ring of the ſame metal, for the convenient holding of it.

A collection of the magnet. Exp. *communicated to the* R. S. *by* Gowin Knight, *M.B. F. R. S. in* 1746, 1747. N° 484. p. 656. Oct. &c. 1747. *Read* Feb. 19. 1746-7. *An account of ſome* magnet. Exp *exhibited before the* R.S. *on* Thurſ. Feb. 19. 1746. *of which* the Preſident, *who had before ſeen the ſame performed with more deliberation on the 11th, was pleaſed to make this report.*

II. 1. Being on *Wed. Feb.* 11. at the houſe of Mr *Knight*, I did there in company with *Will. Jones*, Eſq; ſee the following experiments; which Mr *Knight* was deſirous I ſhould, as on this day, report to the *Society*: before whom he is alſo now prepared to exhibit the ſame, as well as the circumſtances of the place and the number of the company will allow.

He firſt produced two almoſt equal bars of hardened ſteel, to which he had communicated a ſtrong magnetic virtue. Theſe bars were nearly ſquare, each being of the length of about 15 inches $\frac{2}{10}$, and of the breadth and thickneſs of a little more than $\frac{1}{2}$ an inch: one of theſe bars weighed 2 pounds and 6 pennyweight *Troy*, the other 4 pennyweight leſs than 2 pounds; and either of them readily lifted with one of it's ends better than $3\frac{1}{2}$ pounds.

Theſe bars were then laid down on a table, ſo as to be nearly in one and the ſame ſtrait line, the N. pole of the one being next to the S. pole of the other, and at the diſtance of about an inch from it: that is to ſay, that the N. poles of both bars were pointed the ſame way, but without any regard to the poſition of the natural meridian.

Mr *Knight* then produced a piece of natural magnet, which was one of the ſame he had formerly made uſe of, in ſome experiments he had before ſhewed to the *Royal Society*. This piece was in length an inch and $\frac{1}{10}$, in breadth $\frac{7}{10}$, and in thickneſs about $\frac{3}{10}$ of an inch at a medium, being conſiderably thicker at the one end than at the other.

This piece of magnet was then applied, ſo as to lie between the 2 firſt mentioned bars, with it's thin end cloſe to the N. pole of one of them, and it's thick end cloſe to the S. pole of the other. After it had lain in this poſition a few moments, it was taken out, and upon preſenting it to the magnetic needle of a ſmall compaſs-box, it was obſerved that it's thinner end, the ſame which had juſt been contiguous to the N. pole of one of the bars, attracted the N. end of the needle; and that the thicker end, the ſame which had been contiguous to the S. pole of the other bar, attracted the S. end of the ſame needle.

This ſame piece of ſtone was then again put in between the bars, but in a contrary poſition; the thicker end now lying next to the N. pole of one of the bars, and the thinner end next to the S. pole of the other. After a few moments it was again taken out, and preſented as before to the compaſs-box: when it was found that the thin end now attracted the S. end of the magnetic needle, and that the thicker end attracted the N. end of the ſame.

The piece of ſtone was then again placed between the bars as at the firſt, and being again taken out and preſented to the compaſs-box: the thin end was again found as at the firſt to draw the N. end, and the thick end to draw the S. end of the needle.

This ſame piece of magnet was then again placed between the bars, but in a poſition at right angles to both the former, one of it's ſides being now contiguous to the N. pole of one of the bars, and it's other ſide to the S. pole of the other. After which being again in a few moments taken out, and preſented to the compaſs-box as before; it was found that the ſide which had been in contact with the N. pole of one of the bars, did attract the N. end of the needle, and that the other ſide which had been in contact with the S. pole of the other bar, did attract the S. end of the ſame needle: whilſt the two ends of the ſtone in which the polarity was before obſerved, were now found to be indifferent to either end of the needle; ſo that the line of direction of the poles in the ſtone now lay at right angles to the poſition in which it was ſituated in the former experiments.

Mr *Knight* then produced 2 ſteel needles, of the ſame ſort as thoſe which are uſually fixed to the cards of ſea-compaſſes. Theſe needles were of the length of 5 inches and $\frac{3}{10}$, and weighed ſeverally with their caps 7 pennyweight 8, and 7 pennyweight 9 grains; one of theſe was tempered and of a blue colour, and the other was quite hard. He alſo produced two iron weights, ſeverally weighing 14 pennyweight 22 grains, and 15 pennyweight 7 grains, both nearly of a cylindrical form, but with one of the ends rounded off.

The 2 large bars were then placed in a line, as in the former experiments, but with their ends ſo near together, as only to admit of the cap of one of the needles between them.

The tempered needle was then placed flat upon the bars, ſo that nearly one half of it reſted upon one bar, and the other half upon the other, the cap lying between the two. The needle was preſſed cloſe to the bars in this poſition, after which the bars were drawn away, both at the ſame time contrarywiſe, till they were clear of the needle; and this operation was repeated 3 or 4 times: after which that end of the needle which had reſted upon the N. part of one of the bars, was found ſtrongly to attract the N. end of the needle in the compaſs-box; and the other end which had reſted upon the S. part of the other bar, was found to attract in like manner the S. end of the ſame needle in the box. The power of attraction alſo acquired by this needle appeared to

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be very conſiderable, it lifting eaſily with either of it's ends, the two iron weights abovementioned, when cemented the one to the other with wax, and weighing together 1 ounce 10 pennyweights 5 grains.

The hard needle was then applied to the bars like the other and with the very ſame ſucceſs, it lifted alſo, as the other had done, both the weights together.

The two needles were then themſelves applied to each other, firſt the N. half of the one, in a contrary direction, to the N. half of the other; and then the S. half of the firſt, in a like contrary direction to the S half of the laſt; and from theſe ſeveral poſitions, they were ſeverally drawn till they were clear of each other, and this ſeveral times ſucceſſively: after which operation it was found that the tempered needle had loſt ſo far it's virtue, that it's N. end had hardly any effect upon the needle in the box; that it's S. end even began to attract the contrary end of the needle from what it did before, and that it was no longer able to lift at either of it's ends any ſenſible weight.

But as to the hard needle, that ſtill retained a conſiderable ſhare of it's former virtue; it's ends ſtill ſtrongly drawing the ſame ends of the needle in the compaſs box as they drew before, and either of them lifting with eaſe the heavier of the two above-mentioned weights.

Mr *Knight* then produced one of it's common ſmall magnetic bars; the which being applied to the forementioned large bars, in the ſame manner as the needles had been applied to the ſame, but in a poſition contrary to that of it's preſent polarity, it had it's poles thereby counterchanged or inverted, and was found to lift at that which was now become it's northern end, the weight of 6 ounces 8 pennyweight and 5 grains.

He laſtly produced one of his large artificial armed magnets, compoſed of ſeveral thin plates of ſteel cramped together, with which he acquainted us he had ſome time before lifted 36 pounds, and with which he did now actually lift before us 31 pounds 9 ounces and three fourths.

The tempered needle ſpoken of above, and which had nearly loſt all it's virtue, had the ſame again reſtored in great meaſure, upon being touched in the common way, on the armed poles of this artificial magnet; after which it diſcovered a ſtrong verticity, and was able to lift at one of it's ends, the heavier of the 2 abovementioned weights, that is to ſay ſomewhat more than three quarters of an ounce.

The hard needle which ſtill retained, as has been obſerved, a conſiderable part of the virtue it had acquired by the touch of the large ſteel bars, was laſtly touched alſo in a contrary ſenſe, upon the armed poles of this artificial magnet; whereby it not only loſt the polarity yet remaining, but acquired a new one the other way, it would not however after this laſt touch lift more than 9 pennyweight.

This is the true ſubſtance of the minutes I took, when theſe experiments were made, and which I preſume will now be verified by thoſe Mr *Knight* is here prepared to ſhew.

After the reading of this report, Mr *Knight* did accordingly produce before the *Society* the two large bars and all the other particulars therein mentioned, with which he publickly repeated all the ſame experiments; which notwithſtanding the diſadvantagious circumſtances of the place, ſucceeded perfectly in every particular, and to the entire ſatisfaction of all the company.

It was then further propoſed, that the tempered needle, having it's virtue again deſtroyed, ſhould be touched upon the fine armed *terella* belonging to the *Society*, which was the noble preſent of their late worthy member the R. Hon. *James* E. of *Abercorn*, and is eſteemed one of the beſt in *England*, and is ſaid to have lifted in his Lordſhip's hands upwards of 40 pounds: the ſame was immediately brought, and the needle being touched therewith, was found to have acquired a ſtrong polarity, and to lift about the ſame weight, as when it was before touched upon by Mr *Knight*'s large armed artificial magnet; that is to ſay, about 15 pennyweight.

An account of ſome new Exp. *lately made with* Artificial Magnets; *by the ſame.* Ibid. p. 662. *Read* July 2. 1747.

2. The *apparatus* for touching of needles, which I ſometime ſince had the honour to ſhew before the *R S.* was as perfect as I could have wiſhed, as far as relates to the intended uſe of it: but the manner in which the two bars were diſpoſed in their caſes made the length of them ſomething incommodious, eſpecially in thoſe of the largeſt ſize. This made me deſirous of trying if ſome method could not be found out of placing the bars parallel to each other without danger of weakening their force, by which means the caſes would be reduced to half their length. I remembered that ſome years ago, I had tried ſome experiments to this purpoſe, by placing ſome bars parallel and in contact, but ſo that their poles were turned different ways: in which poſition I found the virtue of ſome of them remained pretty entire, but that others were weakened thereby. I imagined the reaſon of their loſing their force was this; that the magnetic virtue was by degrees habituated to paſs out of the ſide of one bar into that of the other in contact with it, and thereby was hindered from arriving at the ends in it's full vigour. The reaſon why ſome ſuffered more than others was doubtleſs to be aſcribed to their difference in temper. I repeated the experiment about two months ago, with a little alteration. I placed the bars parallel with their poles in an alternate poſition, as before, but not in contact, having kept them at the diſtance of about $\frac{1}{4}$ of an inch. Then I applied to their ends two pieces of ſoft iron. Each piece was laid acroſs from the N. end of one bar to the S. of the other, in the ſame manner as the lifter is applied to the feet of an armed loadſtone. The intent of this was to draw the magnetic virtue thereby down to the ends of the bars, and to convey it through the pieces of iron from one to the other. In this condition I let them lie for about a month, and then tried if they would lift the ſame weight as before, which I found they did, and I thought with more vigour. After this I repeated the experiment with other

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bars of various ſizes, and with the ſame ſucceſs: I have therefore now ventured to fit them up in caſes in the manner juſt deſcribed.

The ſucceſs of this experiment had led me to another improvement: I provided a caſe of braſs that would juſt contain two bars, ſuch as are ſold for $\frac{1}{2}$ a guinea. At one end of the caſe were fixed two feet of ſoft iron, like thoſe of an armed loadſtone, the upper ſurface of which was within the caſe in contact with the ends of the two bars: which being parallel to each other, and their poles in an alternate poſition, the N. end of one bar will be in contact with one of the feet, and the S. end of the other bar will be in like manner applied to the ſurface of the other foot. Upon fitting a lifter to this new kind of armour, I found I was able to ſupport a weight of about 6 pounds: the bars are kept aſunder at the diſtance of about $\frac{1}{4}$ of an inch, by a ſlip of wood, which ſlides in betwixt them.

An inſtrument thus conſtructed ſeems capable of anſwering all the purpoſes for which loadſtones are uſed; for when the bars are taken out of the caſe, they are fit for touching needles, or other magnetical uſes, which may require ſingle bars; when in the caſe, the whole together becomes an armed magnet, able to lift a conſiderable weight. And if we want to ſeparate iron filings from thoſe of other metals, the feet and all the lower part of the caſe will take them up in great plenty, and by drawing the bars a little way out of the caſe the filings will fall off.

3. The cauſe of the ſurprizing *phænomena* of the load-ſtone has hitherto eſcaped our knowledge, though diligently inquired after by men of abilities. Such a diſcovery is not to be made without long experience, and a great variety of facts: and the nature of the ſubject is ſuch, that the more facts we are acquainted with, the more we find ourſelves perplexed. The concluſions we draw from ſome experiments are ſeemingly contradicted by others: and yet theſe ſeeming contradictions are oft-times very reconcileable upon further experience. If what I am about to lay before the *Society* will in any-wiſe contribute to remove theſe difficulties, I am in hopes it will not be unacceptable, though I ſhould not ſo properly explain the nature of the cauſe, as the manner in which it acts. Many of theſe experiments are not altogether new, but have not been ſo much attended to as they ſeem to deſerve.

Some further Exp. relating to the general phænomena of Magnetiſm; by the ſame. ibid p. 665. *Read* Dec. 17. 1747.

The magnetic matter of a load-ſtone moves in a ſtream from one pole to the other internally, and is then carried back in curve lines externally, till it arrives again at the pole where it firſt entred, to be again admitted. Prop. I.

If we lay a magnetical body under a piece of paper or glaſs that is ſtrewed over with ſteel filings or magnetical ſand, and by ſtriking the table put the filings in motion, they will readily diſpoſe themſelves in ſuch a manner as to repreſent, with great exactneſs, the courſe of the magnetic matter. Steel rendered magnetical is beſt for this purpoſe, Exp. I.

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becauſe it is of a more uniform texture, than load-ſtones, and will on that account exhibit a more regular appearance. By this exp. the curve lines in which the magnetical matter returns back to the pole where it firſt entered, are accurately expreſſed by the arrangement of the filings. The largeſt curves are ſuch as take their riſe from one polar ſurface, and are extended to the other; being larger in proportion as they ariſe nearer the axis or centre of the polar ſurface. Thoſe curves which ariſe from the ſides of a magnetical body, are always interior to thoſe which ariſe from the polar ſurface; and are leſs and leſs in proportion to their diſtance from the ends. If any one ſhould doubt, whether the magnetical matter, which thus diſpoſes the filings is really moving back in a direction contrary to that with which it paſſes through the magnetical body; let him try it in different parts with a ſmall compaſs needle, and the fact will appear beyond diſpute.

Exp. II. The larger the diſtance is from pole to pole in different magnets, the larger will theſe curves be. This appears from examining magnets of different lengths. And this is the reaſon why in the ſame magnet the curves are leſs in proportion to their greater diſtance from the ends of the bars. For the poles from whence theſe curves ariſe are proportionably nearer each other.

Exp. III. If the S. pole of one magnet be oppoſed to the N. of another, moſt of the magnetic matter is carried directly out of one into the other: and does not return back in curve lines till after having paſſed through both magnets. It appears from the arrangement of the filings that the magnetic matter proceeding from the polar ſurface, does not now diverge from the axis as before, but runs more in ſtreight lines till it arrives at the polar ſurface of the other magnet. The curves ariſing from the ſides, which before were bent towards the oppoſite end of the ſame magnet, are many of them now bent the contrary way towards the correſponding ſides of the other magnet. Thoſe which are not bent the contrary way, are ſuch as are too remote from the oppoſed pole of the other magnet to be influenced thereby; and therefore continue their natural courſe.

Exp. IV. Whilſt the bars are in the poſition of the laſt experiment, if a ſmall load-ſtone be placed in the ſtream running from one to the other in any poſition whatſoever, the ſtream will paſs through the ſtone: which being again removed, will be found to have a polarity exactly in the

Exp. V. direction of that ſtream.

If the N. or S. poles of two magnets be oppoſed to each other, the filings will exhibit the appearance of two ſtreams meeting; and the curves of each will all be turned towards the oppoſite pole of the ſame magnet. The appearance is altogether the ſame, whether two N. or two S. poles be oppoſed to each other. So that it is not to be determined from any of theſe experiments at which of the poles the magnetic ſtream enters. As we have ſome reaſon to think it enters at the N. pole, we may ſuppoſe that the caſe, without danger of error; provided we

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build nothing upon the ſuppoſition, but what would hold good (*mutatis mutandis*) if the contrary ſhould be true. This being ſuppoſed, when the S. poles are oppoſite, the two ſtreams coming out at them are directly contrary, whereby the magnetic matter is accumulated, and therefore diverges ſo much the faſter to return back to the N. poles. When the N. poles are oppoſed to each other, the ſtreams of magnetic matter returning from the S. poles are directly contrary; and by crouding at once towards each polar ſurface are accumulated betwixt them, and converge towards them ſo much the faſter.

Theſe 5 experiments ſeem ſufficient to eſtabliſh the truth of the propoſition; many more might be produced to the ſame purpoſe.

The immediate cauſe, why two or more magnetical bodies attract each other, is the flux of one and the ſame ſtream of magnetical matter through them. Prop. 2.

It appeared in the third experiment, that when the S. pole of one magnet was oppoſed to the N. of another, a ſtream of magnetic matter was carried from one to the other, and did not return back to the pole where it firſt entered, till after having paſſed through both bars; and it is needleſs to obſerve, that two bars in this poſition are in a ſtate of attraction. The fifth experiment ſhewed, that when the two S. or N. poles were oppoſed, there was no ſtream common to both. Now it is well known, that magnetical bodies in this ſituation are ſo far from attracting, that they ſtrongly repel each other. If the third experiment be repeated, with the magnets placed at different diſtances from each other, we ſhall find that more of the magnetical matter will paſs from one polar ſurface to the other, in proportion as the diſtance betwixt is leſs. The attraction is therefore greater as the diſtances diminiſh. And at diſtances where none of the magnetic ſtream paſſes from one magnet to the other, there is no ſign of attraction. So that this cauſe is not only co-exiſtent with the effect, but alſo proportionable thereto. *Exp.* VI.

If a piece of ſoft iron which has no fixed magnetiſm is any where placed in the magnetical ſtream, it will be in a ſtate of attraction whilſt it remains in that ſtream, and no longer. *Exp.* VII.

A ball of ſoft iron in contact with the pole of a magnet will attract a ſecond ball, and that a third, and ſo on, till the ſtream becomes too weak to produce an attraction ſufficient to ſupport a greater weight. *Exp.* VIII.

Having hung a number of balls to each other, by applying the firſt to the N. pole of a magnet, upon preſenting the S. of another magnet to one of the middle balls; all thoſe below it will thereby be deprived of the magnetic ſtream, and inſtantly loſing their power of attraction fall aſunder: the ball, to which the magnet was applied, will be attracted by it, and all the others will ſtill remain ſuſpended. But if the N. end of a magnet be preſented, then the ball to which it is applied will alſo drop. *Exp.* IX.

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Exp. X. In a magnet unarmed the magnetic ſtream is carried back on all ſides in curve lines to the contrary pole, as was ſeen in *Exp.* I. but when armour is applied to each pole, the magnetic matter is thereby conducted to the feet of the armour; and a lifter being thus applyed to the feet, the whole ſtream coming out at one pole is carried back through it to the other: by which means the lifter is made to adhere to the feet of the armour with very great force. When the lifter is thus in contact, the magnet ſeems externally to have loſt the greateſt part of it's force; though in reality it never acted with more. If inſtead of the lifter we ſuſpend a number of iron balls in contact, they will adhere together, and hang like a bracelet betwixt the two feet; the returning ſtream paſſing now through them, as before through the lifter. Preſent the pole of a magnet, and they inſtantly fall aſunder.

Prop. III. *The immediate cauſe of magnetic repulſion is the conflux and accumulation of the magnetic matter.*

It appeared in *Exp.* V. that the ſame poles of two different magnets being oppoſed to each other, there was a conflux and accumulation of the magnetic matter; and we find by experience, that all magnetical bodies in a like ſituation are in a ſtate of repulſion.

Exp. XI. Two ſmall bars, the one hard, the other of a ſpring temper, being both magnetical matter, were oppoſed to each other, S. to S. the filings produced the ſame appearance of repulſion, as deſcribed in *Exp.* V. then the bars being brought ſo near as to touch each other at the ſame poles, the repulſion was inſtantly changed into attraction.

A letter from the ſame to the Preſ. *concerning the* poles *of* magnets *being variouſly placed.* N°. 476. p. 361. Apr. *&c.* 1745. dated *London, April* 3. 1745. *Read* April 4. 1745.

III. The favourable reception which thoſe magnetical experiments met with, which you lately did me the honour to communicate to your learned *Society*, incourages me to hope, that the following facts are remarkable enough to merit their attention.

1. I cut a piece of natural loadſtone into the ſhape of a parallelopiped, 1 inch $\frac{8}{10}$ in length, in breadth $\frac{4}{10}$ of an inch, and $\frac{2}{10}$ in thickneſs; it's weight was 3 drams and 10 grains. In this ſtone I placed the magnetical virtue, in ſuch a manner that the two oppoſite ends became, both of them, S. poles; and the middle was, quite round, a N. pole.

2. Another ſtone was in length 1 inch $\frac{1}{10}$, in breadth $\frac{7}{10}$ and in thickneſs about $\frac{2}{10}$ at a *medium*, it being thicker at one end than at the other: its weight 1 dram 57 grains. The 2 oppoſite ends of this ſtone I made both N. poles, and the 2 oppoſite ſides S. poles.

3. An irregular ſtone, that weighed about 5 ounces and a half, had 2 broad flat ſurfaces oppoſite to each other, at the diſtance of 1 inch and $\frac{3}{10}$. I made half of each of theſe ſurfaces a N. pole, and the other half a S. pole; ſo that the N. pole of one ſurface was oppoſite to the S. pole of the other ſurface, and *vice verſâ*.

4. I took a ſtone of a pretty good kind, that had a grain very apparent, running the lengthways of it: it was 1 inch $\frac{4}{10}$ in length, 1 inch $\frac{3}{10}$ in

in breadth, and it's thickneſs at the ſides was $\frac{6}{10}$ of an inch; but in the middle $\frac{7}{10}$ it being tapered away from the middle to the ſides; it's weight was 3 ounces wanting 4 grains. At one end of it I placed a N. pole ſurrounded by a S. and at the other end a S. ſurrounded by a N. pole; ſo that the edges of each ſurface had a pole of a different denomination from that which occupied the middle.

A great many varieties of this kind might be eaſily deviſed, but the examples ſeem ſufficient to ſhew how manageable the magnetic virtue is in reſpect to it's direction; and how defective moſt of the hypotheſes are, which have been raiſed to account for the *phænomena* of the loadſtone.

A deſcription of a Mariner's Compaſs *contrived by* Gowin Knight, *M. B. F. R. S.* N°. 495. p. 505. Apr. &c. 1750. *Read* July 5. 1750.

IV. The diſcovery of the mariners compaſs has probably been of more general and important uſe to human ſociety, than the invention of any one inſtrument whatſoever: and yet ſo far have they been from ſtudying the improvement of it, that there would be no abſurdity in ſuppoſing that the firſt which was made might be as much ſuperior to thoſe in common uſe now, as the moſt improved inſtrument we have is ſuperior to it's firſt contrivance.

The compaſs which appeared before this *Society* laſt year on account of it's being rendered uſeleſs by lightning *, was what afforded me the firſt idea of their imperfections, ſome of which I then enumerated; but others have ſince occurred to me, ariſing from the ſtructure of the needle, which I had not ſufficiently conſidered at that time. It was then obſerved, that almoſt all the compaſſes on board our merchantſhips had their needles formed of two pieces of ſteel wire; each of which was bent in the middle, ſo as to make an obtuſe angle; and their ends, being applied together, make an acute one; ſo that the whole repreſents the form of a lozenge; in the centre of which, and of the card, is placed the braſs cap. I procured 20 cards, with needles of this kind fixed to them; and after touching them with a pair of large bars, I tried each of them, with the ſame cup and pin, by drawing them aſide 90° from the true point, and then ſeeing where they would reſt. I found them all to vary more or leſs, either to the E. or W. and ſome of them as far as 8°. Few of them came to the ſame degree twice together; and when they did, that was never the true point. In ſhort, they not only varied from the true direction, but from one another, and from themſelves. I then tried, by drawing them gently aſide, how far I could make them ſtand from the true point, without returning; and found they might frequently be made to do it at the diſtance of a whole point on either ſide. One of them, which generally varied 6 or 7° to the E. being drawn the ſame way, would ſtand at 16°.

All theſe irregularities are owing to the ſtructure of the needle: for the wires, of which it is compoſed, are only hardened at the ends; and

* See Art. VI. of this Chap.

that is done by making the ends red hot, and quenching them in water: if all theſe ends are not equally hard, or if one end be hardened higher up than the other, when they come to be put together, in fixing them to the card, that end which is hardeſt, will deſtroy much of the virtue of the other; by which means the hardeſt end will have moſt power in directing the card, and muſt conſequently make it vary towards it's own direction. If you retouch theſe wires when fixed to the card, the error will ſtill remain; for that wire which is beſt hardened will always become the ſtrongeſt. Conſidering how uncertain this method of hardening the ends of the wires muſt be, it is a great chance if they ſhould once in an hundred times be equally and uniformly hard: and unleſs they are, the card to which they are fixed muſt neceſſarily vary.

The wires being diſpoſed in the form of a lozenge, is the reaſon why theſe cards had ſo little force, that they might be made to ſtand at the diſtance of ſeveral degrees, on either ſide the point from whence they were drawn. For all magnetical bodies receive an additional ſtrength, by being placed in the direction of the earth's magnetiſm, and act proportionably leſs vigorouſly when turned out of it. Wherefore, when theſe kind of needles are drawn aſide from their true point, two of the parallel ſides of the lozenge will conſpire more directly than before with the earth's magnetiſm; and the other two will be leſs in that direction: by which means the two firſt ſides will very much impede it's return; and the two latter will have that impediment to overcome, as well as the friction, by their own force alone.

The needles that are uſed on board the men of war, and ſome of the larger trading ſhips, are made of one piece of ſteel, of a ſpring temper, and are broad towards the ends, but tapering towards the middle, where a hole is made to receive the cap. At the ends they terminate in an angle greater or leſs, according to the ſkill or fancy of the workman. Now, though the worſt of theſe are infinitely preferable to thoſe of wire, yet the beſt of them are far from being perfect. Every needle of this form has 6 poles inſtead of two. There is one at each end, two where it becomes tapering, and two at the hole in the middle. This is owing to their ſhape; for the middle part being very ſlender, it has not ſubſtance enough to conduct the magnetic ſtream quite through from one end to the other. All theſe poles appear very diſtinctly, when examined with a glaſs that is ſprinkled over with magnetic ſand. Nevertheleſs this circumſtance does not hinder the needle from pointing true; but as it has leſs force to move the card, than when the magnetic ſtream moves in large curves from one end to the other, it is certainly an imperfection.

I examined a hard needle of this ſort, whoſe ends were very broad, and terminated in an acute angle; and obſerved, that, tho' it's motion was very free and vigorous, yet I could make it ſtand one degree on either ſide the true point; and being at a loſs to account for it, I tried what appearance it would make under a glaſs with magnetic ſand, and diſcovered

diſcover'd that the magnetic ſtream came out of the ſides, which formed the acute angle at the ends, in lines that were almoſt perpendicular to thoſe ſides, and then was bent round to go to the other pole: from whence I concluded, that when the needle was drawn a little from the true point, the ſtream, which came out of one of theſe ſides, would be more in the direction of the earth's magnetiſm than before; on which account it would act ſtronger in retaining the needle in that ſituation, than the ſtream of the other ſide in reſtoring it; eſpecially as that ſtream would be now weaker, on account of it's being turned out of the magnetical line, and would have the friction betwixt the cap and pin to overcome at the ſame time.

I tried two other needles, whoſe ends were formed into angles very obtuſe, and could not find that they were liable to the ſame objection.

Two needles, that were quite ſtrait, and ſquare at the ends, were found to have only two poles; but about the hole in the middle the curves were a little confuſed. Theſe always came exactly to the ſame point, after vibrating a long time; and if drawn never ſo little on one ſide, would return to it again without any ſenſible difference. We may therefore conclude, that a regular parallelopiped is the beſt ſhape for a needle, as well as the ſimpleſt; with the holes for the caps as ſmall as can well be contrived; or if it can be made to anſwer the purpoſe without any hole at all, it will be ſtill more perfect.

Yet the common ſhape has one advantage which this has not: for, being made broad at the ends, and ſlender in the middle, it's weight is removed as far as poſſible from the centre: on which account, if it once points true, the friction at the centre cannot ſo eaſily put it in motion; and it's vibrations, when in motion, will be ſlower; ſo that their limits may be more nicely obſerved, and the middle point betwixt them is that where it would ſtand, if at reſt. Being unwilling to part with theſe advantages, I contrived a light circle of braſs, of the ſame diameter with the card, which will ſupply a weight acting at the greateſt diſtance from the centre of motion, and alſo ſerve to ſupport the card; which may now be made of thin paper, without any thing to ſtiffen it. So that the extraordinary weight of the braſs ring is compenſated in a great meaſure by the lightneſs of the card. This ring is of ſervice in another reſpect; for, being fixed below the card, and the needle above it, the centre of gravity is placed low enough to admit of the cap being put under the needle; whereby the hole in the needle becomes unneceſſary; and the latter being placed above the card, renders it eaſier to be touched with a pair of bars.

Having thus completed the needle and card to my ſatisfaction, what chiefly remains, is, to contrive ſuch a cap and point as will have the leaſt friction, and be moſt likely to continue in a ſtate of perfection. The caps in uſe are either of braſs, a mixed metal, like that of a reflecting teleſcope, cryſtal, or agate. The two firſt will only admit of braſs points, and the latter are rather too expenſive for common uſe. Where-

fore I bethought myſelf of trying glaſs caps: I had three of them made by a glaſs-blower, two of which I got poliſhed: they were all ſet in braſs, ſo as to ſcrew into the ſame needle, which had alſo one of agate fitted to it. I compared them with that of agate, by trying with each of them how many vibrations the ſame card and needle would make, when drawn aſide 90°, on the ſame point; which was a very ſmall ſewing needle.

The number of vibrations with the agate cap, on the firſt trial, were 39, then 37, then 39 again; with one of the glaſs caps it made 23, and then 20. This difference from the agate cap was ſo great, that I concluded the point muſt be damaged, and therefore choſe a finer; on which the ſame glaſs cap made 41 vibrations; then 43; and another glaſs cap made 47, and the next time 43. But the agate cap with this point made 51, 57, and 58 vibrations. The unpoliſhed glaſs cap performed much the ſame with the others. I had two of them poliſhed again by Mr *Smeaton*; and in company with him repeated the ſame experiments; but with no better ſucceſs. The agate cap made always many more vibrations than the glaſs one; and generally with the latter the number diminiſhed by repeated trials; whereas with the agate cap it uſually increaſed.

Theſe experiments made me lay aſide the thoughts of glaſs caps, and put me upon thinking how agate ones might be made with as little expence as poſſible.

With this view I got a cap turned of ivory, in ſuch a manner as to receive a ſmall bit of agate at the top. This being ground concave, and poliſhed on that ſide, where it formed the *apex* of the hollow cone in the cap, was capable of anſwering the purpoſe as well as if the whole had been agate, and was much lighter. Theſe caps may be made cheap enough for common uſe; and, if good at firſt, cannot eaſily be impaired.

For a point, I choſe a common ſewing needle, and contrived to fix it in ſuch a manner as to be taken out with the greateſt eaſe, and replaced by another, if neceſſary; by which means an excellent point may be always had with little trouble or expence. Common needles, when well tempered, have all the qualifications that can be deſired for the purpoſe intended. The ſmalleſt are ſtrong enough to bear the weight of a card; and are neither ſo ſoft as to be liable to bend, nor ſo hard and brittle as to break; and they are generally better pointed than any that a common workman could pretend to make *extempore*.

Thus we have gone through all the parts that are eſſential to a mariner's compaſs; and endeavoured to put them upon ſuch a footing, as to leave as little room as poſſible for error in their firſt conſtruction, or failure in the long continued uſe of them.

This, which I have now the honour to exhibit to the *Society*, was made by Mr *Smeaton*, a gentleman whoſe uncommon ſkill in the theory and practice of mechanics has enabled him to execute whatever I propoſed

poſed in ſuch a manner as always to exceed my expectations: and not only ſo, but he has added a conſiderable improvement of his own. By a very ſimple contrivance he has made the ſame inſtrument capable of ſerving the purpoſes of an azimuth and amplitude compaſs; and that in a manner much preferable to any thing hitherto contrived; the deſcription and uſe of which he has drawn up himſelf, for the peruſal of the *Society*.

An account of ſome improvements of the Mariners Compaſs, *in order to render the* Card *and* Needle *propoſed by Dr* Knight, *of general uſe; by* John Smeaton, *Philoſophical Inſtrument-maker.* Ibid. p 513. *Preſented* July 5, 1750. *here printed with alterations.*

V. The cover of the wooden box being taken off, the compaſs is in a condition to be made uſe of in the binacle, when the weather is moderate: but if the ſea runs high, as the inner box is hung very free upon it's centres (the better to anſwer it's other purpoſes) it will be neceſſary to ſlacken the milled nut, placed upon one of the axes that ſupports the ring, and to tighten the nut on the outſide that correſponds to it. By this means the inner box and ring will be lifted up from the edges, upon which they reſt, when free; and the friction will be increaſed, and that to any degree neceſſary to prevent the too great vibrations; which otherwiſe would be occaſioned by the motion of the ſhip.

To make the compaſs uſeful in taking the magnetic azimuth, or amplitude of the ſun and ſtars, as alſo the bearings of head-lands, ſhips, and other objects at a diſtance, the braſs edge, deſigned at firſt to ſupport the card, and throw the weight thereof as near the circumference as poſſible, is itſelf divided into degrees and halves; which may be eaſily eſtimated into ſmaller parts, if neceſſary. The diviſions are determined by means of a cat-gut line ſtretched perpendicularly within the box as near the braſs edge as may be, that the parallax ariſing from a different poſition of the obſerver may be as little as poſſible.

Underneath the card are two ſmall weights, ſliding on two wires, placed at right angles to each other; which, being moved nearer to, or farther from the center, counterbalance the dipping of the card in different latitudes, or reſtore the *equilibrium* of it, where it happens by any other means to be got too much out of level.

There is alſo added an *index* at the top of the inner box, which may be put on and taken off at pleaſure, and ſerves for all altitudes of the object. It conſiſts of a bar, equal in length to the diameter of the inner box; each end being furniſhed with a perpendicular ſtile, with a ſlit parallel to the ſides thereof. One of the ſlits is narrow, to which the eye is applied, and the other is wider, with a ſmall cat-gut ſtretched up the middle of it, and from thence continued horizontally from the top of one ſtile to the top of the other: there is alſo a line drawn along the upper ſurface of the bar. Theſe four, *viz.* the narrow ſlit, the horizontal cat-gut thread, the perpendicular one, and the line on the bar, are in the ſame plane, which diſpoſes itſelf perpendicular to the horizon, when the inner box is at reſt, and hangs free. This *index* does not move round, but is always placed on ſo as to anſwer the ſame ſide of the box.

When

When the ſun's azimuth is deſired, and his rays are ſtrong enough to caſt a ſhadow, turn about the wooden box, till the ſhadow of the horizontal thread; or (if the ſun be too low) till that of the perpendicular thread in one ſtile, or the light through the ſlit in the other, falls upon the line on the *index* bar, or vibrates to an equal diſtance on each ſide of it, gently touching the box, if it vibrate too far: obſerve at the ſame time the degree marked upon the braſs edge by the cat-gut line. In counting the degree for the azimuth, or any other angle that is reckoned from the meridian, make uſe of the outward circle of figures upon the braſs edge, and the ſituation of the *index* bar, with regard to the card and needle, will always direct upon what quarter of the compaſs the object is placed.

But if the ſun does not ſhine out ſufficiently ſtrong, place the eye behind the narrow ſlit in one of the ſtiles, and turn the wooden box about, till ſome part of the horizontal or perpendicular thread appears to interſect the centre of the ſun, or vibrate to an equal diſtance on each ſide of it, uſing ſmoked glaſs next the eye, if the ſun's light is too ſtrong. In this method another obſerver will be generally neceſſary to note the degree cut by the cat-gut line, at the ſame time the firſt gives notice that the thread appears to ſplit the object.

From what has been ſaid, the other obſervations will be eaſily performed; only in caſe of the ſun's amplitude, take care to number the degree by the help of the inner circle of figures on the card, which are the complements of the outer to 90, and conſequently ſhew the diſtance from E. or W.

The azimuth of the ſtars may alſo be obſerved by night; a proper light ſerving equally for one obſerver to ſee the thread, and the other the degree upon the card.

It may not be amiſs to remark farther, that, in caſe the inner box ſhould loſe it's *equilibrium*, and conſequently the *index* be out of the plane of a vertical circle, an accurate obſervation may ſtill be made, provided the ſun's ſhadow is diſtinct: for, by obſerving firſt with one end of the *index* towards the ſun, and then the other, a mean of the two obſervations will be the truth.

Explanation of the figures. Fig 102.

Fig. 102. is a perſpective view of the compaſs, when in order for obſervation. The point of view being the centre of the card, and the diſtance of the eye two feet. *AB*, is the wooden box. *C* and *D* are two milled nuts; by means whereof the *axes* of the inner box and ring are taken from their edges, on which they move, and the friction increaſed, when neceſſary. *EF* is the ring that ſupports the inner box. *GH* is the inner box; and *I* is one of it's *axes*, by which it is ſuſpended on the ring *EF*. *KL* is the magnet or needle; and *M* a ſmall brace of ivory, that confines the cap to it's place. See *Fig.* 103. The card is a ſingle varniſhed paper, reaching as far as the outer circle of figures, which is a circle of thin braſs, the edge whereof is turned down

down at right angles to the plane of the card to make it more ſtiff. *O* is a cat-gut line drawn down the inſide of the box; for determining the degree upon the braſs edge. *P Q R S* is the *index* bar, with it's two ſtiles and cat-gut threads; which being taken off from the top of the box, is placed in two pieces, *T* and *V*, notched properly to receive it. *W* is a place cut out in the wood, ſerving as an handle.

Fig. 103. is the card *in plano* with the needle fixed upon it; being one third of the diameter of the real card. Fig. 103.

Fig. 104. is a perſpective view of the backſide of the card, where *A B* repreſents the turning down of the braſs edge. *C* is the under part of the ivory cap. *D* and *E* are the two ſliding weights to balance the card; and *F* and *G*, two ſcrews that fix the braſs edge, *&c.* to the needle. Fig. 104.

Fig. 105. is the pedeſtal that ſupports the card, containing a ſewing needle, fixed in two ſmall grooves to receive it, by means of the collet *C*, in the manner of a port-creyon. At *D* the ſtem is filed into an octogon, that it may be the more eaſily unſcrewed. Fig. 105.

VI. *Jan.* 9. 1748-9. the new ſhip *Dover*, bound from *New York* to *London*, being then in Lat. 47° 30′ north, and long. 22° 15′ weſt, from *London*, met with a very hard ſtorm of wind, attended with thunder and lightning, as uſual, moſt part of the evening, and ſundry very large comazants (as we call them) over-head, ſome of which ſettled on the ſpintles at the topmaſt heads, which burnt like very large torches; and at 9 *p. m.* a ſingle loud clap of thunder with lightning ſtruck the ſhip in a violent manner, which diſabled myſelf, and great part of the ſhip's company, in the eyes and limbs; it ſtruck the mainmaſt about $\frac{2}{3}$ up almoſt half-through, and ſtove the upper deck one carling, and quick-work; part of which lightning got in between decks, ſtarted off the bulk-head, drove down all the cabbins on one ſide of the ſteerage, ſtove the lower deck, and one of the lower deck main lodging-knees.

Another part of it went through the ſtarboard ſide, without any hurt to the cieling (or inſide plank); and ſtarted off from the timbers four out-ſide planks being the whale upwards; one of which planks, being the ſecond from the whale, was broke quite aſunder, and let in, in about 10 or 15′, 9 feet water in the ſhip.

It alſo drew the virtue of the loadſtone from all the compaſſes, being 4 in number, all in good order before, one in a braſs and three in wooden boxes. The hanging compaſs in the cabbin was not quite ſo much diſabled as the reſt; they were at firſt very near reverſed, the N. to the S.; and after a little while rambled about ſo as to be of no ſervice. The ſtorm laſted 5 days, we loſt our mainmaſt and mizenmaſt, and almoſt all our ſails; arrived at *Cowes Jan.* 21. in a very ſhattered condition.

A letter from Capt. John Waddell *to Mr* Naphthali Franks, *Merchant, concerning the effects of* Lightning, *in deſtroying the* Polarity *of a* Mariners Compaſs; *to which are ſubjoined ſome* Remarks *thereon, by* Gow. Knight, *M. B. F. R. S.* N°. 492. p. 111. Apr. &c. 1749. *Read* April 13. 1749.

When

An account of the Mariners Compaſs, *that was ſtruck with* Lightning, *and ſhewn at the laſt meeting of the* Royal Society; *with ſome further particulars relating to that accident; communicated by* Gowin Knight, *M. B. F. R. S.*

When I came to examine the compaſs ſtruck with lightning, I obſerved that the outward caſe was joined together with pieces of iron wire, 16 of which were found in the ſides of the box, and 10 in the bottom. I applied a ſmall needle to each of theſe wires, and immediately perceived that the lightning had made them ſtrongly magnetical; particularly thoſe that joined the ſides. All the heads of the wires on one ſide of the box attracted the N. point of the needle, and repelled the S. whilſt all the heads on the the other ſide attracted the S. and repelled the N. The wires at the bottom attracted the S. and repelled the N. but it is not certain, whether this polarity was any-ways owing to the lightning; ſince it might be acquired by their continuing long in an erect poſture.

In examining the card, I found the needle was vigorous enough in performing it's vibrations, but that it's polarity was inverted; the N. point turning conſtantly to the S. I then tried to take out the card, to examine the ſtate and ſtructure of the needle: but the junctures were every-where well ſecured with putty, and that grown ſo hard, that I was obliged to uſe ſome violence, and at laſt broke the glaſs. The needle (if I may ſo call it) conſiſted of two pieces of ſteel wire, each of which was bent in the middle, ſo as to make an obtuſe angle; and the ends of theſe wires applied together, forming an acute one, the whole appeared in the ſhape of a lozenge; in the centre of which was placed the braſs cap whereon the card turned. And ſo far was it from being made with any tolerable degree of exactneſs, that there was not the leaſt care taken either to bend the wires in the middle, or to fix the cap exactly in the centre of the lozenge: for, upon trying it with a pair of compaſſes, I found it's greateſt eccentricity to be full $\frac{2}{10}$ of an inch. The pin, upon which it turned was made of a ſlip of plate-braſs ſharpened to a point.

Beſides the particulars already communicated to the Society, the Captain informed me, that he was obliged to ſail above 300 leagues, after this accident happened, without a compaſs, till he arrived at *Cowes* in the *Iſle of Wight*; where being provided with one, he placed it in the binacle, but was much ſurprized to find that it varied from the direction it ſtood at when out of the binacle nearly 2 points. He removed the binacle to different parts of the deck, but found that it always made the needle to vary after the ſame manner when placed in it. He repeated the ſame experiment lately in the river, with the like ſucceſs; only that he obſerved, that the variation of the needle, when placed in the binacle, was rather leſs than at firſt. It was natural to enquire if there was any iron about the binacle; but I was ſurprized when the Captain informed me, he had given ſtrict charge to the maker not to put ſo much as a ſingle nail in it; and that he firmly believed that there was not the leaſt bit of iron about it

Being willing to be ſatisfied of the truth of a circumſtance ſo very extraordinary, the Captain was deſired to ſend the binacle to a houſe in the

the City; where, in company with the Captain, Mr *Ellicot*, and another Gentleman, I tried it with a large compaſs touched by my bars; but finding no ſenſible variation, we at that time deſiſted, thinking the fact quite improbable: but having diſcovered the effect which the lightning had produced upon the wires which faſtened the ſides of the compaſs-box, I was induced to examine the binacle a ſecond time; which I did with a ſmall compaſs, and with great care, in every part; and at laſt, about the middle of the binacle, I found it to vary very ſenſibly, but could not diſcover any nails or iron any-where thereabouts; till, turning it up to examine the bottom, I there found 3 or 4 large nails or rather ſpikes, driven through it to faſten the upright partitions in the middle of the binacle.

It would not be difficult to explain why any needles, under the like circumſtances with thoſe above related, ſhould be rendered uſeleſs by lightning, though the needles themſelves had remained unhurt. So many iron wires made ſtrongly magnetical would doubtleſs have effected it; and 3 or 4 large nails in the binacle, if made magnetical, would alone have been ſufficient to have done it. But I have already taken notice that the polarity of the needle was inverted by this accident; and I would further obſerve, that all needles conſtructed after this manner are liable to be rendered uſeleſs not only by the lightning's deſtroying their virtue, but alſo by it's placing it in a particular direction; *e. g.* if the lightning ſtruck the needle in the direction of either of the two parallel ſides of the lozenge, it muſt ſtrike the other two ſides very obliquely; whereby the firſt two ſides may have their polarity deſtroyed, and a very ſtrong one given them in the contrary direction; whilſt that of the other ſides, if it be inverted, will be very weak; but it is probable that the virtue would be placed obliquely in the direction of the ſtroke; in either caſe, theſe two ſides can contribute but very little (if any thing) in directing the card; and if the two firſt ſides only are capable of acting upon it, it will point in the direction of thoſe ſides, which will produce a variation of about 4 points.

It may further be obſerved, that a needle would not continue long in this ſtate, but would every day grow more and more regular; becauſe if the virtue be placed obliquely, it generally turns itſelf in the direction of any piece of ſteel that is long and ſlender; and that may be the reaſon why this card is now become regular, except that it is inverted.

The wires that join the box ſeem weaker than when I firſt examined them; which makes it very probable, that they might be vaſtly ſtronger when firſt ſtruck with the lightning: and the ſame may be likewiſe true in regard to the nails in the binacle; which may account for the experiments not anſwering exactly the ſame as at firſt.

From what has been ſaid it appears, that this form of needles is very improper, and ought to be changed for that of one ſtrait piece of ſteel; and then if a needle ſhould be inverted it might ſtill be uſed. It alſo ſhews the abſurdity of permitting iron of any kind about the compaſs-

box, or the binacle. Whoever conſiders the whole deſcription here given of this compaſs, I am perſuaded, he will eſteem it a moſt deſpicable inſtrument: how then muſt any one be ſhocked to hear, that almoſt all the compaſſes, made uſe of by our trading veſſels, are of the ſame ſort! the boxes all joined with iron wire, and the ſame degree of accuracy obſerved throughout the whole!

This I am credibly informed, is the caſe; and that for no other reaſon, but that one of this ſort may be purchaſed for 5 *s.* and it will coſt about 2 *s.* 6 *d.* more to buy a tolerable good one. So that the lives and fortunes of thouſands are every day hazarded for ſuch a trifling conſideration.

Obſervations, made during thelaſt 3 years, of the quantity of the variation *of the* magnetic *horizontal* needle *to the W. by Mr* Geo. Graham, *F. R. S. at his houſe in* Fleet-ſtreet, London. N°. 487. p. 279. Apr. &c. 1748. *Read* Apr. 21. 1748.

VII.

				o	′
1745	*March*	26	- - - - -	17 =	0
		29	- - - - -	17 =	0
	March	18	- - - - -	17 =	10
		21	- - - - -	17 =	10
	April	22	- - - - -	17 =	15
	May	4	- - - - -	17 =	18
		14	- - - - -	17 =	20
		16	- - - - -	17 =	15
	Dec.	18	- - - - -	17 =	25 +
	Febr.	24	- - - - -	17 =	30
1747	*Dec.*	19	- - - - -	17 =	40
	Jan.	4	- - - - -	17 =	40 —

The inclination of the dipping needle has been during the ſame time about 73 $\frac{1}{2}$ degrees.

N. B. As the variation of the Needle at London *has not been regularly publiſhed from time to time in the* Philoſ. Tranſ. *it may not be improper to take notice here, that according to the beſt obſervations extant, and which were made by perſons of great ſkill and exactneſs, the Needle at* London *declined to the* E. 11° 15′ *in the year* 1580. *In* 1657 *there was no variation, the needle then pointing due N. In* 1672 *the variation was obſerved by the late Dr* Halley 2° 30′ *towards the* W. *and in* 1692 6° 0′. *And towards the beginning of the year* 1723, *it was found by* Mr Graham, *from the medium of a vaſt number of obſervations, to be then* 14° 17′ *the ſame way. So that, during the courſe of* 167 *years elapſed ſince the year* 1580 *to the end of the laſt year* 1747, *the magnetic needle at* London *has moved to the* weſtward 28° 55′. See before N°. 148, and N°. 383, of the *Philoſ. Tranſ.*

CHAP.

Fig. 102.

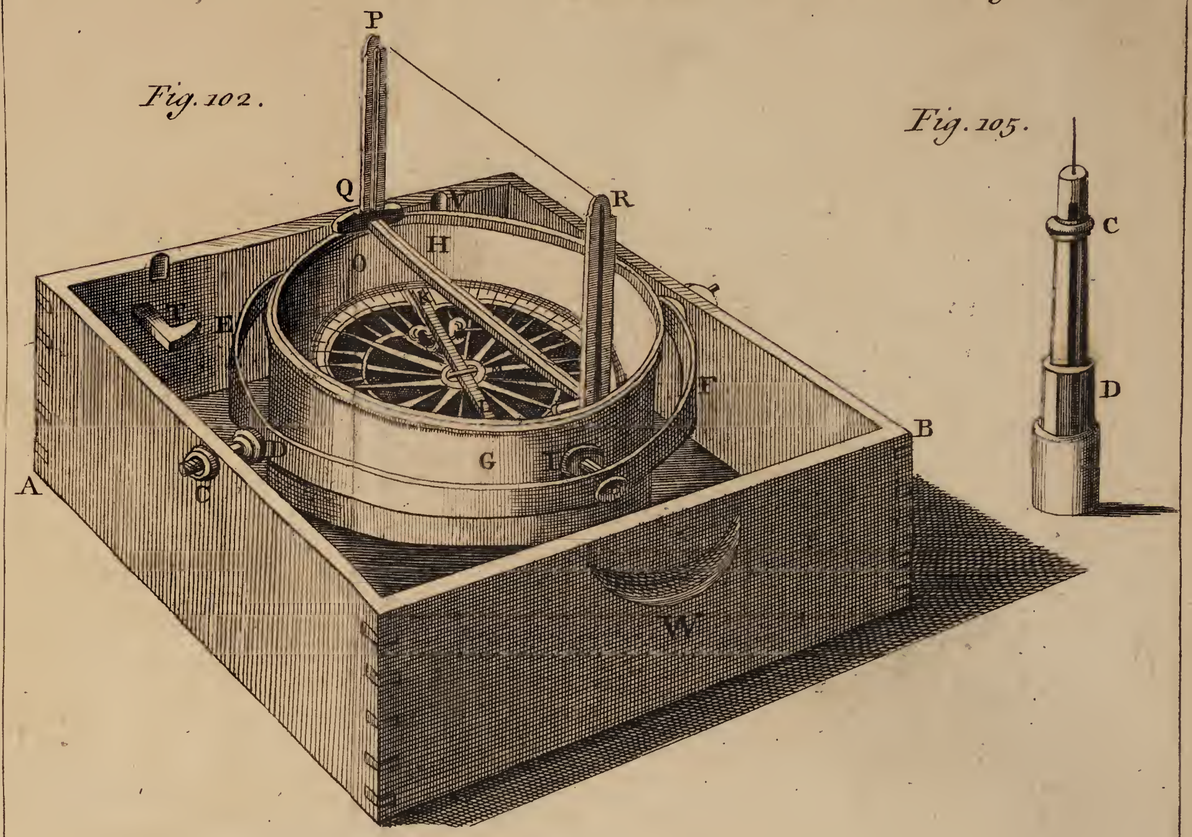


Fig. 105.

Fig. 103.

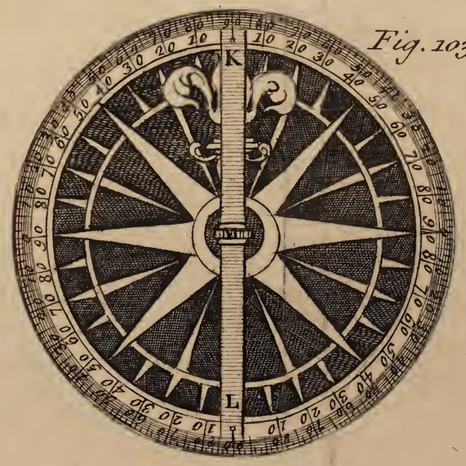
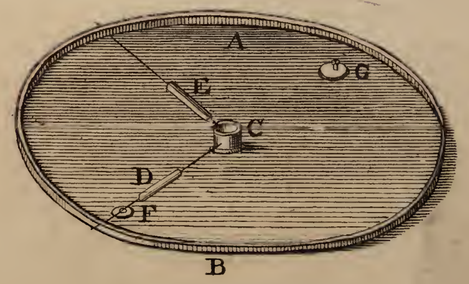


Fig. 104.



CHAP. V.

BOTANY, AGRICULTURE.

The eſtabliſhment of a new Genus of Plants, called Salvadora, with it's deſcription; by Laurence Garcin, *M.D. F.R.S. of* Neufchatel *in* Switzerland; *communicated in a letter to Dr* Mortimer, *Secr. R.S.* N°. 491. p 47. Jan. &c. 1749. *Tranſlated from the* French, *by* T. Stack, *M.D. Read* Feb. 23. 1748-9.

I. THIS plant is woody. It grows ſometimes into a tree, ſometimes into a ſhrub, and ſometimes into a buſh; ſpreading very tufted branches on all ſides down to the ground. It's native countries are the parts adjacent to the *Perſic Gulph*, the N. of *Arabia*, and the S. of *Perſia*. I cannot find that any author has known, or made the leaſt mention of it.

Firſt I will give it's characters, and then it's deſcription from accurate obſervations, which I have made on the ſpot.

Characters.

Calix. This is a monophyllous cup, divided into 4 lobes, which, as ſoon as they ſpread open, turn outward, and roll backward on themſelves; then wither, grow whitiſh, and dry up.

Corolla. It's flower is void of petals.

Stamina. Theſe are 4 in number, anſwering to the 4 lobes of the *calix*, and being likewiſe of the ſame length. They ſpring from the baſis of the *piſtillum*, and, as they ſhoot up, tend outward. Their ſummits are round, with a furrow turning in on one ſide; which gives each of them the form of a purſe.

Piſtillum. It is round, it's ſtyle ſingle and ſhort, and the *ſtigma* is blunt, and ſhaped like a navel.

Pericarpium. Is a round berry, of a middle ſize, with one cell or lodgment in it.

Semen. It is ſingle, ſpherical, incloſed in a callous firm ſkin, beſet with ſpots, forming a ſort of huſk like that of hemp.

I know but one ſpecies of this *genus*, which I deſcribe thus.

Deſcription.

It is a plant which varies conſiderably in ſize; that of a larger ſort of ſhrub, is what it moſt frequently grows to. It produces a number of boughs without order, and very tufted branches, which moſt commonly hang down to the ground. It's bark is moderately thick, ſometimes ſmooth, ſometimes full of cracks, of an aſh-colour, both in the trunk and branches, but green on the tender ſhoots. It's wood is every where brittle, and nearly of a ſtraw-colour.

The leaves are borne on young ſprigs, which ſhoot out along the boughs. Theſe ſprigs are ſtrait, generally ſhort, but ſometimes pretty long, like little wands. The leaves are thick-ſet, and tufted on the former, but thin on the latter. They grow ſometimes oppoſite to one

another by pairs, croſſing alternately; and ſometimes by three and three diſpoſed like rays; but this more rarely. Their length, which varies on the ſame ſtalk, is generally from 1 ½ inch to 2 ½ inches, and their width is from 9 lines to an inch a little below the middle in each, which is the wideſt part. They are thick, pointed at their extremity, and rounded at their baſe, very even on their edges, ſomewhat ſucculent, but firm: their colour is a pale green, but ſomewhat yellowiſh, in thoſe that are ſhooting out. The pedicles which ſupport them are very ſhort, each being but ½ a line in length, and ¼ in thickneſs. Every one of theſe pedicles, which is round, furniſhes a little nerve, which runs thro' the middle of the leaf; it is a little hollowed on the upper ſide, and ſomewhat raiſed on the back; and terminates at the end of it's reſpective leaf. This nerve gives 2 or 3 pair of almoſt imperceptible threads, which ſpread and divide into other ſmall irregular threads, through the body of the leaf. In fine, theſe leaves in ſhape nearly reſemble thoſe of the ſea-purſlain, and ſometimes thoſe of the miſletoe of the apple-tree. There are ſome generally on each plant, which have one, two, or more black ſpots, as in the *Perſicaria*, but almoſt round, and ſmaller.

The flowers, which are ſtamineous, that is, without petals, are ſmall, and diſpoſed in cluſters on the tops of the ſhoots. Theſe bunches of flowers intirely reſemble thoſe of the vine-bloſſoms. The empalement is ſmall, green on the under ſide, having four ſegments almoſt pointed, which roll outward, and then dry up. It's diameter in this rolled ſtate of it's lobes, is but of one line. The *ſtamina* are of a ſtraw-colour. The hollow furrow in each of their ſummits is not eaſily diſcovered without a glaſs.

The piſtil or *embryo* of the fruit, which is little, and yet occupies the whole inſide of the *calix*, is of the ſame colour with the bottom of this, that is, green. Afterwards it ſwells in all dimenſions, and grows into a berry, of the ſhape and ſize of a gooſeberry, of three or four lines in diameter. At firſt it is of a pale green, then a bright purple, and in it's maturity of a dark red. Each berry is ſupported on a ſtrong thick pedicle, attached to a ſmall bunch. It's ſubſtance is a white tranſparent fleſh, full of juice, much reſembling gelly, which ſurrounds a ſingle round grain, marbled with black or brown ſpots, as in the tortoiſe-ſhell, when ripe. This grain is as large as a grain of hemp-ſeed, that is, about two lines in diameter, but ſometimes leſs. It is properly a kernel, or a ſhell that has a cavity, which incloſes a ſort of little round almond, of a ſtraw-colour, yellowiſh on it's outward ſurface, and pale in it's inward ſubſtance, which is pretty firm.

Qualities. All the parts of our plant have an acid pungent taſte and ſmell, vaſtly like our garden-creſſes, but more biting. The fruit is the moſt pungent part of the whole. The ſmell of the plant is perceptible at 7 or 8 paces diſtance, when a perſon is to leeward.

The natives of the country uſe it againſt the bite of the ſcorpion, by rubbing the wounded part with it's bruiſed leaves. They employ alſo it's

it's warm infuſion to waſh the bodies of their children, in order to keep them healthy. And they feed camels with it, who love it naturally.

This ſhrub, which is ſometimes large, ſometimes ſmall, is moſt commonly found along high roads, and in dry low places of it's climate. As it's branches, which are ſlender and brittle, ſpontaneouſly bend downward, and form a thick tuft, this makes it generally reſemble a great buſh, which takes up a good deal of ground in moſt places, where it grows naturally. It delights in the hotteſt and drieſt places, ſuch as thoſe adjacent to the *Perſic Gulph*, and perhaps more ſo than palm-trees: wherefore I doubt of there being any growing in the countries that lie to the eaſt of the *Gulph*. And accordingly I have met with none, either in the neighbourhood of *Surat*, or in the kindom of *Bengal*, where there are regular rainy ſeaſons every year. *Remarks.*

I ſhould rather believe, it is more likely to be found in the deſerts of *Africa*, on this ſide of our tropic; thoſe being proper places for it, and where it rains ſeldomer than in any other part of the globe.

It's leaves have frequently excreſcences of different ſizes and ſhape, round, oval, and ſometimes very large. They are the work of thoſe flying inſects, which commonly abound in theſe parts.

The inhabitants of the *Gulph* call this ſhrub by the name of *Tchuch*. Perhaps it's nature would not allow it to grow in lands far diſtant from the ſea, no more than the *ſea-plants*, to which this ſurname is given for that reaſon.

It's parts are all brittle, and even the leaves crack, if bent in the middle.

In fine, I have choſen a name, which I imagined I ought to give it, after the example of Mr *Linnæus*, who has called ſeveral plants by the names of Botaniſts of reputation. This laudable proceeding is a way to perpetuate the memory of all thoſe who have contributed to the progreſs of Botany; and that much better than medals do with regard to Princes or Emperors. A proceeding, which, if duly purſued, will encourage thoſe who come after us, to make uſeful diſcoveries in this ſcience for the good of mankind, and in much greater number than have been publiſhed on the ſubject of plants up to our times. For it is eaſy to comprehend, that what remains to be diſcovered on this ſubject for our uſe, muſt infinitely ſurpaſs all that man has hitherto found out.

The name of *Salvadora*, which I have choſen for our ſhrub, is that of the late Mr *Salvador* of *Barcelona*, a very ſkilful Botaniſt, of whom M. *de Tournefort* makes mention in his Introduction to his *Inſt. rei herbariæ*, where he ſtiles him *the Phœnix of his Nation*; becauſe he was really the richeſt Naturaliſt, and the moſt expert in botanical matters that *Spain* ever produced. Before the laſt ſiege of *Barcelona*, in the years 1713 and 1714, they herborized together in *Catalonia*, and on the *Pyreneans*, while M. *de Tournefort* was on his travels there. They were intimate friends, and carried on a correſpondence ſome years: and as I was perſonally acquainted with him for 3 or 4 years, and have likewiſe

likewiſe herborized with him before the ſaid ſiege, and have ſince been honoured with his friendſhip and correſpondence, I thought it incumbent on me to do honour to his memory, by giving his name to this plant. And I have done it with the greater juſtice, becauſe it is certain, that, had he lived, he would have given a hiſtory of the plants of *Spain*, which, by it's accuracy, would have afforded much pleaſure to the Botaniſts of *Europe*.

To conclude; from the characters of our *Salvadora* it is manifeſt, that it's place in *Tournefort*'s ſyſtem ought to be in the firſt ſection of the 18th claſs. In the ſyſtem of Mr *Linnæus* it ought to be placed after the *Rivina* in the fourth claſs, which receives plants with 4 *ſtamina* (*tetrandria* and *monogynia*).

An aquatic Plant *found at* Bagneres *in* Gaſcony; *by* *M.* Secondat de Monteſquieu; *in a letter to* M. Folkes, *Eſq*; *Pr. R. S.* N°. 472. p. 31. Jan &c. 1744. *Read* Mar. 8. 1743-4.

II. I have found, at *Bagneres*, a particular aquatic plant, which I had ſeen, for the firſt time, in the great baſon of the boiling ſpring at *Dax*: it bears neither fruit nor flower, as far as appears; it's ſubſtance is entirely compoſed of ſmall bladders full of air; the ſurface of it is like net-work or canvas; it grows only in the hotteſt mineral ſprings; it may be found at the ſpring, called, *de la Reine*, at the *Bath des Pauvres*, and at the *New Spring*; but moſt plentifully at that place where part of the *Spring de la Reine* iſſues out of a rock near the *Capuchins*. Nobody, as far as I know, has ever ſpoken of this plant, before I gave an account of it two years ago, at the public reſumption of our academical meetings. The vegetation, and particular qualities of it, may, perhaps, deſerve to be more narrowly examined; and I believe it may be properly called, *Fucus thermalis veſicularis, ſuperficie reticulari*.

A deſcription of the Cyanus foliis radicalibus partim integris, partim pinnatis, bractea calycis ovali, flore ſulphureo; *by* Albert Haller, *Prof. Anat. & Bot.* Gotting. *R. S.* Aug. &, Suec. Soc. N°. 472. p. 94. Jan. &c. 1744.

III. The root is perennial: the leaves at the root are various, they are all green and firm, and have a ſmall quantity of ſhort down. Some are ſimple, with entire edges, and others are ſerrated. Others are half divided into unequal lobes. Others are variouſly ſemipennated and jagged. Others are quite pennated, with broad lobes, and the extreme one large and almoſt rhomboide. There are other leaves alſo of this ſort accompanying the ſtalk, with a long, firm, hollow rib, on which are placed a great many pairs of lobes, 12 or more.

The ſtalk is a cubit high, winged, and having leaves riſing under the origine of the branches.

The flowers are like thoſe of the *jacea vulgaris laciniata*, but of a full yellow colour, like gamboge, and without ſmell. The head is of the ſame ſize as in the figure.

The *calyx* is globoſe, contracted at the upper part. The green part of the ſcales is oval; the dry part ovato-rhomboide, yellowiſh, and fringed. In the upper ſcales it has ſome dry, thin, ſerrated additions. The crown is compoſed of barren, reflext, bilabiated, quadrifid petals. The ſeed of the fertile ones is crowned with down; the floret is long, crooked, quinquefid, having one ſegment deeper cut. The tube of

Fig. 106.

chives

Fig. 106.

chives appears out of the floret, and out of that a club-headed tube. The ripe ſeed is a flat oval, and crowned with black hairs.

It was ſent me by M. *Gerber* under the name of *jacea laciniata, flore luteo magno, ſquamis calycum ciliaribus, ſplendentibus.*

It's native place is in *Ruſſia*, or at leaſt on the banks of the *Wolga*.

By the empalement and flower, it is a ſpecies of *jacea*, according to *Vaillant*, according to me a *cyanus*, and according to *Linnæus*, a *centaurea*, under which name, he comprehends toò great a number of plants, to have convenient names aſſigned them.

IV. *Geaſter volvæ radiis & operculo elevatis.* *Geaſter* is a *genus* of plants conſtituted by *Micheli*; of which the author diſcovered 5 *ſpecies*, and figured them in his *Nova Plantarum Genera*. It is ſo called from γῆ *terra*, and ἀστὴρ *ſtella*, on account of ſome acute angles radiated from the centre in all the *ſpecies* of this plant, like the coruſcations of ſtars repreſented in pictures.

Concerning a plant but little known, and hitherto undeſcribed; by W. Watſon, *F. R. S.* N°. 474. p. 234. June, &c. 1744. *Read* Dec. 20. 1744. *here printed with alterations.*

This *genus* is very nearly related to the *Lycoperdon* (*a*) of *Tournefort* and (*b*) *Linnæus*, or *Boviſta* (*c*) of *Dillenius*, to the (*d*) *Lycoperdoïdes*, *Lycoperdaſtrum*, and (*e*) *Carpobolus* of *Micheli*, (*f*) *Linnæus*, and his follower (*g*) *van Royen* have compriſed all theſe plants under the generical name of *Lycoperdon*. But with all the reſpect due to thoſe eminent maſters of Botany, to whoſe rules in conſtituting the *genera* of plants from the fructification I ſtrictly adhere; yet in many of the *Cryptogamia* of *Linnæus*, as well as in the *Cryptantheræ* of *van Royen* it ſeems to be abſolutely neceſſary to have recourſed to the habit of the plant, in conſtituting *genera*; eſpecially in the *Fungi*, *Fuci*, *Algæ*, and perhaps in the Capillaries.

In order to form a deſcription of this wonderful plant, I ſhall conſider it in it's 3 parts, the *volva*, the *operculum*, and the *fructification*.

The *volva*, to ſpeak not only with *Pliny*, but with *Cluſius* alſo, and other moderns, is concave, repreſenting the form of a cup, 1 $\frac{1}{2}$ inch in diameter. This cup has a broad baſe, from the centre of which, whilſt the plant is ſtill growing, it ſends forth ſmall fibrous roots, but few like the reſt of it's kind that ſupply it with nouriſhment. But as moiſture is by no means agreeable to this whole family of plants, the little roots, together with the centre of the *volva*, into which they were inſerted, wither before the plant comes to maturity; which cauſes a hole to appear in the bottom of the baſe. But when the *volva* is riſen to $\frac{1}{3}$ of an inch, it becomes quadrifid, having jaggs obtuſely laciniated, a little reflexed at the point but entire at the edges. This *volva* is elaſtic; on the outſide it is of an aſh colour with an uneven ſurface, but it's inſide is ſmooth and whitiſh.

(*a*) Inſt. R. Herb. p. 563. (*b*) Linn. Gent. Plant. p. 510. (*c*) Cat. Giſſ. p. 196. (*d*) Michel. Nova Plant. Gen. p. 221. (*e*) Ibid. (*f*) Linn. Gen. p. 510. H. Clifford. p. 479. (*g*) Flor. Ledenſ. Prod. p. 518.

From

From earth of the tips of the reflexed ſegments of the *volva* ariſes, that part of the plant, which I call the *operculum*. By the gradual extenſion of this part, from it's joining with the *volva* are formed 2 arches, the heighth of which from the ſegments of the *volva* does not exceed $1\frac{1}{2}$ inch. The ſubſtance and thickneſs of the *operculum* equal thoſe of cinnamon; it turns up a little at the edge, it is whitiſh within, and of a reddiſh yellow without. We may add, that a ſort of brown membrane, divided into 4 unequal parts, adheres to the top of each arch.

From the centre of the *operculum* juſt deſcribed, on a pedicle ſcarce $\frac{1}{4}$ of an inch in heighth, appears a *pericarpium*, of an oblate ſpheroïdal figure, brown, $\frac{2}{3}$ of an inch broad, reſembling the head of a poppy. At the top is a circular hole, in which are ſome *lamellæ*, adhering to the inſide of the *pericarpium*, through it's whole length, filled with a woolly ſubſtance; and to theſe *lamellæ* a great number of ſeeds, like a very fine duſt, adhere, as to ſo many *placentæ*.

It may ſeem perhaps very difficult to many Botaniſts to comprehend, by what means a proper nouriſhment is ſupplied to the *operculum* and *pericarpium*, whilſt the plant is in a flouriſhing ſtate. They muſt therefore be informed, that in the more tender ſtate of the plant, the *volva* and *operculum* lie ſpread on the ground, not unlike ſtar-fiſhes, and are joined together with a ſort of glutinous ſubſtance, by means of which they are nouriſhed together with the *pericarpium* and ſeed. In this ſituation all the ſpecies of *Geaſter* are ſhewn by the celebrated *Micheli*; as is alſo the *Fungus crepitus lupi dictus coronatus & inferne ſtellatus* firſt mentioned by our great Mr *Ray* (*a*), of which there is a figure (*b*) in the third edition of his *Syn. Stirp. Brit.* But when the ſeed is ripe, the glutinous matter, which lies between the *volva* and the *operculum*, dries up; which makes them rigid and elaſtic; whence they appear divided, except at the tips of the ſegments. Things being thus conſtituted, the *radii* of the *volva*, the *operculum*, and the fruit, gradually ariſe, and the whole plant nearly repreſents an arched tower. This manner of reaſoning, for no one has hitherto had the opportunity of examining it as it grows, ſcarce admits of any doubt, ſince the wrinkled exterior coat of the *volva* at this very time contains not only ſand, but alſo a ſmall ſtone. We learn alſo on the authority of *Micheli* (*c*) that the *Carpobolus*, which, as I ſaid before, is very nearly related to the *Geaſter*, cannot only raiſe it's *operculum* from concave to convex, but even do it violently in an inſtant, ſo that it's ſmall globoſe fruit is thrown up on high.

I have never ſeen more than 2 ſpecimens of this plant, which were communicated to me by Mr *Robert Nicholls*, Apothecary of London. The larger was gathered not far from *Reading* by Dr *Merrick*: the ſmaller was found near *Wickham* in *Kent*; both of them about the end of *March*.

A. the

(*a*) Raii Synopſ. Ed. 2. p. 16. (*b*) Tab. I. (*c*) Nov. Plant. Gen. Tab. 101.

A. The *Pericarpium.* *B.* the *Operculum.* *C.* the *Volva.* Fig. 107.

The *Pericarpium* ſeen in front; of which *A* is the circular *foramen.* Fig. 108.

An account of a new ſpecies of Fungus, *by* John Martyn, *F. R. S. Prof. Bot.* Cantab. N°. 475. p. 263. Jan. &c. 1745. *Read* Jan. 24. 17$\frac{44}{45}$.

V. In the latter part of the ſummer of 1744, Mr *Ehret* the Painter brought me a *Fungus* of a very extraordinary ſhape and ſize, which had been found growing on a piece of the trunk of an elm, in a damp cellar in the Hay-Market.

The whole plant was about 2 feet in height; and, at firſt ſight, ſeemed not very unlike the horns of ſome deer, being variouſly branched, and covered with a thick down. It was of a ſpongeous ſubſtance, and of a duſky-red colour inclining to black. The tips of the ſmaller branches were of a cream-colour. The larger branches or rather the tops of the whole plant, were expanded in form of a funnel, ſmooth on the concave, and full of pores on the convex ſide. The inner and lower part of the funnel was of the ſame colour with the ſtalk; the reſt of it was of a cream-colour

I have not been able to find, that this plant has been mentioned by any author: and am perſuaded, that it is a new ſpecies; and, perhaps, the remarkable branching of the ſtalks may induce ſome to think it a new *Genus.* As the funnel may be eſteemed a cap, and as this cap is not lamellated, it will be a *Boletus*, according to the method obſerved in the 3 Edit. of *Ray's Synopſis.* According to *Micheli* it ſeems to belong to the *genus* of *polyporus.* The method, which I have long uſed in the diſtribution of this claſs, is expreſſed in the following ſynoptical table, which, I think, comprehends all the ſpecies hitherto known.

FUNGI ſunt,
- lamellati,
  - cauliferi; AMANITA.
  - ſeſſiles; AGARICOIDES.
- poroſi,
  - cauliferi; BOLETUS.
  - ſeſſiles; BOLETOIDES.
- cancellati, aut ſcrobiculis excavati;
  - ex pila erumpentes; PHALLUS.
  - ex pila non erumpentes; MERULIUS.
- echinati; ERINACEUS.
- in pulverem abeuntes; LYCOPERDON.
- ſolidi,
  - cauliferi; CHANTERELLA.
  - ſeſſiles,
    - calyciformes; PEZICA.
    - non calyciformes,
      - in longitudinem producti; DIGITELLUS.
      - horizontaliter prodeuntes; AGARICUS.
      - ſubterranei; TUBER.

According to this method of mine, as well as that of the Editor of *Ray*'s *Synopſis*, the plant in queſtion will be a *boletus*: and, as I do not think it neceſſary to conſtitute a new *genus*, I have taken the liberty to call it

BOLETUS caule ramoſo; ſummitatibus concavis expanſis; ramis minoribus in acutum mucronem deſinentibus. See Fig. 109.

Fig. 109.

A deſcription of a curious Sea-plant; by Sir Hans Sloane, *Bart. M. D. late* Preſ *R. S. and* Col Med. Lond. *&c.* N°. 478 p. 51. Jan. & Feb. 1746. *Read* Feb. 6. 1745-6.

VI. King *Charles* the ſecond had in his cloſet at *Whitehall*, this coralline (as I call it)*; which, I ſuppoſe, had been preſented to him by ſome of his ſea-officers, appointed to cruiſe in the ſoundings, lying off the W. of *England*, towards the *Atlantic* Ocean. I have had it from thence entire, and in perfection, from ſome of the late commanders on that ſtation (of which I here give an entire figure when young) who, by their ſounding lines, brought it up from the rocks at the bottom of the ſea; and which being a very curious coralline, I wonder it has been ſo little taken notice of.

Fig. 110.

It riſes to 4 feet high, from a woody baſis, near an inch diameter, giving it a firm foundation on the rocks in the bottom of the ſea, ſpreading out it's branches like a fan, the ſubſtance or inner part of which is woody, of a light brown, or blackiſh colour (as at *a*, *b*,) covered all over with a thin tuberculated cruſt, of an aſh-colour, or ſometimes yellowiſh, ſeldom joined together, as the *rete marinum*, but looſe, and diſtorted; and not ſtrait, as moſt of this kind.

I have had it from *Tangier*, *Antigua*, and *Newfoundland*; from which laſt place, one with the *ſtella arboreſcens* Rondeletii, *p.* 121. (mentioned by Mr *Winthorp*, in theſe *Tranſ.* N°. 57. *p.* 1152.) having it's branches faſtened ſeveral times round thoſe of this coralline; a branch of which is here figured, with the animal ſticking to it, at *Fig.* 111. in which *a* is the mouth, and *Fig.* 112. repreſents the back part of it, having a crack in it by ſome accident. The fineſt of this kind was given me by the late Ducheſs of *Beaufort*; who told me, ſhe had it preſented to her by the late Colonel *Codrington*, Proprietor of the iſland of *Barbuda*; from whence in all likelihood he had it.

Fig. 111.
Fig. 112.

I do not pretend to give a new name to this coralline, to make confuſion; but only mention ſuch authors, as have already taken notice of it; of whom *John Bauhin* is the firſt that deſcribes it plainly, both by words, and an imperfect figure of a ſmall piece or branch, which he had communicated to him by a perſon whom he does not name, by reaſon (as I ſuppoſe) he had by ſtealth broke it off a large branch, kept in what he calls *Theatrum Naturalium Sereniſſimæ Reginæ Angliæ*; whom I ſuppoſe to be Queen *Elizabeth*; and which, to my knowledge, is too much practiſed ſince by unworthy perſons.

* Frutex *marinus* Flabelliformis *cortice verrucoſo obductus*. Doodii *Raii* Hiſt. *Tom.* III. *p.* 7. *& Syn. Ed.* 3. *p.* 32. Coralloïdes *granuloſa alba*. *J. B. Tom.* III. *p.* 809. Erica *marina alba fruteſcens*. *Muſ.* Pet. 50. Keratophyton *Flabelliforme, cortice verrucoſo obductum*. Raii *Syn. Ed.* 3. *p.* 32.

It

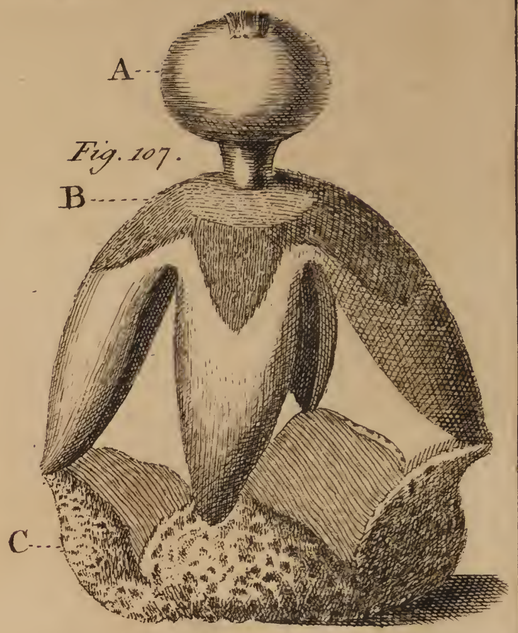


Fig. 107.

Fig. 108.

Fig. 109.

Fig. m.

Fig. no.

Fig. n2.

It is likely, that many of the coralline ſubſtances mentioned by authors, may be this, or parts of it, the cruſt being rubbed off more or leſs, and it's colour changed, and thereby deſcribed for different corallines.

VII.

N°. 474. p. 189. Ann. 1742. 1001. *Abrotanum Lini folio acriori & odorato* Tourn. *Draco herba* Park. Tarragon.

N°. 472. p. 75. Ann. 1741. 951. *Abſinthium Ponticum Galeni* Ger. Roman or Cypreſs Wormwood.

952. *Abſinthium Tanaceti folio odoratiſſimum* Amm. *p.* 142. The moſt odorous Wormwood with Tanſey leaves.

N°. 474. p. 189. Ann. 1742. 1002. *Abutilon* Americanum *Ribeſii folio, flore carneo, fructu pentagono aſpero* Houſt. American Abutilon with currant leaves, a fleſh-coloured flower, and a rough five cornered fruit.

N°. 494. p. 359. Ann. 1748. 1301. *Abutilon Lavateræ flore, fructu criſtato Hort. Elt.* Abutilon with a flower like Lavatera, and a creſted fruit.

N°. 480. p. 213. Ann. 1744. 1101. *Acacia* Americana, *flore albo, pinnis latiuſculis glabris, ſiliquis latis* Houſton. American *Acacia*, with a white flower, broad ſmooth leaves, and broad pods.

N°. 480. p. 213. Ann. 1744. 1102. *Acetoſa arboreſcens, ex* Inſulis Fortunatis *Pluknet.* Shrubby Sorrel with a round leaf, from the *Fortunate Iſlands.*

N°. 494. p. 359. Ann. 1748. 1302. *Acetoſa rotundifolia repens Eboracenſis, folio in medio deliquium patiente* *. Creeping round-leaved Sorrel of the North.

N°. 474. p. 189. Ann. 1742. 1003. } N°. 475. p. 403. Ann. 1749. 1351. } *Aconitum cæruleum, ſive Napellus C. B.* 183. Blue Helmet-flower or Monks-hood.

N°. 476. p. 421. Ann. 1743. 1051. *Aconitum hyemale Ger. Park.* Winter Wolfs-bane.

A catalogue of plants preſented to the R. S. *by the Comp. of Apothecaries of* London, *purſuant to the direction of Sir* Hans Sloane, *Bart. by Mr* Joſeph Miller, *Apothecary*, Hort. Chelſ Præf. & Prælect. Botan.

* Mor. *Hiſt.* ii. 583.

N°. 484. p. 597. Ann. 1745. 1152. *Aconitum Lycoctonum cæruleum calcari oblongo J. B.* Blue Wolfs-bane with a large ſpur.

N°. 474. p. 189. Ann. 1742. 1004. *Aconitum Lycoctonum luteum C. B.* The yellow poiſonous Wolfs-bane.

N°. 495. p. 403. Ann. 1749. 1352. *Aconitum Pyrenaicum, ampliore folio tenuius laciniato T.* 424. Pyrenean Wolfs-bane, with a larger leaf, divided into finer ſegments.

N°. 484. p. 597. Ann. 1745. 1151. *Acorus verus, ſive Calamus aromaticus.* Off. & *C. B.* The ſweet-ſmelling Flag or Calamus.

No. 491. p. 43. Ann. 1746. 1203. *Acriviola maxima, odorata, flore pleno Boerh.* The great double *Naſturtium*, or Indian Creſs.

N°. 495. p. 403. Ann. 1749. 1351. *Adhatoda minor Canarienſis Pluk.* The ſmaller Canary Adhatoda.

N°. 491. p. 43. Ann. 1746. 1201. *Adiantum Americanum Cornut.* Black Maiden-hair of America.

1202. *Adiantum nigrum Offic.* Common black Maiden-hair, *or* Oak-fern.

N°. 484. p. 597. Ann. 1745. 1153. *Adonis alias Eranthemum, J. B.* Adonis flower, or red Maithes.

1154. *Agrimonia Orientalis, ſpica brevi craſſa, &c. Tourn.* † Dwarf Eaſtern Agrimony, with thick creeping roots, and the fruit growing in ſhort thick ſpikes.

N°. 494. p. 359. Ann. 1748. 1303. *Agrimonoides* Column. *Ec.* 1. 145. Baſtard Agrimony.

N°. 491. p. 43. Ann. 1746. 1204. *Alaternoides Africana Telephii Imperati folio* Hort. *Amſt.* Baſtard Alaternus of *Africa*, with leaves like the *Telephium Imperati.*

N°. 472. p. 75. Ann. 1741. 953. *Alcea tenuifolia criſpa J. B.* Narrow curled leaved Vervain-Mallow.

† *Agrimonia Orientalis, humilis, radice craſſiſſima repente, fructu in ſpicam brevem & denſam congeſto* Tourn. *Cor.* 21.

N°. 494. p. 331. Ann. 1747. 1251. *Alchimilla Alpina Quinquefolii folio, ſubtus argenteo J. R. H.* 218. The Alpine five-leaved Ladies Mantle with the under part of the leaves white.

N°. 491. p. 43. Ann. 1746. 1205. *Alnus nigra Offic.* The black Alder-tree.

N°. 494. p. 331. Ann. 1747. 1252. *Alyſſoides incanum, foliis ſinuatis* Inſt. *R. H.* 218. Hoary *Alyſſoides* with ſinuated leaves.

1253. *Alyſſon Creticum, foliis angulatis, flore violaceo* T. *Cor.* 15. Candy Madwort, with angular leaves, and violet coloured flowers.

1254. *Alyſſon Creticum, Saxatile, foliis undulatis, incanis* T. *Cor.* 15. The *Alyſſon* of *Candia*, with hoary undulated leaves.

——— p. 359. Ann. 1748. 1304. *Alyſſon fruticoſum incanum Tourn.* hoary, ſhrub Madwort.

N°. 472. p. 75. Ann. 1741. 955. *Amaranthoides Lychnidis folio capitulis argenteis Tourn.* The white or ſilver coloured Globe Amaranthus, or Eternal flower.

956. *Idem capitulis purpureis.* The purple Globe-Amaranthus, or Eternal-flower.

954. *Amaranthus maximus* Offic. *purpureus major Park.* The tree Amaranth.

N°. 484. p. 597. Ann. 1745. 1155. *Amaranthus* Siculus *Spicatus* Boccone. *Boccone*'s perennial ſpiked Flower-gentle of Sicily.

N°. 474. p. 189. Ann. 1742. 1005. *Ammi majus* Off. *C. B.* 159. Common broad-leaved Biſhops Weed.

N°. 494. p. 359. Ann. 1748. 1301. *Ananthocyclus Coronopi folio Vaill.* Ananthocyclus with a Bucks-horn leaf.

N°. 474. p. 181. Ann. 1742. 1006. *Anchuſa purpurea Park.* Purple Alkanet.

N°. 472. p. 75. Ann. 1741. 957. *Aniſum* Offic. *herbariis C. B.* Aniſe.

N°. 474. p. 189. Ann. 1742. 1007. *Anonis non ſpinoſa, viſcida, hirſuta, odore Theriacæ Hort. Cathol.* Hairy, viſcous Reſt-harrow

harrow, without ſpines, and ſmelling like Venice Treacle.

N°. 494. p. 359. Ann. 1748. 1306. *Anonis purpurea, ſpicata, alopecuroides major Boerh.* The greater fox-tail, purple, ſpiked Reſt-harrow.

N°. 495. p. 403. Ann. 1749. 1354. *Apocynum majus Syriacum rectum Cornut.* 91. The greater upright Syrian Dogs-bane.

N°. 491. p. 43. Ann. 1746. 1306. *Aquilegia hortenſis multiplex, flore * pleno C. B.* Double Columbines.

N°. 476. p. 421. Ann. 1743. 1052. *Arbutus folio ſerrato C. B.* Strawberry-tree.

N°. 484. p. 597. Ann. 1745. 1156. *Ariſtolochia, Clematitis recta Off. & C. B.* Creeping Birthwort.

N°. 495. p. 403. Ann. 1749. 1354. *Ariſtolochia, Piſtolochia Cretica.* The ever-green Birthwort from *Crete.*

N°. 474. p. 189. Ann. 1742. 1008. *Arum Africanum flore albo Parad. Bat.* The African *Arum* with white flowers.

N°. 491. p. 43. Ann. 1746. 1207. *Arum maximum, quod* Colocaſia *vulgo caulibus nigricantibus* Hort. *Lugd.* The greateſt Egyptian *Arum* with blackiſh ſtalks.

N°. 476. p. 421. Ann. 1743. 1053. *Arum venis albis, lituris nigris maculatum* Hort. *R. Par.* The white-veined *Arum*, ſpotted with black.

N°. 494. p. 360. Ann. 1748. 1307. *Aſarina caule erecto ramoſo, foliis oblongis acutis ſeſſilibus, floribus erectis.* Upright *Aſarina* with long ſharp-pointed leaves.

p. 331. Ann. 1747. 1255. *Aſarum* Dod. *Pempt.* 358. *Offic.* 54. Aſarabacca.

N°. 472. p. 75. Ann. 1741. 859. *Aſparagus aculeatus ſpinis horridus C. B.* Prickly Sparagus or Sperage.

858. *Aſparagus ſylveſtris tenuiſſimo folio C. B.* Wild Sparagus or Sperage with narrow leaves.

N°. 491. p. 43. Ann. 1746. 1208. *Aſperula odorata, flore albo Offic. & Dodon.* Wood-roof.

* *Magno.*

N°. 484.

N°. 484. p. 597. Ann. 1745. 1157. *After* Atticus *cæruleus vulgaris C. B.* Purple *Italian* Starwort.

N°. 491. p. 43. Ann. 1746. 1209. *Asteriscus perennis maritimus patulus Tournef.* The maritime *perennial dwarf yellow Starwort.*

N°. 476. p. 421. Ann. 1743. 1054. *Asteroides* Alpina, *salicis folio Tourn.* Baſtard Starwort of the Alps, with a Willow leaf.

N°. 494. p. 360. Ann. 1748. 1308. *Astragalus annuus, anguſtis floribus, pediculis longis Tourn.* Annual Milk-Vetch, with narrow flowers, and long foot-ſtalks.

———p. 331. Ann. 1747. 1256. *Astragalus luteus annuus Monſpeliac. procumbens* Mor. *Hiſt.* Yellow annual trailing Milk-Vetch of *Montpelier.*

N°. 494. p. 360. Ann. 1748. 1310. *Astragalus luteus perennis procumbens vulgaris ſive ſylveſtris* *. Wild-Liquorice *or* Liquorice-Vetch.

N°. 480. p. 213. Ann. 1744. 1103. *Astragalus luteus perennis ſiliqua gemella rotunda Tourn.* Yellow perennial Milk-Vetch with a round pod.

N°. 494. p. 360. Ann. 1748. 1309. *Astragalus Orientalis altiſſ. Galegæ foliis* T. Tall Oriental Milk-Vetch with Goats-rue leaves.

N°. 480. p. 213. Ann. 1744. 1104. *Astragalus pumilus ſiliqua epiglottidis forma Tourn.* Dwarf Milk-Vetch with a pod ſhaped like the *epiglottis.*

1105. *Atriplicis marinæ ſpecies* Valerandi *J. Bauh.*

N°. 495. p. 403. Ann. 1740. 1356. *Baccharis Africana, Coronopi folio* Vaill. *Acad.* African Plowman's Spikenard with a Buckshorn leaf.

N°. 484. p. 597. Ann. 1745. 1158. *Balſamina fœmina C. B.* The female Balſam Apple.

N°. 474. p. 189. Ann. 1742. 1009. *Balſamita major* Dod. *Coſtus Hortorum Off.* The greater Coſtmar.

N°. 495. p. 403. Ann. 1749. 1357. *Barba Jovis Africana, foliis viridibus pinnatis; flore cæruleo Boerh.* The African Jupiter's Beard.

* Mor. *Hiſt.*

Beard with deep green leaves and blue flowers.

N°. 480. p. 213. Ann. 1744. 1106. *Barba Jovis Hiſpanica incana flore luteo Tourn.* Spaniſh Jupiter's Beard.

N°. 476. p. 421. Ann. 1743. 1055. *Barbarea* J. B. *flore ſimplici Park.* Winter Creſſes.

N°. 484. p. 597. Ann. 1745. 1159. *Bellis radice repente, latioribus ſerratis foliis Moriſon* *. Creeping-rooted Ox-eye Daiſie with broad ſerrated leaves.

N°. 472. p. 75. Ann. 1741. 961. *Bellis ſpinoſa foliis Agerati* C. B. *Santolina ſpinoſa Agerati foliis Tourn.* Prickly-leaved naked yellow Daiſie.

960. *Betonica Alpina incana purpurea* Barellier *Icon.* Hoary Alpine Betony with purple flowers.

N°. 474. p. 189. Ann. 1742. 1010. *Bidens folio tripartito, diviſo Tourn.* Water Hemp-Agrimony, with a yellow flower.

N°. 484. p. 597. Ann. 1745. 1160. *Bidens foliis non diſſectis Tourn.* Water Hemp-Agrimony with an undivided leaf.

No. 480. p. 213. Ann. 1744. 1107. *Biſtorta major, radice minus intorta C. B.* The greater Biſtort or Snake-weed.

N°. 495. p. 403. Ann. 1749. 1358. *Blattaria alba C. B.* 241. The white Moth-Mullein.

N°. 476. p. 421. Ann. 1743. 1056. *Blitum perenne :* Bonus Henricus *J. B. G.* Engliſh Mercury *or* All-good.

N°. 494. p. 331. Ann. 1747. 1257. *Borrago Conſtantinopolitana, flore reflexo cæruleo, calyce veſic. T. Cor.* Borrage of Conſtantinople, with a blue reflexed flower, and a ſwelling empalement.

——— p. 360. Ann. 1748. 1312. *Brunella folio laciniato C. B.* 261. Laciniated Self-heal with a ſmall white flower.

1311. *Brunella major, folio non diſſecto* C. B. 260. *Off.* 386. Common Self-heal.

* *Bellis major radice repente foliis latioribus ſerratis D. Moriſ. Prælud. Bot.* Hiſt. Ox. iii. 29.

N°. 494. p. 360. Ann. 1748. 1313. *Bugloſſum Creticum majus, flore cæruleo purpurante H. R. Par.* Greater Bugloſs of Candy with a blue flower inclining to a purple colour.

——— p. 331. Ann. 1747. 1358. *Bugloſſum Creticum verrucoſum perlatum quibuſdam H. R. Par.* Warted Bugloſs from Crete.

N°. 474. p. 189. Ann. 1742. 1011. *Bugloſſum latifolium ſempervirens C. B.* Ever-green Borrage.

1012. *Bugloſſum Orientale, flore luteo Tourn.* The Eaſtern Bugloſs with yellow flowers.

N°. 472. p. 75. Ann. 1741. 962. *Bugula Orientalis flore inverſo cæruleo Tourn.* * Hairy Eaſtern Bugle, with an inverted blue flower, ſpotted with white.

N°. 491. p. 43. Ann. 1746. 1210. *Buphthalmum Orientale, Tanaceti minoris folio, flore luteo amplo* † *Tourn.*

N°. 495. p. 403. Ann. 1749. 1359. *Bupleurum foliis linearibus acutis ſeſſilibus Fl. Leyd.* Hares-ear, with narrow ſharp-pointed ſeſſile leaves.

N°. 480. p. 213. Ann. 1744. 1108. *Burſa-Paſtoris major, folio ſinuato eleganti, inſtar Coronopi repentis C. B.* Greater Shepherds-purſe, with an elegant ſinuated leaf.

N°. 491. p. 43. Ann. 1746. 1211. *Calendula minor Hiſpanica* Hort. Lugd. *Bat.* Small Spaniſh Marygold.

N°. 480. p. 213. Ann. 1744. 1109. *Calendula* ‖ *polyanthos maxima C. B.* The largeſt double Marygold.

1111. *Calthoides, foliis oblongis, cæſiis, craſſis* Shaw *Specimen.* Calthoides with thick, bluiſh-green, oblong leaves.

N°. 474. p. 189. Ann. 1742. 1013. *Campanula hortenſis, folio & flore oblongo C. B.* 94. Coventry Bells.

* *Bugula Orientalis, villoſa, flore inverſo cæruleo, alba macula notato* Tourn. Cor. 14.
† *Ampliſſimo.* ‖ Caltha.

N°. 494. p. 331. Ann. 1747. 1259. *Campanula minor annua foliis incisis* Mor. *Hist.* 2. 458. Small annual cut-leaved Bell-flower, *or* Venus's Looking-glass.

——— p. 360. Ann. 1748. 1314. *Campanula minor, foliis incisis H. L. B.* Small cut-leaved Bell-flower.

N°. 474. p. 189. Ann. 1742. 1014. *Campanula Persicæ folio, flore albo pleno Lob.* The double Peach-leaved Bell-flower.

N°. 476. p. 421. Ann. 1743. 1057. *Canella alba* Off. *Cort.* Winteranus *vulgo* Winter's Bark.

1058. *Cardiaca flore canescente Amman.* Mother-wort with a hoary flower.

N°. 474. p. 190. Ann. 1742. 1015. *Carduus acaulis minor, flore purpureo C. B.* 380. * Dwarf Carline Thistle.

1016. *Carduus aculeatus, Ptarmicae Austriacae folio Triumfetti.* Prickly Thistle with leaves like the Austrian Sneezewort.

N°. 484. p. 507. Ann. 1745. 1161. *Carthamus Off. flore croceo Tourn.* Bastard Saffron *or Saf*-flower.

N°. 472. p. 75. Ann. 1741. 964. *Carthamus Africanus folio Ilicis, flore aureo Boerh.* Shrubby African Bastard Saffron with an ever-green Oak leaf, and a golden flower.

N°. 474. p. 190. Ann. 1742. 1018. *Carum Off.* Carawaies.

N°. 494. p. 331. Ann. 1747. 1260. *Caryophyllata montana, flore luteo, nutante H. R. Par.* Mountain Avens, with yellow nodding flowers.

N°. 480. p. 213. Ann. 1744. 1112. *Caryophyllus barbatus sylvestris C. B.* Deptford Pink.

N°. 484. p. 597. Ann. 1745. 1163. *Caryophyllus Sinensis flore vario* Rand. *Hort. Chelf.* Chinese Pink with a variable flower.

1162. *Catanance flore luteo latifolia* † *Tourn.* Broad-leaved Candy Lion's-foot with a yellow flower.

* There is no such name in Bauhin's *Pinax*: I suppose the *Carlina acaulos minore purpureo flore* of that author is the plant intended.

† *latiore folio.*

N°. 494.

No. 494. p. 360. Ann. 1748. 1315. *Cataria minor Alpina* T. *Inſt.* 202. The ſmaller mountain Catmint.

———p. 332. Ann. 1747. 1261. *Cataria, quæ Nepeta minor foliis Meliſſæ Turcicæ* Hort. *Cath.* Leſſer Catmint with leaves like Turkey Baum.

N°. 495. p. 403. Ann. 1749. 1360. *Centaurea calicibus ſetaceo-ſpinoſis, foliis lanceolatis petiolatis dentatis* Hort. *Cliff.* Great Centory with ſpear-pointed leaves.

N°. 480. p. 213. Ann. 1744. 1113. *Centaurium majus Alpinum luteum C. B.* Greater yellow Centory of the Alps.

1114. *Centaurium minus luteum perfoliatum J. B.* Yellow Centory.

1115. *Cerinthe major, flore verſicolore C. B.* * Great Mountain Honey-wort.

N°. 494. p. 360. Ann. 1748. 1316. *Cerinthe minor, flavo flore C. B.* 258. Yellow flowered Honey-wort.

N°. 484. p. 597. Ann. 1745. 1164. *Chamæceraſus Alpina fructu gemino rubro C. B.* Mountain upright Honey-ſuckle with red berries.

N°. 491. p. 43. Ann. 1746. 1212. *Chamæpitys lutea vulgaris, folio trifido C. B. & Offic.* Common Ground-pine.

N°. 494. p. 360. Ann. 1748. 1317. *Chenopodio-morus minor* Boer. *Ind.* 91. Smaller Bloody Spinach.

N°. 494. p. 332. Ann. 1747. 1262. *Chondrilla Sonchi foliis* †, *flore purpuraſcente major Tourn.* 475. Blue flowered Mountain Lettuce of the woods.

N°. 480. p. 213. Ann. 1744. 1110. *Chriſtophoriana vulgaris Park.* Herb *Chriſtopher* or Bane-berries.

N°. 484. p. 597. Ann. 1745. 1166. *Chryſanthemum Bermudenſe, Leucoii folio craſſo Pluk.* Corn Marygold of Bermudas.

* *Cerinthe quorundam major verſicolore flore* J. B.

† *folio.*

N°. 480. p. 214. Ann. 1744. 1116. *Chryſanthemum Creticum Ger. Park.* Candy Corn Marygold

1117. *Chryſanthemum flore pleno Tourn.* Corn Marygold with double flowers.

1118. *Chryſanthemum mixtum, flore pleno* Hort. *Eyſt.* Mixt Corn Marygold with a double flower.

N°. 484. p. 598. Ann. 1745. 1166. *Cirſium Anglicum Ger.* The Engliſh ſoft or gentle Thiſtle.

1167. *Ciſtus fœmina Salviæ folio C. B.* Female Rock-roſe with Sage leaves.

N°. 494. p. 360. Ann. 1748. 1318. *Ciſtus ladanifera Hiſpanica, flore albo macula punicante inſignito.** Spaniſh gum-bearing *Ciſtus*, or Rock-roſe, with Willow leaves, and white flowers ſpotted with purple.

N°. 484. p. 598. Ann. 1745. 1169. *Ciſtus ladanifera Hiſpanica Salicis folio C. B.* † Spaniſh gum-bearing *Ciſtus* or Rock-roſe, with Willow leaves and white flowers.

1168. *Ciſtus mas Luſitanicus folio ampliſſimo incano Tourn.* Male Portugal Rock-roſe, with an ample hoary leaf.

N°. 472. p. 75. Ann. 1741. 963. *Citrullus ſive Anguria vulgo Park.* Citruls.

N°. 491. p. 43. Ann. 1746. 1214. *Clinopodium Americanum rotundifolium, Pulegii odore Houſt.* Round-leaved American Field-Baſil, with a ſmell of Penny-royal.

1313. *Clinopodium minus Ocimi facie C. B.* § Wild Baſil.

N°. 494. p. 332. Ann. 1747. 1263. *Clutia foliis petiolatis* Linn. *Hort. Cliff.* Clutia with foot-ſtalks to the leaves.

* *Ciſtus Ladanifera, Hiſpanica, Salicis folio, flore albo, macula punicante inſignito Tourn.*

† *Ciſtus Ladanifera Hiſpanica incana* C. B.

§ *Clinopodium arvenſe Ocimi facie* C. B.

N°. 474. p. 190. Ann. 1742. 1017. *Cnicus Atractylis lutea dictus Off. Hort. Lugd. Bat.* The Diſtaff Thiſtle.

N°. 480. p. 214. Ann. 1744. 1119. *Colutea Æthiopica flore phœniceo, Barbæ Jovis folio Breyn.* Æthiopian Bladder-Senna with ſcarlet flowers and leaves like the Silver Buſh.

N°. 480. p. 214. Ann. 1744. 1120. *Coma aurea Africana fruticans, foliis Linariæ anguſtis* Hort. Amſt. African ſhrubby Goldilocks.

N°. 495. p. 403. Ann. 1749. 1361. *Commelina foliis ovato-lanceolatis; petalis tribus majoribus æqualibus Linn.* Commelina with oval lanceolated leaves, and the three greater petals equal.

N°. 494. p. 360. Ann. 1748. 1319. *Commelina radice Anacampſerotis* Hort. Elt. *Tab.* 79. Commelina with a root like Orpine.

N°. 495. p. 403. Ann. 1749. 1362. *Convolvulus argenteus elegantiſſimus; foliis tenuiter inciſis* Inſt. *R. H.* The moſt elegant Silver Bindweed with fine cut leaves.

N°. 491. p. 44. Ann. 1746. 1215. *Convolvulus cæruleus major, folio ſubrotundo Ger. Park.* The greater Bindweed with roundiſh leaves.

N°. 476. p. 421. Ann. 1743. 1059. *Convolvulus cæruleus minor Hiſpanicus Park.* Spaniſh, blue, leſſer Bindweed, with an oblong leaf.

N°. 495. p. 404. Ann. 1749. 1363. *Convolvulus peregrinus pulcher folio Betonicæ J. B.* Bindweed with Marſh-mallow leaves.

1364. *Convolvulus Siculus minor; flore parvo auriculato* Bocc. *Rar.* Small blue-flowered Sicilian Bindweed.

N°. 472. p. 76. Ann. 1741. 965. *Conyza humilior, Linariæ folio, floribus luteis umbellatis Amm.* 141. Low Flea-bane with Toad-flax leaves, and yellow umbellated flowers.

N°. 491. p. 44. Ann. 1746. 1216. *Conyza minima Ger. emac.* Small Fleabane.

N°. 494. p. 360. Ann. 1748. 1310. *Cornus foliis lanceolatis acutis ſubtus incanis, umbellis minoribus.* Cornel with ſharp ſpear-pointed leaves, hoary underneath, and ſmaller umbells.

N°. 491. p. 44. Ann. 1746. 1217. *Corona Solis annua, flore pleno Tourn.* The double annual Sun-flower.

1218. *Coronilla herbacea, flore vario Tourn.* Herbaceous jointed-podded *Colutea*, with a variable flower.

N°. 494. p. 360. Ann. 1748. 1311. *Coronilla leguminibus teretibus articulatis erectis Fl. Leyd.* 387. * *Candia* herbaceous jointed-podded *Colutea* with a ſmall purpliſh flower.

N°. 484. p. 598. Ann. 1745. 1170. *Cotyledon Africana fruteſcens flore coccineo umbellato, Commelin.* Shrubby African Navel-wort, with ſcarlet umbellated flowers.

N°. 480. p. 214. Ann. 1744. 1121. *Cuminoides Tourn.* Baſtard *or* Wild Cummin.

1122. *Cyanus minor, flore purpureo C. B.* Small Blue-bottle with a purple flower.

N°. 472. p. 76. Ann. 1741. 966. *Cyanus Turcicus, ſeu orientalis odoratus major Park.* The Sultan flower.

N°. 494. p. 360. Ann. 1748. 1322. *Cynogloſſum Creticum argenteo folio C. B.* † *Candia* Hounds-tongue with narrow ſilver coloured leaves.

N°. 476. p. 421. Ann. 1743. 1060. *Cyperus odoratus radice longa C. B.* The ordinary ſweet *Cyperus*, *or* Engliſh Galingale.

1061. *Cyperus rotundus eſculentus anguſtifolius Ibid.* Sweet *Cyperus*, *or* Ruſhmit.

N°. 484. p. 598. Ann. 1745. 1171. *Cytiſus* Alpinus *flore luteo racemoſo pendulo.* Bean-trefoil, with a yellow flower, hanging in bunches.

* *Coronilla legum teret. artic. erect.* caule herbaceo *Fl. Leyd.*

† *Cynogloſſum Creticum argenteo* anguſto *folio C. B.*

N°. 476.

N°. 476. p. 421. Ann. 1743. 1063. *Cytiſus glaber foliis ſubrotundis pediculis breviſſimis C. B.* Round-leaved ſmooth Baſe Shrub-Trefoil with ſhort foot-ſtalks.

1062. *Cytiſus incanus, ſiliquis falcatis Ibid.* Shrubby three leaved hoary Moon-trefoil.

N°. 472. p. 76. Ann. 1741. 967. *Dictamnus montis Sipyli. Sir Geo. Wheeler.* Dittany of Mount Sipylus.

N°. 480. p. 214. Ann. 1744. 1123. *Digitalis lutea magno flore C. B.* Fox-glove with a large yellow flower.

N°. 495. p. 404. Ann. 1749. 1365. *Digitalis lutea major ; parvo flore* Mor. *Hiſt.* Great yellow Fox-glove with a ſmall flower.

N°. 474. p. 190. Ann. 1742. 1019. *Doronicum Americanum Park.* American Leopard's-bane.

N°. 480. p. 214. Ann. 1744. 1124. *Dorycnium Monſpelienſium* Lob. *Icon.* Shrub-trefoil of Montpelier.

N°. 476. p. 421. Ann. 1743. 1064. *Draba ſiliquis donata C. B.* *

N°. 495. p. 404. Ann. 1749. 1366. *Dracocephalon Canarienſe triphyllon ; Cedronella* H. *Amſtel.* Trifoliated Dragon's-head of the Canary Iſlands.

1367. *Echium Creticum anguſtifolium rubrum C. B.* Narrow-leaved *Candia* Viper's Bugloſs, with a red flower.

N°. 472. p. 76. Ann. 1741. 968. *Elaterium* Tourn. *Cucumis aſininus Ger.* Wild Cucumber.

N°. 474. p. 190. Ann. 1742. 1021. *Elichryſum latifolium Americanum Tourn.* Broad-leaved American Eternal-flower.

1020. *Elichryſum, ſeu Stœchas citrina anguſtifolia C. B.* Goldilocks or Caſſidony.

N°. 484. p. 598. Ann. 1745. 1174. *Elichryſum Spicatum Tourn.* Spiked Eternal-flower.

N°. 495. p. 404. Ann. 1749. 1368. *Emerus Americanus, ſiliqua incurva* Inſt. *R. H.* † American Scorpion-Senna with a crooked pod.

* I do not find this name in the author here quoted.

† This name is not in Tournefort's *Inſtitutiones R. H.*

N°. 484. p. 598. Ann. 1748. 1172. *Emerus* Tourn. * *Colutea Scorpioides Park.* The leſſer Scorpion Senna.

1173. *Ephedra major maritima Tourn.* Greater Sea Horſe-tail.

N°. 494. p. 332. Ann. 1747. 1266. *Eryſimum Orientale, foliis Sonchi, flore ſulphureo, ſiliquis longiſſimis Boerh.* Oriental Hedge-muſtard, with Sow-thiſtle leaves, a pale yellow flower, and very long pods.

N°. 474. p. 190. Ann. 1742. 1022. *Eryſimum polyceratium vel corniculatum C. B.* 101. Hedge-muſtard with many crooked pods.

N°. 472. p. 76. Ann. 1741. 969. *Euonymo adfinis Æthiopica fructu globoſo, Salicis folio Plukn.* An Ethiopic plant approaching to the Spindle-tree, with a globoſe fruit, and Willow leaves.

N°. 494. p. 332. Ann. 1747. 1165. *Euonymus latifolius C. B.* 428. Broad-leaved Spindle-tree.

1164. *Euonymus vulgaris, granis rubentibus C. B.* 428. Spindle-tree or Prickwood.

N°. 491. p. 44. Ann. 1746. 1219. *Euphraſia pratenſis rubra C. B.* Red meadow Eye-bright, *or* Eye-bright Cow-wheat.

N°. 474 p. 190. Ann. 1742. 1023. *Fabago* Belgarum, *ſive Peplus* Pariſienſium *Lugdunenſ.* Bean-Caper.

N°. 476. p. 421. Ann. 1743. 1665. *Fagopyrum vulgare erectum Tourn.* Common upright Buck-wheat.

N°. 480. p. 214. Ann. 1744. 1125. *Filipendula omni parte major Boerhaave* †. Larger Drop-wort with a narrower leaf.

N°. 474. p. 190. Ann. 1742. 1324. *Filix mas aculeata, pinnulis auriculatis anguſtioribus Raii* ‖. Prickly Male-Fern with narrower leaves.

N°. 484. p. 598. Ann. 1745. 1176. *Fritillaria alba præcox C. B.* The early white Fritillary.

* *Emerus minor* Tourn.

† *Filipendula omni parte major, folio anguſtiori* Boer.

‖ *Filix aculeata major, pinnulis auriculatis crebrioribus foliis integris anguſtioribus* Raii Syn. Ed. 3. 121.

N°. 494. p. 332. Ann. 1747. 1267. *Fritillaria lutea, foliis Polygonati, fructu breviore* Boerh. 2. 139. Yellow Fritillary, with Solomon's-seal leaves, and a shorter fruit.

N°. 484. p. 598. Ann. 1745. 1175. *Fritillaria præcox purpurea variegata C. B.* Common Fritillary or chequer'd Lily.

N°. 491. p. 44. Ann. 1746. 1220. *Galega Africana, floribus majoribus, & siliquis crassioribus Tourn.* African Goat's-rue with large flowers and thick pods.

N°. 484. p. 598. Ann. 1745. 1177. *Galega vulgaris floribus cæruleis C. B.* Common Goat's-rue with blue flowers.

N°. 495. p. 404. Ann. 1749. 1369. *Gallium saxatile minimum supinum & pumilum* Inst. *R. H.* 115. The smallest supine dwarf Ladies Bedstraw.

N°. 474. p. 190. Ann. 1742. 1025. *Genista juncea* J. B. *Hispanica Ger.* The Yellow Spanish Broom.

1026. *Gentiana Alpina magno flore J. B.* Large flowered Gentian of the Alps, commonly called *Gentianella.*

N°. 484. p. 598. Ann. 1745. 1177. *Gentiana* Offic. *major lutea C. B.* The most common great Gentian *or* Felwort.

N°. 491. p. 44. Ann. 1746. 1221. *Geranium Africanum Malvæ folio, petalis florum inferioribus vix conspicuis* Index. Hort. *Chelf.* African Mallow-leaved Cranesbill, with the lower petals scarce discernible.

N°. 494. p. 332. Ann. 1747. 1268. *Geranium latifolium longissima acu C. B.* 319. Candy Cranes-bill.

N°. 480. p. 214. Ann. 1744. 1126. *Geum folio * rotundo majori, pistillo floris rubro Tourn.* London Pride *or* None-so-pretty.

1127. *Geum folio subrotundo minori* †. Sanicle with a lesser roundish leaf, and a red pointal.

* *Subrotundo.* † *Geum fol. subrot. min.* pistillo floris *rubro* Tourn.

N°. 474. p. 190. Ann. 1742. 1027. *Glaucium flore luteo Tourn.* Yellow horned Poppy.

N°. 476. p. 422. Ann. 1743. 1066. *Glaucium flore purpureo Tourn.* Purple horned Poppy.

N°. 495. p. 404. Ann. 1749. 1370. *Glaucium hirsutum flore phœniceo Tourn.* Hairy horned Poppy with a deep scarlet flower.

1371. *Glaucium Orientale flore magno aureo* * T. *Cor.* Eastern Horned Poppy with a large red flower.

N°. 484. p. 598. Ann. 1745. 1179. *Glychirrhiza sylvestris flore luteo pallido C. B.* † Wild Liquorice *or* Liquorice-Vetch.

N°. 474. p. 190. Ann. 1742. 1028. *Gramen Dactylon esculentum C. B. Manna vulg.* H. L. *Bat.* The Manna Grass.

N°. 472. p. 76. Ann. 1741. 970. *Guaicana Virginiana* Pishamin *dicta Park.* The Pishamin Plum.

N°. 491. p. 44. Ann. 1746. 1222. *Guidonia Ulmi folio flore roseo Plum.* Guidonia with Elm-leaves and a rose-coloured flower.

N°. 476. p. 422. Ann. 1743. 1067. *Hedysarum annuum siliquis asperis pendulis intortis Tourn.* ‖ Annual French Honeysuckle with a rough waved and wreathed pod.

1068. *Helenium* Off. *Enula campana Park.* Elecampane.

N°. 494. p. 360. Ann. 1748. 1323. *Helianthemum foliis Sampsuchi, capitulis valde hirsutis J. B.* Dwarf Cistus with a Marjoram leaf and very hairy heads.

——— p. 332. Ann. 1743. 1269. *Helianthemum Salicis folio T.* 249. Dwarf Cistus with a Willow leaf.

N°. 491. p. 44. Ann. 1746. 1223. *Helianthemum vulgare, flore luteo J. B.* Dwarf Cistus *or* little Sun-flower.

N°. 495. p. 404. Ann. 1749. 1372. *Helleborus Fumariæ foliis* Amman. *Ruth.* 74. Hellebore with Fumitory leaves.

* *rubro.* † *Glychirrhiza Sylv.* floribus luteo-pallescentibus *C. B.*
‖ *Hedysarum annuum, siliqua aspera, undulata, intorta* Tourn.

N°. 476. p. 422. Ann. 1743. 1069. *Helleborus niger, flore roſeo C. B. Off.* True black Hellebore *or* Chriſtmas flower.

N°. 474. p. 190. Ann. 1742. 1029. *Helleborus niger fœtidus C. B.* 126. Stinking black Hellebore, Bears-foot *or* Setterwort.

N°. 484. p. 598. Ann. 1745. 1180. *Helleborus niger, folio Ranunculi flore globoſo Tourn.* The Globe-flower *or* Locker-gowlons.

N°. 474. p. 190. Ann. 1742. 1030. *Hermannia, folio Lavendulæ obtuſo, flore parvo aureo Boerhaave.* * Shrubby Hermannia with a broad blunt Lavender leaf, and a ſmall golden flower.

N°. 494. p. 332. Ann. 1747. 1270. *Hermannia fruteſcens, folio oblongo ſerrato T.* 656. Shrubby Hermannia with an oblong ſerrated leaf.

1274. *Heſperis caule ramoſiſſimo; foliis lanceolatis ſæpius dentatis* Linn. *Hort. Cliff.* Dames-violet with a very branched ſtalk, and ſpear pointed, thickly indented leaves.

1272. *Heſperis exigua lutea; folio dentato anguſto Boerh.* 2. 20. Small yellow Dames-violet with a narrow indented leaf.

1275. *Heſperis flore albo minimo; ſiliqua longa; flore* † *profunde dentato* Boerh. *Ind.* Dames-violet with a very ſmall white flower, a long pod, and a deeply indented leaf.

1273. *Heſperis maritima, anguſtifolia, incana T.* 223. Dwarf annual ſtock.

1271. *Heſperis ſylveſtris inodora C. B.* 202. Unſavoury wild Dames-violet.

N°. 494. p. 360. Ann. 1748. 1324. *Hieracium amygdalas amaras olens flore ſuaverubente C. B.* 127. ‖

* *Hermannia fruteſcens; folio Lavendulæ latiori & obtuſo; flore parvo aureo H.R.D.* Boer. Ind. I. 273. † *folio.*

‖ I do not find this name in *C. B.* perhaps it ſhould be *Hieracium amygdalas amaras olens, ſeu odore apuli ſuaverubentis* H. R. Par.

N°. 495. p. 404. Ann. 1749. 1373. *Hieracium calyce barbato* Col. *Ec.* 2. 27. Hawkweed with a bearded empalement.

N°. 474. p. 190. Ann. 1742. 1031. *Hieracium montanum tomentoſum* Moriſon. *Hort. Reg. Bleſ.* Downy Mountain Hawkweed.

N°. 491. p. 44. Ann. 1746. 1224. *Horminum coma purpuro-violacea J. B.* Clary with a purple violet top.

N°. 476. p. 422. Ann. 1743. 1070. *Horminum luteum, glutinoſum C. B.* Yellow Clary *or* Jupiter's Diſtaff.

N°. 474. p. 190. Ann. 1742. 1032. *Horminum pratenſe, flore minimo* Schol. *Botan.* Meadow Clary with a very ſmall flower.

1033. *Horminum ſylveſtre, Lavendulæ flore C. B.* 219. Common Engliſh wild Clary.

N°. 476. p. 422. Ann. 1743. 1021. *Hydrophyllum* Morini *Tourn.* * Water-leaf of *Morinus*.

N°. 494. p. 360. Ann. 1748. 1325. *Hypecoum latiore folio T.* 230. Broad-leaved *Hypecoon*.

N°. 480. p. 214. Ann. 1744. 1128. *Hypericum orientale, Roriſmarini folio Amman.* Oriental Saint John's-wort with a Roſemary leaf.

N°. 494. p. 360. Ann. 1748. 1326. *Jacea cinerea laciniata, flore purpureo Triumfet.* Jagged aſh-coloured Knapweed with a purple flower.

N°. 472. p. 76. Ann. 1741. 971. *Jacea non ramoſa tuberoſa radice latifolia* Baniſter. *Pluk.* Broad-leaved unbranched Knapweed with a tuberous root.

972. *Eadem anguſtifolia &c.* The ſame with a narrow leaf.

N°. 491. p. 44. Ann. 1746. 1225. *Jacea ſpinoſa alata cinerea caule ſpinis luteis longiſſimis.* Aſh-coloured winged Knapweed with very long yellow prickles.

N°. 494. p. 360. Ann. 1748. 1327. *Jacea ſpinoſa Cretica Zanon.* Prickly Knapweed of Candy.

* *Hydrophyllon Morini* Hort. Joncq.

N°. 472. p. 76. Ann. 1741. 973. *Jacobæa Ætnica Chenopodii folio* Hort. *Cathol.* Ragwort of Ætna, with a Gooſe-foot leaf.

N°. 495. p. 404. Ann. 1749. 1374. *Jaſminum Africanum; ilicis folio; flore albo* Com. *Rar.* African Jaſmine with a Holm-oak leaf, and a white flower.

N°. 476. p. 422. Ann. 1743. 1072. *Jaſminum luteum Indicum odoratiſſimum Ferrar.* Yellow Indian Jaſmine.

N°. 484. p. 598. Ann. 1745. 1181. *Jaſminum ſive* Sumbach *Arabum* Alpini *J. B.* Single Arabian Jaſmine.

N°. 474. p. 190. Ann. 1742. 1034. *Ilex oblongo ſerrato folio C. B.* 234. Narrow-leaved Ever-green Oak with ſerrated leaves.

N°. 480. p. 214. Ann. 1744. 1129. *Imperatoria major C. B.* Common Maſter-wort.

N°. 474. p. 190. Ann. 1742. 1035. *Ketmia Syrorum flore albo* Boerhaave. *Althæa frutex* with white flowers.

N°. 484. p. 598. Ann. 1745. 1182. *Lamium rubrum minus foliis profunde inciſis* Raii *Syn.* Leſſer red Dead-Nettle with leaves deeply cut.

N°. 494. p. 361. Ann. 1748. 1328. *Lathyrus tuberoſus arvenſis repens C. B.* Peaſe-Earthnut.

N°. 491. p. 44. Ann. 1746. 1226. *Lavatera folio & facie Althææ* Act. *Reg. Sc.* Lavatera with the leaf and face of Marſh-mallow.

1227. *Lavatera flore albo.* Lavatera with a white flower.

N°. 476. p. 422. Ann. 1743. 1073. *Laurus Alexandrina Off.* Alexandrian Laurel.

N°. 474. p. 191. Ann. 1742. 1036. *Lentiſcus vulgaris C. B.* 399. Common Maſtich-tree.

N°. 472. p. 76. Ann. 1741. 974. *Leonurus Africanus, Sideritidis folio, floribus phœniceis Boer.* * Perenniai African Lion's-tail with an Ironwort leaf, and a large ſcarlet flower.

N°. 484. p. 598. Ann. 1745. 1183. *Lepidium latifolium* Off. & *C. B.* Dittander *or* Pepper-wort.

* *Leonurus perennis Africanus, Sideritidis folio; flore phœniceo majore* Breyn. Prod.

N°. 494. p. 332. Ann. 1747. 1278. *Leucoium Hesperidis folio T.* 221. Great Tower-Mustard.

——— p. 361. Ann. 1748. 1329. *Lilium convallium, flore pleno variegato* Didac *T.* 77. Broad-leaved Lily of the valley, with a double striped flower.

N°. 472. p. 76. Ann. 1741. 976. *Limonium lignosum Bellidis folio C. B.* *

975. *Limonium maritimum majus C. B.* Sea Lavender.

N°. 476. p. 422. Ann. 1743. 1074. *Linaria latifolia Dalmatica C. B.* † Broad-leaved Dalmatian Toad-flax, with a large flower.

N°. 495. p. 404. Ann. 1749. 1375. *Linaria pumila foliis carnosis, flosculis minimis flavis C. B.* 213. Dwarf Toad-flax with fleshy leaves and very small yellow flowers.

N°. 494. p. 361. Ann. 1748. 1330. *Linaria triphylla minor lutea, floris vexillo & calcari purpureo Boer.* Small three-leaved yellow Toad-flax, with the standard and heel of a purple colour.

N°. 474. p. 191. Ann. 1742. 1037. *Lingua cervina multifida C. B.* 354. Harts-tongue with divided leaves.

N°. 491. p. 44. Ann. 1746. 1228. *Lotus angustifolia flore luteo-purpureo ex insula Sancti Jacobi* Hort. *Amst.* Narrow-leaved Birds-foot Trefoil from the island of St James, with a purple yellow flower.

N°. 474. p. 191. Ann. 1742. 1038. *Lotus hæmorrhoidalis major Park.* Upright hoary Birds-foot Trefoil.

N°. 484. p. 598. Ann. 1745. 1184. *Lotus rubra siliqua angulosa C. B.* Square-codded Vetch.

N°. 480. p. 214. Ann. 1744. 1130. *Lotus siliquis Ornithopodii J. B.* Birds-foot-Trefoil with pods like a Birds-foot.

N°. 472. p. 76. Ann. 1741. 977. *Lupinaster floribus purpureis siliquis minoribus Amm. p.* 147. Lupinaster with purple flowers and smaller pods.

* This name is not in *C. B.*

† This is Parkinson's name: *C. B.* adds *magno flore.*

N°. 476. p. 422. Ann. 1743. 1075. *Lupinus anguſtifoliis ſylveſtris cæruleus elatior* Hort. *Eyſt.* Narrow-leaved tall blue wild Lupine.

N°. 474. p. 191. Ann. 1742. 1041. *Lupinus peregrinus major villoſus cæruleus C. B.** Great blue Lupine.

1039. *Lupinus ſativus flore albo Ibid.* White Lupines.

1040 *Lupinus ſylveſtris flore luteo Ibid.* The yellow Lupine.

1042. *Lychnis Chalcedonica flore miniato Park.* Noneſuch *or* flower of Conſtantinople.

N°. 476. p. 422. Ann. 1743. 1076. *Lychnis coronaria vulgaris J. B.* Garden Campions *or* Roſe Campion.

N°. 494. p. 332. Ann. 1747. 1276. *Lychnis Cretica parvo flore; calyce ſtriato purpuraſcente* T. *Cor.* 24. Lychnis of Candy, with a ſmall flower, and a purpliſh ſtriated empalement.

N°. 480. p. 214. Ann. 1744. 1131. *Lychnis hirſuta, flore eleganter variegato* Raii *Hiſt.* Hairy Campion with a flower beautifully variegated.

N°. 494. p. 332. Ann. 1747. 1277. *Lychnis ſupina Sicula, calyce ampliſſimo ſtriato T.* 337. Low Sicilian Campion, with a large ſtriated empalement.

N°. 491. p. 44. Ann. 1746. 1229. *Lychnis ſylveſtris flore albo minimo* Raii Hiſt. Small Corn Campion with a very ſmall white flower.

1230. *Lychnis ſylv. quæ Saponaria Tourn.* Common Sopewort.

N°. 476. p. 422. Ann. 1743. 1077. *Lychnis viſcoſa rubra anguſtifolia C. B.* Red German Catchfly.

N°. 484. p. 598. Ann. 1745. 1185. *Lyſimachia lutea major quæ* Dioſcoridis *Off.* & *C. B.* Yellow Willow-herb *or* Looſe-ſtrife.

N°. 494. p. 361. Ann. 1748. 1331. *Malva Alexandrina Alchimillæ folio Sherard.* Alexandrian Mallow with a Ladies-Mantle leaf.

* *Lupinus peregrinus major vel villoſus cæruleus major C. B.*

N°. 480.

N°. 480. p. 214. Ann. 1744. 1132. *Malva ſylv. minor, folio rotundo C. B.* Small wild Mallow or Dwarf Mallow.

N°. 494. p. 361. Ann. 1748. 1332. *Malva-viſcus arboreſcens, flore miniato clauſo* Hort. *Elth.* Tree Malva-viſcus, with a ſhut ſcarlet flower.

1333. *Marrubiaſtrum limbo atro-purpureo &c.* * Baſtard Horehound with a yellow flower, edged with a dark purple.

N°. 472. p. 76. Ann. 1741. 978. *Marum vulgare* Park. *vulgo Maſtichen redolens C. B.* Herb-Maſtick or Maſtick Thyme.

N°. 478. p. 422. Ann. 1743. 1078. *Matricaria foliis florum fiſtuloſis Hort. R. Par.* Feverfew with fiſtulous flowers.

N°. 484. p. 578. Ann. 1745. 1186. *Medica orbiculata J. B.* Orbiculated Snail-Trefoil.

N°. 495. p. 404. Ann. 1749. 1376. *Melilotus Italica folliculis rotundis C. B. P.* 331. Italian Melilot.

N°. 474. p. 191. Ann. 1742. 1043. *Melilotus major candida Tragi.* White flowered Melilot.

1044. *Melilotus odorata violacea* Hiſt. Oxon. *Lotus urbana Off.* Sweet Trefoil.

N°. 484. p. 598. Ann. 1745. 1187. *Melilotus Offic. &. C. B.* Common Melilot.

N°. 476. p. 422. Ann. 1743. 1079. *Meliſſa Moldavia flore albo Park.* Turkey Baulm with a white flower.

N°. 495. p. 404. Ann. 1749. 1377. *Meliſſa Romana molliter hirſuta & graveolens* H. R. *Par.* Stinking Roman Baulm, with ſofter hairy leaves.

N°. 472. p. 76. Ann. 1741. 979. *Mentha verticillata Ocimi odore, venis luteis* Ind. Hort. Chelſ. Whirled Mint, with a Baſil ſmell, and yellow veins.

N°. 491. p. 44. Ann. 1746. 1231. *Meſpilus aculeata, Pyri denticulato folio, ſplendens Virginiana Pluk.* The Virginian Azarol with red fruit.

* *Marrubiaſtrum Sideritidis folio, caliculis aculeatis, flore flavo cum limbo atropurpureo, coma flaveſcente T. Cor.* 12.

N°. 494. p. 332. Ann. 1747. 1279. *Meſpilus Canadenſis, Sorbi terminalis facie T.* 642. Medlar of Canada, with the appearance of the common Service-tree.

——— p. 332. Ann. 1748. 1334. *Meſpilus Virginiana, Apii folio, vulgari ſimilis, major.* Medlar of Virginia, reſembling the common Haw-thorn, but larger.

N°. 472. p. 76. Ann. 1741. 980. *Meum foliis Anethi C. B. — vulgatius Park.* Common Spignel *or* Meu.

N°. 495. p. 404. Ann. 1749. 1378. *Milleria annua erecta, foliis conjugatis, floribus luteis ſpicatis Houſt.* Greater upright annual Milleria with oppoſite leaves, and yellow ſpiked flowers.

N°. 472. p. 76. Ann. 1741. 921. *Mimoſa folio lato Sennæ ſpinoſa Boerh.* The prickly ſenſitive plant, with a broad Senna leaf.

N°. 484. p. 598. Ann. 1745. 1188. *Mirabilis Peruana flore variegato* Park. *Parad.* Marvel of Peru with a variegated flower.

N°. 474. p. 191. Ann. 1742. 1045. *Moldavica Americana trifolia, odore gravi Tourn.* Trifoliated American Moldavica with a ſtrong ſmell.

N°. 480. p. 214. Ann. 1744. 1133. *Moldavica orientalis, Salicis folio, flore parvo cæruleo Tourn.* Oriental Moldavica with a Willow leaf, and a ſmall blue flower.

N°. 494. p. 361. Ann. 1748. 1335. *Mollugo foliis verticillatis, cuneiformibus acutis* Hort. *Upſal.* Baſtard-Madder with ſharp wedge ſhaped whorled leaves.

N°. 495. p. 404. Ann. 1749. 1379. *Monarda floribus capitatis, caule obtuſo* Hort. *Cliff.* The Ozweega Tea.

N°. 491. p. 44. Ann. 1746. 1232. *Myagrum ſiliculis longis C. B.* * Treacle Wormſeed.

N°. 494. p. 361. Ann. 1748. 1336. *Myagrum ſiliculis obverſe ovatis, lateribus depreſſis* Fl. *Lugd.* † Gold of pleaſure.

* *Myagrum ſiliqua longa* C. B.

† Linn. H. Cliff. 328.

N°. 495. p. 404. Ann. 1749. 1380. *Myagrum ſiliculis ſulcatis, rugoſis foliis obtuſis dentatis* H. *Upſal.* Gold of pleaſure with wrinkled pods, and obtuſe indented leaves.

N°. 494. p. 361. Ann. 1748. 1337. *Myrrhis lutea ducoides* Mor. *H. R. Bleſ.* Yellow, carrot-like Cicely.

——— p. 332. Ann. 1747. 1280. *Myrrhis major, vel Cicutaria odorata C. B.* 160. *Officin.* 321. Sweet Cicely *or* great ſweet Chervil, *by ſome* Sweet-Fern.

N°. 476. p. 422. Ann. 1743. 1080. *Myrto-ciſtus* Pennei *Cluſ.* Myrtle Rock-roſe.

N°. 491. p. 44. Ann. 1746. 1234. *Myrtus Buxi folio* Schyl. *Hort.* Box-leaved Myrtle.

N°. 472. p. 76. Ann. 1741. 984. *Myrtus flore pleno Cornuti.* The double-flowered Myrtle.

982. *Myrtus foliis odore Nucis moſchatæ* Schyl. *Cat.* The Nutmeg Myrtle.

983. *Myrtus latifolia Bætica, foliis confertim naſcentibus C. B.* The Orange-leaved Myrtle.

N°. 491. p. 44. Ann. 1746. 1233. *Myrtus latifolia Romana C. B.* Common broad-leaved Myrtle.

N°. 476. p. 422. Ann. 1743. 1081. *Napus dulcis ſativus Off.* Navew gentle.

N°. 495. p. 404. Ann. 1749. 1381. *Nardus Americana procerior; foliis cæſiis* Pluk. *Alm.* Tall American Nard with blueiſh leaves.

N°. 472. p. 76. Ann. 1741. 985. *Nigella Cretica ſemine aromatico C. B.* Candy Fennel-flower with an aromatic ſeed.

N°. 480. p. 214. Ann. 1744. 1134. *Niſſolia Tourn.* Crimſon Graſs-Vetch.

N°. 495. p. 405. Ann. 1749. 1383. *Obeliſcotheca Hydrophylli foliis, lobis anguſtioribus Vaill.* Dwarf Sun-flower with Water-leaf leaves, and narrower lobes.

1382. *Obeliſcotheca Hydrophilli foliis, lobis latioribus Vaill.* Dwarf Sun-flower, with Water-leaf leaves, and broader lobes.

N°. 480. p. 214. Ann. 1744. 1135. *Obeliſcotheca integrifolia, radio aureo, umbone atro-rubente* Hort. *Eltham.* Dwarf Sun-flower, with entire leaves, a golden ray, and a dark red diſk.

N°. 491. p. 44. Ann. 1746. 1235. *Obeliſcotheca minor integro folio Dillen.* Smaller Dwarf Sun-flower, with an entire leaf.

N°. 474. p. 191. Ann. 1742. 1047. *Ochrus folio integro capreolas emittente* C. B. 243. *Ochrus* or winged Pea, with an entire leaf, ſending forth tendrils.

N°. 484. p. 599. Ann. 1745. 1190. *Oenanthe Apii folio C. B.* Water Dropwort with Smallage leaves.

1189. *Oenanthe Staphilini folio aliquatenus accedens J. B.* Dropwort with Carrot leaves.

N°. 476. p. 422. Ann. 1743. 1084. *Oenanthe Stellata* Cretica P. Alpini *Park.* Starry Dropwort of Candy.

N°. 472. p. 77. Ann. 1741. 987. *Olea maxima Hiſpanica C. B.* * The Spaniſh Olive.

988. *Olea minor Lucenſis, fructus odorato Ibid.* † The Luca Olive.

——— p. 76. ——— 986. *Olea vulgaris ſativa C. B.* The manured Olive.

N°. 494. p. 333. Ann. 1747. 1281. *Omphalodes Luſitanica Lini folio T.* 140. Venus Navel-wort.

N°. 472. p. 77. Ann. 1741. 987. *Onagra fruteſcens argentea anguſtifolia* Ind. *Hort. Chelſ.* Narrow-leaved ſilver ſhrubby Tree-Primroſe.

N°. 476. p. 422. Ann. 1743. 1082. *Onagra latifolia Tourn.* Broad-leaved Tree-Primroſe.

N°. 491. p. 44. Ann. 1746. 1236. *Ophioglo ſſum, Lingua Serpentina Park.* Adder's-tongue.

1237. *Ophris bifolia C. B.* Common Tway-blade.

N°. 476. p. 422. Ann. 1743. 1083. *Origanum Heracleoticum, Cunila gallinacea* Plinii *C. B.* Winter Sweet Marjoram.

N°. 474. p. 191. Ann. 1742. 1046. *Origanum* Off. *Origanum Anglicum Ger.* Wild Marjoram.

* C. B. makes a query what this is; *ſed quid ſunt Olivæ maximæ Hiſpanicæ?* Tournefort calls it *Olea fructu maximo, Olive d'Eſpagne.*

† This is not the name of *C. B.* but of Tournefort.

N°. 494. p. 361. Ann. 1748. 1338. *Ornithopodium portulacæ folio T.* 400. Birds-foot with a purſlane leaf.

N°. 491. p. 44. Ann. 1746. 1238. *Orobus ſylvaticus Viciæ foliis C. B.* Wood *Orobus* with Vetch leaves.

N°. 474. p. 191. Ann. 1742. 1048. *Orobus vulgaris herbariorum Ger. Park.* The common bitter Vetch.

N°. 495. p. 405. Ann. 1749. 1384. *Oxys lutea J. B.* 388. Wood-Sorrel with a yellow flower.

N°. 494. p. 333. Ann. 1747. 1282. *Padus Theophraſti Dalechampio Lugd.* 312. The wild Cluſter-Cherry, *or* Birds Cherry.

N°. 491. p. 45. Ann. 1746. 1239. *Pæonia mas* Officin. & *C. B.* The Male Peiony.

N°. 474. p. 191. Ann. 1742. 1049. *Panicum Indicum ſpica longiſſima C. B.* 343. Indian Panick with a very long ſpike.

N°. 484. p. 599. Ann. 1745. 1191. *Papaver hortenſe ſemine nigro ſylveſt.* Dioſcoridis *C. B.* Garden Poppy with black ſeeds.

N°. 495. p. 405. Ann. 1749. 1385. *Partheniaſtrum Helenii folio* Hort. Elth. *Partheniaſtrum* with an Elecampane leaf.

N°. 494. p. 333. Ann. 1747. 1283. *Pavia* Boerh. Ind. Alt. 2. 260. Scarlet-flowering Horſe-Cheſtnut.

N°. 476. p. 422. Ann. 1743. 1085. *Periploca foliis oblongis Tourn.* Periploca with oblong leaves.

N°. 480. p. 214. Ann. 1744. 1136. *Phalaris major ſemine albo* C. B. Canary-graſs.

1137. *Phalaris major ſpica longiori Ibid.* Canary-graſs with a longer ſpike.

N°. 491. p. 45. Ann. 1746. 1240. *Phaſeolus flore coccineo Cornut.* The ſcarlet Kidney-bean.

N°. 472. p. 77. Ann. 1741. 990. *Piſum cortice eduli Tourn.* Pea with an eſculent huſk.

N°. 491. p. 45. Ann. 1746. 1241. *Piſum humile caule firmo Tourn.* Dwarf Pea.

N°. 476. p. 422. Ann. 1743. 1086. *Plantago major incana Park.* Hoary Plantain *or* Lamb's tongue.

N°. 491. p. 45. Ann. 1746. 1242. *Polemonium vulgare Tourn.* Greek Valerian, Ladder to Heaven, *or* Jacob's Ladder.

N°. 494. p. 361. Ann. 1748. 1339. *Polygonatum caule purpuraſcente* Raii *Syn.* 2. 148. Solomon's Seal with a purpliſh ſtalk.

N°. 476. p. 422. Ann. 1743. 1087. *Polygonatum floribus ex ſingulis pediculis J. B.* Broad-leaved Solomon's Seal, with a large ſweet flower.

N°. 494. p. 333. Ann. 1747. 1284. *Polygonatum latifolium, Hellebori albi foliis C. B.* Broad-leaved Solomon's Seal with leaves like the white Hellebore.

N°. 494. p. 361. Ann. 1748. 1340. *Primula veris flore rubro Ger.* Red Primroſe.

1349. *Pſeudo-acacia T.* 649. Baſtard Acacia.

N°. 472. p. 77. Ann. 1741. 991. *Pſeudo-dictamnus acetabulis Moluccæ C. B.* Baſtard Dittany with Molucca Baulm leaves.

992. *Ptarmica flore pleno.* Double Sneezewort.

N°. 494. p. 333. Ann. 1747. 1285. *Pulmonaria maxima, foliis quaſi ſaccharo incruſtatis Pluknet.* Greateſt Lungwort with leaves very much ſpotted.

1286. *Pulſatilla folio craſſiore, & majore flore C. B.* 177. The greater *or* Daniſh Paſque-flower.

N°. 474. p. 191. Ann. 1742. 1050. *Punica malus* Off. *Malus Granata Ger.* The Pomegranate-tree.

N°. 480. p. 215. Ann. 1744. 1138. *Ranunculus aconiti folio, flore albo multiplici C. B.* Crowfoot with a Monks-hood leaf, and a double white flower, *commonly called* the fair maid of France.

N°. 476. p. 422. Ann. 1743. 1090. *Ranunculus echinatus Creticus C. B.* * Starry Hedge-hog Candy Crowfoot.

N°. 494. p. 333. Ann. 1747. 1287. *Ranunculus montanus, foliis Plantaginis C. B.* 180. Mountain Crowfoot with a Plantain leaf.

N°. 480. p. 215. Ann. 1744. 1139. *Ranunculus pratenſis, erectus, acris, flore pleno C. B.* Upright meadow Crowfoot with a double flower.

* *Ranunculus ſtellatus echinatus Creticus* C. B.

N°. 494 p. 361. Ann. 1748. 1342. *Rapiſtrum* * *maximum Cornuti* 147. The greateſt Charlock.

N°. 476. p. 422. Ann. 1743. 1088. *Rapunculus montanus corniculatus Ger.* Horned Rampion with a round ſpike.

N°. 491. p. 45. Ann. 1746. 1243. *Rapuntium maximum coccineo ſpicato flore* Col. *in Rech.* Greater Rampion, with a crimſon ſpiked flower, *commonly called* the ſcarlet Cardinal's flower.

N°. 484. p. 599. Ann. 1745. 1292. *Rawolfia tetraphylla latifolia Plumier.* Four-leaved Rawolfia with broad leaves.

1293. *Reſeda calcitrapæ folio Moriſon.* † Great white baſtard Rocket.

N°. 494. p. 361. Ann. 1748. 1343. *Reſeda foliis inferioribus integris, ſuperioribus laciniatis.* Baſtard Rocket with the lower leaves entire, and the upper jagged.

N°. 491. p. 45. Ann. 1746. 1244. *Rhamnus catharticus* Off. & *C. B.* Buck-thorn *or* common Purging-Thorn.

N°. 480. p. 215. Ann. 1744. 1140. *Rhus Virginiana, ſparſa panicula, ramis patulis glabris* Hort. *Elth.* Virginian Sumach, with a ſparſed panicle, and ſmooth ſpreading branches.

N°. 472. p. 77. Ann. 1741. 993. *Ricinus humilis, folio ſubrotundo, flore fructuque conglomerato Houſt.* ‖ Dwarf Oil-ſeed with roundiſh ſerrated leaves, ſilvered underneath, and the flower and fruit growing in bunches.

N°. 484. p. 599. Ann. 1745. 1194. *Roſa ſylveſtris pomifera noſtras* Raii *Syn.* § The greater Engliſh Apple-Roſe.

N°. 472. p. 77. Ann. 1741. 994. *Rubia procumbens hexaphylla purpurea H. L. Bat.* Procumbent ſix-leaved purple Madder.

N°. 476. p. 422. Ann. 1743. 1089. *Rubia tinctorum ſativa Off.* Madder.

* *Rapiſtrum maximum, rotundifolium, monoſpermon* Cornuti.

† *Reſeda folio Calcitrapæ flore albo* Mor. H. R Bleſ.

‖ *Ricinus humilis, foliis ſubrotundis, ſerratis & ſubtus argenteis, flore fructuque conglomeratis* Houſt. *Miller. Dict.*

§ *Roſa ſylveſtris pomifera major noſtras* Raii Syn. 11. 297.

N°. 484.

N°. 484. p. 599. Ann. 1745. 1195. *Salvia major, an Sphacelus* Dioſcoridis * *C. B.* The greater *or* common Sage.

1196. *Salvia minor aurita & non aurita C. B.* Sage of Virtue.

N°. 494. p. 361. Ann. 1748. 1344. *Sambucus folio laciniato C. B.*456. The cut *or* Parſley-leaved Elder.

N°. 494. p. 361. Ann. 1748. 1345. *Sambucus humilis, ſive Ebulus* C.B. *Off.* 180. Dwarf-Elder, Wallwort *or* Danewort.

N°. 491. p. 45. Ann. 1746. 1245. *Satureia hortenſis æſtiva C. B.* Summer Savoury.

1247. *Saurucus humilis, folio carnoſo rotundo Plum.* Dwarf Lizard's-tail with a round fleſhy leaf.

N°. 480. p. 215. Ann. 1744. 1141. *Saxifraga pratenſis flore pleno.* Meadow Saxifrage with a double flower.

N°. 484. p. 599. Ann. 1745. 1197. *Scabioſa arborea Cretica Ponæ.* Shrubby Scabious of Candy.

N°. 480. p. 215. Ann. 1744. 1143. *Scabioſa peregrina, capite oblongo nigricante, Zibethi odore C. B.*†

N°. 476. p. 422. Ann. 1743. 1091. *Sclarea Off.* Common Garden Clary.

N°. 491. p. 45. Ann. 1746. 1246. *Sclarea Indica flore variegato Boerh.* Indian Clary with a variegated leaf.

N°. 495. p. 405. Ann. 1749. 1386. *Scrophularia foliis Filicis modo laciniatis, vel Ruta canina latifolia* ‖. Broad-leaved Dog's-Rue.

N°. 494. p. 333. Ann. 1747. 1288. *Scrophularia Hiſpanica Sambuci folio glabro T.* 166. Spaniſh Figwort with a ſmooth Elder leaf.

1289. *Scrophularia maxima Luſitanica, Sambuci folio lanuginoſo T.* 166. Greateſt Portugal Figwort with a woolly Elder leaf.

1290. *Scrophularia peregrina* Cam. Hort. *Tab.* 43. Camerarius's foreign Figwort.

* Theophraſti. † This name is not in *C. B.* ‖ C. B.

N°. 495. p. 405. Ann. 1749. 1386. *Scrophularia Ruta canina dicta, vulgaris C. B.* 236. Dog's-rue.

N°. 494. p. 361. Ann. 1748. 1346. *Scutellaria foliis cordato-lanceolatis ſerratis, pedunculis multifloris.* * Scull-cap with heart-ſhaped ſerrated leaves, and many flowers upon each footſtalk.

N°. 472. p. 77. Ann. 1741. 995. *Serpyllum odoratiſſimum glabrum, longiore folio Amm. p.* 52. Smooth ſweet-ſcented wild Thyme with a longer leaf.

N°. 494. p. 361. Ann. 1748. 1347. *Sherardia* Dillenii *Cat. Giſſ. p.* 96. Little Field Madder.

N°. 495. p. 405. Ann. 1749. 1388. *Sideritis orientalis, Phlomidis folio T. Cor.* Eaſtern Ironwort with a *Phlomis* leaf.

N°. 480. p. 215. Ann. 1744. 1144. *Siliqua dulcis* C. B. † & *Off.* The Carob-tree.

N°. 495. p. 405. Ann. 1749. 1389. *Sinapiſtrum Luſitanicum, triphyllum, flore rubro, ſiliquis corniculatis.* ‖ Three-leaved Portugal *Sinapiſtrum* with a horned pod.

N°. 494. p. 333. Ann. 1747. 1291. *Syſymbrium Orientale, facie Barbareæ, foliis Plantaginis T. Cor.* 16. Oriental Water-creſs with Plantain leaves.

N°. 480. p. 215. Ann. 1744. 1142. *Smilax humillima unifolia Tourn.* The loweſt Bindweed *or* One-Blade.

N°. 494. p. 333. Ann. 1747. 1292. *Smyrnium* Marth. 773. *Officin.* 457. Alexanders.

1293. *Smyrnium peregrinum folio oblongo C. B.* 154. Foreign Alexanders with an oblong leaf.

——— p. 361. Ann. 1748. 1348. *Sonchus maritimus anguſtifolius C. B. P.* Narrow-leaved Sea-Sowthiſtle.

——— p. 333. Ann. 1747. 1294. *Sorbus ſativa* C. B. 415. *Officin.* 464. The true Service-tree *or* Sorb.

N°. 491. p. 45. Ann. 1746. 1248. *Spiræa Hiſpanica, Hyperici crenato folio Tourn.* Spaniſh Spiræa with a notched leaf.

* Flor. Leyd. † *Siliqua edulis* C. B. ‖ H. L.

N°. 480. p. 215. Ann. 1744. 1145. *Spiræa Hyperici folio* Tourn. *Hypericum frutex.*

1146. *Spiræa Opuli folio Tourn.* Virginian Gelder-Rose with a Currant leaf.

1147. *Spiræa salicis folio* Tourn. *Spiræa frutex.*

N°. 495. p. 405. Ann. 1749. 1390. *Stachys alba Michel.* White Base Horehound.

N°. 476. p. 423. Ann. 1743. 1092. *Stachys Canariensis fruteſcens, Verbasci folio Tourn.* Canary Shrubby Base Horehound, with a Mullein leaf.

N°. 495. p. 405. Ann. 1749. 1391. *Staphylodendron Virginianum triphyllum T.* 616. Three-leaved Virginian Bladder Nut.

N°. 484. p. 599. Ann. 1745. 1198. *Statice foliis anguſtioribus, flore rubro Tourn.* * Narrow-leaved Thrift with red flowers.

N°. 495. p. 405. Ann. 1749. 1392. *Statice Luſitanica Scorzoneræ folio* Inſt. *R. H.* 341. Portugal Thrift with a Scorzonera leaf.

N°. 472. p. 77. Ann. 1741. 996. *Stœchas citrina anguſtifolia C. B.* Narrow-leaved Goldylocks *or* Caſſidony.

N°. 484. p. 599. Ann. 1745. 1199. *Stœchas purpurea* Off. & *C. B.* Caſſidony *or* French Lavender, *by ſome* Sticadore.

N°. 494. p. 333. Ann. 1747. 1295. *Symphytum majus, tuberoſo radice C. B.* 476. Greater Comfrey with a tuberous root.

N°. 476. p. 423. Ann. 1743. 1095. *Tacamahac foliis ſerratis* Pluk. *Phyt.* 228. *Fig.* 2. Tacamahaca with ſerrated leaves.

N°. 480. p. 215. Ann. 1744. 1148. *Tagetes minor, flore ſimplici ſtriato* Tourn. †

N°. 476. p. 423. Ann. 1743. 1093. *Tamariſcus latiore folio* Park. *Germanicus Tourn.* ‖ The German Tamariſk.

1094. *Tamariſcus tenuiore folio* Park. *Narbonenſis Tourn.* § The French Tamariſk.

* Boerh. † This name is not in Tournefort. ‖ *Tamariſcus Germanica* Lob.
§ *Tamariſcus Narbonenſis* Lob.

N°. 476. p. 423. Ann. 1743. 1096. *Tanacetum vulgare luteum C. B.* Common Tanſy.

1097. *Tanacetum foliis criſpis C. B.* Curled *or* double Tanſy.

N°. 484. p. 599. Ann. 1745. 1200. *Thlaſpi amarum arvenſe umbellatum J. B.* The bitter Field umbellated Mithridate-muſtard.

1098. *Thlaſpi Creticum purpureum Park.* Candy Tufts.

N°. 495. p. 405. Ann. 1749. 1393. *Thlaſpi Virginianum Iberidis foliis amplioribus & ſerratis Tourn.* Virginia Mithridate-muſtard with leaves like Sciatica Creſs; but larger and ſerrated.

N°. 491. p. 45. Ann. 1746. 1249. *Thuya Theophraſti C. B.* The Tree of Life.

N°. 472. p. 77. Ann. 1741. 997. *Tithymalus Juniperi folio Boccon.* Maritime Spurge with a Juniper leaf.

N°. 494. p. 333. Ann. 1747. 1296. *Tordylium minus, limbo granulato, Syriacum* Mor. *Umb.* 37. Small Hartwort of *Syria*, with a granulated border.

N°. 476. p. 423. Ann. 1743. 1099. *Trachelium umbelliferum Ponæ.** Blue umbelliferous Throatwort.

N°. 495. p. 405. Ann. 1749. 1393. *Tribulus terreſtris, foliis Ciceris, fructu aculeato C. B. P.* Land Caltrop with a Chich leaf and a prickly fruit.

N°. 494. p. 333. Ann. 1747. 1297. *Trifolium clypeatum argenteum* Alp. *Exotic.* 307. Silver clypeated Trefoil.

1298. *Trifolium montanum ſpica longiſſima rubente C. B.* 328. Mountain Trefoil with a very long reddiſh ſpike.

N°. 494. p. 361. Ann. 1748. 1349. *Trigonella leguminibus pedunculatis congeſtis &c.* Hort. *Upſal.* Wild Fenugreek.

——— p. 333. Ann. 1747. 1299. *Turritis annua verna, flore purpuraſcente T.* 224. Spring annual Tower-muſtard with a purple flower.

* *Trachelium azureum umbelliferum* Ponæ.

N°. 491. p. 45. Ann. 1746. 1150. *Turritis muralis hirſuta minor Tourn.* * Small Tower-muſtard.

N°. 476. p. 423. Ann. 1743. 1100. *Valeriana major hortenſis* Moriſon. *Phu Off.* Great Garden Valerian *or* Setwall.

N°. 494. p. 333. Ann. 1747. 1300. *Vella* Lin. *Gen.* 654. Wild Spaniſh Creſs *or* wild Spaniſh Muſtard.

N°. 472. p. 77. Ann. 1741. 998. *Verbena anguſtifolia C. B.* †

N°. 480. p. 215. Ann. 1744. 1150. *Verbena tenuifolia C. B.* Narrow leaved Vervain.

N°. 495. p. 405. Ann. 1749. 1395. *Veronica cærulea, trifido & quinquefido folio* Fl. Bat. Blue Speedwell with a trifid or quinquefid leaf.

1396. *Veronica petræa ſempervirens* Pon. *Bald.* Evergreen Rock Speedwell.

N°. 472. p. 77. Ann. 1741. 1000. *Veronica ſpicata anguſtifolia Ger. Park.* ‖ The leſſer ſpiked Fluellin or Speedwell.

999. *Veronica ſpicata latifolia major Park.* Great broad-leaved ſpiked Speedwell *or* Fluellin.

N°. 495. p. 405. Ann. 1749. 1397. *Veronica Virginiana altiſſima ſpica multiplici floribus candidis* §. Tall Virginian Speedwell with many ſpikes and white flowers.

N°. 494. p. 361. Ann. 1748. 1350. *Viola Martia arboreſcens purpurea C. B.* 199. Purple arboreſcent Spring Violet.

N°. 495. p. 405. Ann. 1749. 1398. *Urtica foliis profunde laciniatis, ſemine lini Amman.* Siberian Nettle with deeply cut leaves, and a Flax ſeed.

N°. 480. p. 215. Ann. 1744. 1150. *Xeranthemum flore albo pleno* Hort. *Lugd. Bat.* Eternal flower *or* Ptarmica with a double white flower.

N°. 495. p. 405. Ann. 1749. 1399. *Xylon Americanum præſtantiſſimum ſemine vireſcente* Lin. The moſt excellent

* This name is not in Tournefort. Perhaps the *Turritis muralis minor Pet.* is intended. † This name is not in *C. B.* ‖ *Veronica ſpicata anguſtifolia* C. B. *aſſurgens ſive ſpicata* Ger. *erecta anguſtifolia* Park. § Flor. Bat.

excellent American Cotton with a greeniſh ſeed.

N°. 495. p. 405. Ann. 1749. 1400. *Zacintha ſive Cichorium verrucarium Matth.* 505. Wart Succory.

N. B. Part of this Catalogue, containing the plants numbered from 1251 to 1400, was drawn up after the death of Mr *Miller*, by *John Wilmer*, M. D. *Hort. Chel. Præf. & Prælect. Botan.*

Some account of the remains of John Tradeſcant's *garden at* Lambeth; *by Mr* W. Watſon, *F. R. S.* N°. 492. p. 160. Apr. &c. 1749. *Read* May 25. 1749.

VIII. Upon a viſit made to Mr *John Tradeſcant*'s garden at *South Lambeth*, *May* 21, 1749. by Dr *Mitchell* and myſelf, were obſerved the under-mentioned exotic plants.

This garden was planted by the above-mentioned gentleman about 120 years ſince, and was, except that of Mr *John Gerard*, the author of the Herbal, probably the firſt botanical garden in *England*. The founder, after many years ſpent in the ſervice of the Lord Treaſurer *Saliſbury*, Lord *Wotton*, &c. travelled ſeveral years, and procured a great variety of plants and ſeeds before not known in *England*; to ſeveral of which at this time the Gardeners give his name, as a mark of diſtinction; as *Tradeſcant*'s Spiderwort, *Tradeſcant*'s Aſter, *Tradeſcant*'s Daffodil. He firſt planted here the *Cupreſſus Americanus Acaciæ foliis deciduis*, which has been ſince ſo much eſteemed, and is now one of the great ornaments of the Duke of *Argyll*'s garden at *Witton*.

Mr *Tradeſcant*'s garden has now been many years totally neglected, and the houſe belonging to it empty and ruined; and though the garden is quite covered with weeds, there remain among them manifeſt footſteps of it's founder. We found there the *Borrago latifolia ſempervirens C. B. Polygonatum vulgare latifolium C. B. Ariſtolochia clematitis recta C. B.* and *Dracontium Dod.* There are yet remaining two trees of the *Arbutus*, the largeſt I have ſeen; which, from their being ſo long uſed to our winters, did not ſuffer by the ſevere colds of 1729 and 1740, when moſt of their kind were killed throughout *England*. In the orchard there is a tree of the *rhamnus catharticus*, about 20 feet high, and near a foot in diameter, by much the greateſt I ever ſaw.

It is not unlikely but there may be ſeveral other plants yet remaining in the garden, but flouriſhing at a different time of the year.

A letter from Dr Laurence Garcin, *of* Neufchatel, *F. R. S. to Sir* H. Sloane, *Bart. late P. R. S. concerning the*

IX. The plant in queſtion is a ſhrub, which varies conſiderably in it's ſize and figure, according to the nature and ſoil of the country where it naturally grows, as well in *Aſia* as in *Africa*, where this plant is much uſed, both as a medicine, and for it's agreeable odour.

Our author has given us the true characters of the fructification of the *Cyprus*, after the method of *Linneus*.

1. It's *calyx* is an expanded monophyllous cup, cut into 4 lobes, pointed at their extremities, and continuing attached to the fruit.

2. It's

Cyprus *of the Ancients; done from the* French *by* W. Watſon, *F. R. S.* N°. 489. p. 564. Oct & Nov. 1748. *Read* Nov. 24. 1748.

2. It's *corolla* conſiſts of 4 oval petals, ſomewhat pointed and ſinuous. They grow diſtant one from the other, and are placed between the lobes of the *calyx*.

3. It has 8 erect *ſtamina*, ranged two by two almoſt horizontally, and parallel to the ſides of the petals, and ſurpaſs them in length about $\frac{1}{2}$ a line. They grow from the baſe of the embryo at a little diſtance one from the other, and ariſe diminiſhing in their bulk to their extremities. Their *antheræ* or ſummits form each of them a little kind of purſe.

4. It's *piſtillum* is round, and occupies the middle of the *calyx*. It's ſtyle is erect, and terminated with a pointed *ſtigma*. It's length ſomewhat exceeds that of the *ſtamina*.

5. It's *pericarpium* is a round dry capſule, ſlightly four-corned; each of which corners has a ſmall prickle. It is divided into 4 compartments by an extremely delicate membrane, ariſing from a *placenta* which occupies the centre of the capſule.

6. It's ſeeds are ſmall and numerous: each of them is pyramidal, and ſomewhat quadrangular, of which the point is ſometimes ſtreight and ſometimes crooked. Every ſeed is faſtened by it's point to the *placenta*, as to a common centre, and their baſes are ſuſtained by the ſides of the capſule, all the cavity of which is filled by them.

There is but one ſpecies of this ſhrub generally known through all the Eaſt; and this is ſubject to vary according to the climate, the ſeaſon, and the ſoil.

It's names are,

1. According to different nations.

Greek, Κύπρος
Latin, *Cyprus*
Hebrew, *Copher*
Arabian and Perſic, *Henna*
Egyptian, *Elhanne*
Italian, *Alchanna*
Spaniſh, *Alkenna*
The Portugueſe in the Indies, *Foula*, *Aybana*
Apothecaries, *Alcanna*
The people of Malabar, *Mail-Anſchi*
The Brachmans, *Mety*
Malayans, *Daun Lacca*
Javans, *Batſchiar*
Chineſe, *Tſingka Hou*
Indians, *Inne*
At Bengal and Surat, *Mendi*

2. According to Authors,

Liguſtrum Dioſcoridis. Matth. 117.
Liguſtrum Ægyptiacum latifolium; item anguſtifolium. C. B. Pin. 476.
Liguſtrum Ægyptium. J. B. I. 532.
Liguſtrum orientale. Park. 1447. Raii Hiſt. 1603.
Rhamnus Malabaricus, fructu racemoſo caliculato. Raii Hiſt. 1573.

The

It's deſcription.

The *Cyprus* grows generally as a ſhrub of 10 or 15 feet in height, and has very much the appearance of privet.

It's trunk grows ſometimes as thick as a man's thigh, is ſometimes ſtreight and ſometimes crooked, and produces a great number of branches irregularly. It's outward bark is aſh-coloured, and much furrowed, and detaches itſelf from the trunk of the tree in long ſcales or pieces, by the heat and dryneſs of the climate, as in the *Perſian* gulf. It's inward bark is reddiſh without, and whitiſh within. That of the branches is ſmooth and red, like that of the hazel-tree, and green within. It's young branches are ſtreight, flexible, and moderately long. The wood of the trunk is hard and whitiſh.

It's leaves are diſpoſed in different orders upon the ſame twig. Sometimes they are placed oppoſite in pairs along the ſmall branches, and this moſt generally croſs-wiſe; ſometimes by three and three; but then the leaves are leſs, and this diſpoſition generally takes place in the larger branches; ſometimes they are alternate, but rarely, and then the leaves are largeſt. The leaſt branches are moſt charged with leaves, the larger ones leaſt. All theſe leaves are pointed at each end; the largeſt are 2 inches long, and about an inch broad in their middle; the ſmalleſt bear $\frac{1}{2}$ the dimenſions of the largeſt: their edges are even: they are ſmooth, ſhining, and of a beautiful green colour: their middle rib, which ſerves to each leaf as a ſhort pedicle, is terminated in their point, but ſends out, in it's paſſage through the leaf, alternately 4 or 5 nervous filaments on each ſide. Theſe leaves are much like thoſe of privet.

The flowers grow in bunches at the extremities of the young branches, and are endowed with a very agreeable and ſingular odour. They are of a ſtraw-colour; but as they grow old and wither, they become of the colour of a citron. The *calyx* is more pale than the *corolla* of the flowers. It's petals are turned up as much if not more than thoſe ſmall petals are which adorn the centre of a double roſe. The *ſtamina*, which are white, teanſparent, and which grow from the baſe of the embryon of the fruit, form as it were a double croſs, by their almoſt parallel ſituation and extenſion between the petals. The lobes of the *calyx*, being of the ſame length and form with the petals, ſeem to give to the entire flower an octogonal figure. The ſummits or *antheræ* are ſmall, and of the ſame colour as the petals, each having a deep furrow in it's bottom; the more theſe decay, the more yellow they grow, in the ſame manner as the petals. The furrow in the *anthera*, which at firſt is of a paliſh black, grows of a deeper hue, as the flower fades. The *piſtillum*, after the flower is gone, grows larger in the *calyx*, and becomes, when perfectly ripe, a dry, membranous, round fruit, of about 3 lines in diameter. But before it arrives to this ſtate, it reſembles very much a fleſhy berty, green on one ſide, purpliſh, and ſometimes black on the other, with very little juice. This falſe berry is the growing capſule, the ſide of which is ſoft, ſucculent, and very thick; which, in proportion as it increaſes, becomes thin, membranous, dry, and brittle: in becoming

becoming thus capacious and thin it gives room to a large number of pyramidal ſeeds, very cloſe one to another, and faſtened all by their points to a common center, a kind of *placenta*. When this capſule is in it's perfection, its outſide is ſhining, and not unlike the ſeed of coriander in colour. The *pericarpium* is as it were divided into four *loculi*, by membranes ſo delicate, that they muſt be regarded with great attention, to be ſatisfied of their reality. The exterior form of this fruit ſufficiently ſhews this diviſion, by it's roundneſs being interrupted by 4 ſlight ribs, like thoſe of a melon, which ſhews as many cells. The membranes, which divide theſe cells, ariſe from the *placenta*, and are inſerted into the ſides of the capſule.

The ſeeds, which fill all the capſule, amount to about 4 or 5 dozen, according as they are more or leſs nouriſhed; becauſe the larger ones receiving more nouriſhment, make the ſmaller ones abortive. They are always ſo preſſed in their apartments, that their pyramidal figure is owing only to this preſſure, which ariſes from their reciprocal increaſe. The pyramidal points of theſe ſeeds are crooked in ſome, and bent in others, according to the direction given them in their growing. Their colour is red or brown, and always ſomewhat ſhining.

We find, in the ancient writers of plants, ſuch as *Theophraſtus*, *Dioſcorides*, and *Pliny*, who have all in their manner treated of vegetables, of how much eſteem the *Cyprus* was among the Ancients. The Hiſtorian *Jeſephus*, and St. *Jerome* have mentioned it as a rare and precious plant, placing it in the ſame rank with the moſt valued ſpices. The fine ſmell, which it's flowers ſend forth in the countries where they grow naturally, as in *Egypt*, *Syria*, *Arabia*, *Perſia*, *&c.* has occaſioned it's uſe in the earlieſt time; and the ſame uſe continues in thoſe countries. It's being twice mentioned in * *Solomon's Song*, is a very great proof of it's being much valued in the moſt ancient time. We there ſee it was accuſtomed to be cultivated even in their vineyards. The perfumers in old times made thereof an oil or precious ointment for various uſes; but principally to give their anointings a grateful odour, and to make ſupple the limbs of the body. *Remarks.*

Modern authors have given themſelves great trouble to be thoroughly ſatisfied of the Hiſtory of this plant. There have been great controverſies among them concerning it in endeavouring to ſettle it's deſcription; but it muſt be confeſſed they have made a very ſmall progreſs in diſcovering to us it's true characters. How many miſtakes have the Botaniſts of the two laſt centuries made, owing to the bad deſcriptions of this plant, which the Ancients have left us.

Dioſcorides, who, by deſcribing the plants he treats of too briefly, always leaves their characters imperfect, ſays (perhaps after ſome other author more ancient than himſelf) *that the leaves of the plant in queſtion*

* *Solomon's Song*, chap. i. 14. chap. iv. 13. In both theſe places the *Engliſh* tranſlation of the bible has it *Camphire*, inſtead of *Cyprus*.

are

are like thoſe of the olive-tree; *that it's flowers are in bunches, and that it's fruit is black, like that of elder.* This was enough to make the *Latins* conjecture, that the κύπρος of this author was the *liguſtrum* or privet; and the more ſo, as the *Cyprus* was entirely unknown to them, ſince it only grew in *Egypt* and in *Syria*, where it was always called *henna*, or *alhenna*, and, by corruption, *alkanna*.

There is ſome appearance, that, as the *Greeks* received a good quantity of this drug from the iſle of *Cyprus*, as a ſpecies of merchandize, they would chuſe to call it *Cyprus*, rather than give it any other denomination, on account of the quantity furniſhed to them from the iſle of that name. *Pliny* took it firſt for a kind of privet or *liguſtrum*, which grew particularly in *Egypt*, and afterwards he thought it to be the common *liguſtrum* of *Europe:* this ſhews how uncertain he was as to the plant in queſtion. He judged ill in comparing the fruit of the *Cyprus* with that of the jujube-tree; but was more happy in likening the fruit (capſule) to that of the coriander, as they agree in colour, tho' that of the *Cyprus* was more large. *Matthiolus*, who thought himſelf greatly above his contemporaries in the theory of plants, aſſerts boldly, that our plant was the common privet: and in this he thinks himſelf juſtified, not only from the deſcription of *Dioſcorides*, but from the virtues attributed to the *Cyprus* by *Pliny*. He even ridicules thoſe who think that the *liguſtrum* and *Cyprus* are different plants. *Fuchſius*, who wrote before *Mathiolus*, had nevertheleſs reaſon to believe them of a different *genus*, by the account given of the *Egyptian* plant by *Pliny*; but he was wrong in confounding it with the *phillyrea* of *Dioſcorides*; and in this miſtake he has been followed by *Dodonæus*.

Bellonius, who has ſeen this plant in it's place of growth, well knew that it was not the *liguſtrum* or privet: he ſaw alſo how the Commentators of the *Arabian* authors were deceived in taking it for ſuch.

Rauwolf and *Proſper Alpinus*, who met with it in their travels, after having obſerved it in the places of it's growth, believed, as *Pliny* had done, that it was a kind of *liguſtrum*, which approached very near to that of *Europe*. They have each of them given a different figure; which made *Caſpar Bauhin* believe that there muſt be two new ſpecies of *liguſtrum*; but herein he was not followed by Mr *Ray*. In fact, we ought to acknowledge, by the characters here ſet down, that our *Cyprus* is of a *genus* truly different, and the only one of it's kind.

The *Hortus Malabar.* has given a figure of this plant under the name of *Mail-Anſchi*, which repreſents the end of a large branch ill-choſen, and ſomewhat withered, without doubt by the fault of the deſigner, who has drawn it in it's natural ſize; which is greater in *Malabar* than elſewhere, becauſe of the rains which fall there in abundance half the year. This ſhrub is leſs in all it's parts in *Arabia*, and to the ſouth of *Perſia*, becauſe in thoſe countries it rains ſeldom; but, in recompence, it's flowers have much more ſmell than in *Malabar*. It muſt be remarked

marked here upon this occaſion, that the deſcription juſt now given, and which contains the ſize of the parts, was made in a garden in the *Perſian* gulf belonging to the *Dutch* factory, and ſituated about a league from the town of *Gameroon*, otherwiſe called *Bender-Abaſſi*, where there was one of theſe trees carefully preſerved, which was the firſt I ſaw in the *Indies*; as it was complete in all it's parts, having flowers and fruit; and as it appeared to me agreeable and curious, eſpecially on account of the fine ſmell of the flowers, and as it was a new *genus* to be eſtabliſhed in Botany, I examined it with great exactneſs, and noted it's characters, figures, and dimenſions. I did not conceive it to be the *Cyprus*, not then knowing what it was. I aſked the people of the country the name of this beautiful ſhrub: they only called it *Henna*, and I could learn no other name: they aſſured me it had no other name, either in *Perſia*, or in *Arabia*. It was on the firſt of *Dec.* 1721. that I obſerved it, and deſcribed it under the name of *Frutex Perſicus, foliis liguſtri, flore & fructu racemoſo, Henna vulgo dictus.* I thus characterized it, in expectation of finding it, if it had already been deſcribed among authors, after my return to *Europe*. When I returned in 1730. I had the ſatisfaction to find it in Mr *Ray*'s Hiſtory, by the deſcription which he has given of it, extracted from various authors, in the chapter of *liguſtrum* under the *ſynonyma* of *Parkinſon*, and to ſee it in the other authors I have mentioned, eſpecially the figure given by *Rauwolf*, which is not a bad one, and is copied by *Cluſius*, *Dodonæus*, *Parkinſon*, and *Dalechamp*.

The figure in the *Hort. Mal.* under the name of *Mail-anſchi*, does not ſo happily repreſent our *Cyprus*, as that excellent work generally does the plants it treats of. The leaves of this plant there are half withered, and not in their natural diſpoſition. *Rauwolf*'s figure is much nearer the truth. The flowers are not much better repreſented than the leaves, in the *Hort. Mal.*; as, beſides other things of leſs moment, the authors of that work have neglected to make the petals appear between the lobes of the *calyx*, as always happens in a natural ſtate; by which diſpoſition the flower appears of an octogonal figure. *Rumphius*, who has written an hiſtory of the plants of *Molucca*, has given a deſcription of this ſhrub, not different from mine.

By what is here laid down of the characters of this plant, we plainly ſee that it differs widely from the *oxycantha* and *rhamnus*; of one of which the authors of the Notes to the *Hortus Malabaricus* ſuſpected the *Cyprus* to be a ſpecies. This occaſioned Mr *Ray* to range it under the laſt, ſuppoſing it's fruit to be a berry, which nevertheleſs it is not. This learned author moreover could not think that the *mail-anſchi* was the *Cyprus*, becauſe of the difference in the deſcriptions among authors, and of the imperfection of thoſe of *Rauwolf* and *Alpinus*. *Rumphius*, juſt now quoted, has ill compared the colour of the leaves of *Cyprus* to thoſe of the olive-tree.

This ſhrub, ſo cheriſhed among the eaſtern nations, is cultivated in *Africa*, *Aſia*, and all the *Indies*; that is to ſay, from near the equinoc-

tial even to 35° of N. lat. where it is much uſed, as we ſhall find by the great commerce cauſed thereby in the *Levant*, according to the relations of travellers of credit.

This plant does not love ſhade, even under the torrid zone, becauſe of the violent rains there at the time of the weſtern *Monſoon*, no more than it does in cold countries, our author means thoſe of the fifth climate; but towards the tropick, and even in *Arabia*, it grows beſt when a little ſheltered from the ſun. In hot and dry countries, as in the *Perſian* gulf, where I firſt ſaw it, it produced a great number of boughs and branches very ſhort, which gave it the appearance of white-thorn. On the contrary, towards the Equator, it's branches are further from each other, and longer, occaſioned by the moiſture from the rain. The bark ſplits into ſcales, and detaches itſelf in pieces from the trunk, in thoſe countries where it rains ſeldom; but in *Malabar*, in the iſles of *Ceylon* and *Sunda*; the bark continues entire and united almoſt all the year, becauſe of the moiſture of thoſe places.

Rauwolf remarks, that the *Turks* and *Moors* cultivate this plant with care, and even keep it in pots, on account of the ſmell of the flowers, which ſomewhat reſemble muſk. They keep theſe pots in winter in chambers or caves to preſerve the plants from cold.

Our author forgot to remark one circumſtance, mentioned by *Bellonius* in the firſt book of his Obſ. (*chap.* 44. *apud Cluſium*), where it is ſaid, that the *Henna*, or *Alchanna*, which is our *Cyprus*, differs from privet, becauſe the leaves of privet fall, and thoſe of *Cyprus* continue all the year. But this obſervation is of no weight, becauſe this difference is only apparent; and it is certain, that if our privet was cultivated in *Egypt*, it's leaves would not fall off in winter, becauſe it is not there ſufficiently cold.

The uſes of Cyprus.

Bellonius, who was the firſt of the Moderns who treated of this ſhrub under the name of *Alcanna*, and ſpoke of it's culture in *Egypt*, tells us, that the powder of it's leaves is ſo great an article of commerce among the *Turks*, that they load ſeveral veſſels from *Alexandria* for *Conſtantinople*, where the ſale of it is ſo great, that the grand Signior's revenue therefrom amounts yearly to 18000 ducats. According to him, the great conſumption of this powder ariſes from it's being uſed in beautifying the ſkin and nails, in making them red with a decoction made therewith. The women, he ſays, generally uſe it all over *Turkey*, to dye the ſkin of thoſe parts which are from the navel downwards, as well as their hands and their hair. Their children are ſerved in the ſame manner. They conſider this as a great ornament; and that the colour may hold longer, and penetrate deeper, they apply it uſually when they go out of the baths. This practice of dyeing, to beautify the body, is extended even to their horſes, of which they tinge the mane, the tail, and the hoofs. They often add alum to heighten the colour. This powder is ſent from *Conſtantinople* to *Ruſſia*. Let us now conſider the other properties of *Cyprus*.

It is not neceſſary here to take notice of what *Dioſcorides* and *Pliny* attribute to this plant; they may be conſulted, if, at the ſame time, they are regarded as being very little ſkilled in it's true qualities. Our author contents himſelf with ſaying, that the *Perſians* and *Arabians*, who appear to have been anciently the firſt that uſed this plant, frequently uſe at preſent not only it's flowers to perfume their linen, cloaths, and tables, but make a greater uſe of it's leaves in a decoction, for the cure of all diſtempers of the ſkin, as the itch, ſcabs, and ring-worm, which the air of their country cauſes from it's heat, and from the drought which often reigns there to a great degree. Theſe diſorders, if they are neglected to be cured as ſoon as poſſible in dry climates, eaſily degenerate into the leproſy; and it is on account of theſe diſorders of the ſkin, that the eating of pork is forbidden to people of every religion in theſe countries; becauſe that food there is known to occaſion theſe diſtempers.

All the nations of the *Eaſt Indies* make uſe of it in medicine, for the ſame, as well as for ſeveral other diſorders; but they particularly uſe the leaves to dye their nails; which our author thinks they had originally from the *Arabians*. In dyeing their nails, the *Indians* make uſe of the freſh leaves, which always grow in great plenty in their gardens, and apply them beaten upon their nails, mixing with them ſometimes a little lime and juice of citron. This colour laſts a great while upon the ſkin, on account of ſweating. A ſtrong decoction of the leaves in water is ſometimes uſed to tinge their nails, but more generally their ſkin and hair.

There is reaſon to believe, that this pretended beautifying of the ſkin, the hair, and nails, which long cuſtom has eſtabliſhed among the eaſtern nations, owes it's origin to a quite different principle than that of beautifying. The Ancients had no other view in the beginning, than the prevention of pruriginous and leprous diſorders in the ſkin, to which their climate ſubjected them, as well as to preſerve them from vermin, as the leaves of *Cyprus* have that property. But as in uſing baths with theſe leaves therein, they dyed their ſkin either red or yellow, according to the preparation, they accuſtomed themſelves to this colour by degrees, and afterwards regarded it as a ſalutary embelliſhment.

Theſe baths, which there are conſtantly employed for the cleanlineſs and health of the ſkin, and which the neceſſity of uſing has eſtabliſhed as a point of religion, and a duty, for the better prevention of theſe maladies, is certainly a true method to preſerve as well the body as the ſkin in a good ſtate. Theſe good effects are extended further by uſing the *Alcanna*; becauſe it's colour, paſſing in the opinion of theſe people for a neceſſary ornament, and a mark of cleanlineſs, makes the practice of bathing better obſerved.

It ſeems to our author, that theſe remarks ſhould be communicated, as well as the characters and deſcription of the plant in queſtion, to render it's hiſtory more complete, and by theſe means to make it known;

to the end that the curious may form ſome opinion of the great praiſes which the Ancients have beſtowed upon this plant.

A letter from the Rev. H. Miles, *D. D. F. R. S. to Mr* H. Baker, *F. R. S. concerning the green* Mould *on fire-wood; with ſome obſervations of Mr* Baker's *upon the minuteneſs of the* Seeds *of ſome plants.* N°. 494 p. 334. Jan. &c. 1750. *Read* Feb. 15. 1749.

X. Some days ago, happening to take notice of a quantity of what we commonly call Mould *, on the bark of ſome fire-wood, I had the curioſity to view it with a lens, of about an inch focus, when I found it to conſiſt of numbers of minute *fungus's*, whoſe regular appearance invited me to examine them in the microſcope, with a good magnifier; upon which their ſpherical heads ſeemed as if they had been nothing elſe but globules of ſeeds; at the ſame time, I obſerved ſeveral ſeeds adhering to the tranſparent footſtalks, which ſupported the heads, and many ſcattered on the glaſs-plate, whereon the ſubſtance was placed, in order to be viewed. And here I had an opportunity of ſeeing many diſtinct ſeeds, which appeared, nearly, of an oval form, but ſeveral times larger than the ſeeds of common muſhrooms, even when ſeen with the ſecond magnifier, and the latter with the firſt.

I pretend not to any ſkill in Botany; ſometimes, and, indeed, but ſeldom, I look into an author on the ſubject, as an amuſement and relief to my mind; therefore it would ill become me to attempt the referring this plant to the proper claſs. *Micheli*, in his *Nova plantarum genera*, has given us the draught of ſome, which well repreſent the figure of them, as they appear, when much magnified, Tab. LXXXII. *Fig.* 1. and in page 200. of his excellent Work, deſcribeth them, under this title, *Fungoidaſtri ſemine in ſuperna parte donati:* but then his figures are ſuch as the plants appear to have, to the naked eye (as we may preſume), ſince he does not ſay any thing to the contrary; not to mention that there are other different characteriſtics in his Deſcription. The ſame celebrated writer deſcribes another ſpecies, p. 215. under the following title, *Mucores pediculo donati*, which in reſpect of ſize, the ſubſtance, and ſome other characters, correſpond with theſe I am ſpeaking of, well enough: but as he refers to Dr *Hooke*'s Micrographia, Tab. XII. for an elegant figure of them (beſides what he has himſelf given us Tab. XCV.), both Dr *Hooke*'s and his own figures repreſent the heads, as quite ſmooth, on the ſurface; and the Doctor, in his deſcription of them, p. 126. expreſsly ſays they are of *a ſmooth ſurface*. Whence I conclude this muſt be a different ſpecies. However what the ingenious author of the *Micrographia reſtaurata* ſays of the ſeeds of theſe diminutive bodies, p. 19. is put out of all doubt.

Permit me to add, that having often viewed the heads of a ſmall kind of *fungus*, which are about $\frac{1}{8}$ inch diameter, of a coriaceous ſubſtance, I have ever found the ſeeds which are produced on the gills, much larger than thoſe of any muſhrooms I ever examined, though rather leſs than thoſe produced by this unregarded plant.

Now, that a body whoſe form is not to be diſtinguiſhed by the unaſſiſted eye, ſhould produce ſeeds ſeveral times larger than another of

* Of a bright verdegriſe colour.

the

the ſame *genus* does, which exceedeth it many millions of times in bulk, muſt ſuggeſt thoſe thoughts to one's mind, which, I know, I need not point out to you.

Some obſervations on the above-mentioned plants and ſeeds; by H. Baker, *F. R. S.*

I have carefully examined the plants and ſeeds ſent me by Dr *Miles*, in order to determine their real bigneſs; and, taking the fungous heads of the middle ſize (ſome being larger and others ſmaller), I find, according to my micrometer, that 3 of them take up the ſide of a ſquare, 70 of which ſquares make an inch in length, and conſequently, that 3 times 70, or 210 of theſe *fungi* are required to make a line whoſe length is one inch; or, in other words, that the diameter of theſe fungous bodies is, at a medium, the 210th part of an inch.

The ſeeds are oval; and I find, by the ſame micrometer, that 10 of them laid by one another the ſhorteſt way of their diameter, or 8 of them the longeſt way, fill up the ſide of a ſquare, 270 of which ſquares make an inch in length. Taking therefore 9 at the medium, 270 times 9, or 2430 of theſe ſeeds will be required to make a line of an inch in length; or, in other words, each ſeed is the 2430th part of an inch in diameter.

And according to theſe calculations 44,100 of the fungous heads, or 5,904,900 of the ſeeds may lie by one another in the ſurface of an inch ſquare.

Yet minute as the ſeeds of this little fungus are, Dr *Miles* obſerves, very juſtly, that they are larger than the ſeeds of ſome muſhrooms, which exceed it many millions of times in bigneſs. As to which, I beg leave to take notice, that the proportion, in ſize of the fruits or ſeeds of trees or plants, to the ſize of the trees or plants that bear them, comes under no regulations that correſpond with our conceptions. For the vaſt bulk of ſome ſorts of timber-trees (the beech and aſh, for inſtance) is produced from a ſeed ſmaller than that of the common garden bean. The towering and mighty oak produces for it's fruit only a little acorn, whereas the pumpkin (ſome whereof weigh above 100 pounds) is the production of a feeble creeping plant, unable to ſupport itſelf, and much leſs it's enormous fruit. The *vanilla* (a plant that riſes to the height of ſeveral feet, by claſping about whatever it finds near it) produces, in long pods, ſeeds ſo ſmall, that their diameter is not more than the 100th part of an inch. Suppoſing therefore the cavity of the pod to be equal to a cylindrical tube of $\frac{1}{100}$ of an inch diameter, and the length of the pod to be ſix inches (which dimenſions are taken with great moderation) the number of ſeeds contained in one ſingle pod will be more than 47000. Moſt kinds of fern, of which ſome are pretty large plants, bear ſeeds ſo extremely minute, that they appear to the naked eye only like a fine duſt; while ſeeds of a conſiderable bigneſs are produced by plants of a great deal ſmaller ſize.

XI. The

Obſervations relating to vegetable Seeds; *by* J. Parſons, *M D. F. R. S.* N°. 474. p. 184. June &c. 1744. *Read* Nov. 22. 1744.

XI. The ſeed of the *muſk ſcabious* reſembles an octogonal *vaſe* with a ſcalloped brim: the whole is bell-ſhaped, having ribs or diviſions, which run down from the mouth of the *vaſe*, and, becoming narrower, form the bottom: between theſe ribs, down to the beginning of the narrow part, it is clear, tho' not quite tranſparent; and, from thence to the bottom, the ribs are hairy. This *vaſe* contains a ſeed, which is like a *peſtle* ſtanding in a *mortar*: the *peſtle* is looſe in an octogonal caſe; but the narrowneſs of the mouth of this caſe hinders the peſtle's being drawn out, becauſe it's extremity, within, is round and bulky. From it's upper end ariſe 5 ſpiculated *ariſtæ*, whoſe little thorns are directed upwards, and are thereby prepared to cauſe the ſeed to recede from any thing that might injure it upon being touched; and the baſin, from which the *ariſtæ* riſe, is of a fine green colour; they are of a ſhining brown.

The *angelica* is one of the moſt fragrant and agreeable ſeeds, for it's ſmell, in the world. When the huſk is pulled off, the *nucleus* appears of a browniſh colour. and it's ſhape is elliptical. By the help of the *microſcope*, we know what produces that charming ſmell, being a fine amber-coloured *gum*, which appears in ridges diſpoſed alternately, with others of a browniſh colour, in a longitudinal direction all over the *nucleus*. What appears white, on the flat ſide, is a *theca*, which receives a very minute *ſtilus* from the pedicle that ſupports it.

The ſeed, which is vulgarly called *Grains of Paradiſe*, although promiſing from it's aſpect but very little that is curious, being only a brown irregular ſeed with flats and angles, and having an *apex* like the mouth of a purſe drawn up with a ſtring; yet, when diſſected, produces a moſt beautiful appearance. In a longitudinal ſection, you ſee, firſt, the edge of the brown *cortex*; next to that, a black pitchy ſubſtance; and, within that, an exceeding white radiated matter, which looks like a fine white ſalt, and is, probably, a mixture of a volatile pungent ſalt with a farinaceous ſubſtance: the radiation ſeems to confirm this opinion; for, if it were only a *farina*, it could have no ſuch appearance, and ſo does it's exceeding ſharp taſte. But the moſt remarkable and curious part of this ſeed, is a little piece of *camphire*, exactly ſhaped like a common vinegar-crewet, having a round bottom, and a long taper neck. This is the conſtant form in hundreds of theſe ſeeds that I have cut. Theſe curious appearances, I believe, were never obſerved before.

I ſhall, at preſent, only mention one more ſeed, which is that of the great *maple-tree*. It conſiſts of a *pod* and it's *wing*: two of theſe grow upon a foot-ſtalk with the pods together, which makes them reſemble the body of an inſect with a pair of expanded wings. The wings are finely vaſculated, and the pod is lined with fine ſilky down, which contains a round compact *pellet* covered with a brown membrane, that ſticks very cloſe to it. When this is peeled off, inſtead of diſcovering a kernel, as in other ſeeds, an entire *green plant* appears to be folded up in a moſt ſurprizing manner, whoſe *pedicle* is about $\frac{2}{5}$ of an inch long, and it's

it's ſeminal leaves about $\frac{6}{8}$ each; between which the *germina* of the next pair of leaves are barely viſible to the naked eye, but plain with a *microſcope*. This diſcovery gave me great pleaſure, as believing myſelf the only one who had obſerved it; but, ſome time after, looking into *Derham*'s *Phyſico-Theology*, upon another account, I found it mentioned, as if Dr *Highmore* had ſeen and communicated it to Mr *Ray*. I believe, however, as none of this learned *Society* have ſeen it, except thoſe I have ſhewn it to, the ſight of it will not be diſagreeable.

Numbers of ſuch amazing *phænomena* appear every day in my obſervations (ſome of which ſhall hereafter be laid before you, if theſe prove acceptable), which excited me to a reſolution, of examining and deſcribing all the *genera* of ſeeds. A work which is now publiſhing under the title of *The Microſcopical Theater of Seeds*, &c. in a manner, which, I hope, will render Botany more eaſily underſtood, will hand down to poſterity the true figures of every ſeed and it's ſections; and, by the new diſcoveries, which often occur thro' the courſe of my obſervations, lay a foundation for future obſervers to build ſomething uſeful upon; and ſettle ſome points relating to the different ſubſtances contained in vegetables, which yet remain doubtful.

XII. 1. I have ſent you ſome *ruſſetings* changed by the *farina* of a next-door neighbour, whoſe name I wanted ſkill to know; but can only ſay, that the *ruſſeting* has exactly acquired his face and complexion.

[Mr *Collinſon* then produced ſeveral ſamples of the apples; an unteinted ruſſeting; a ruſſeting changed in complexion, which grew among a great cluſter of unaltered brethren; and ſome apples of the other tree, which had cauſed the change in the ruſſetings, and whoſe fruit had in return received a rough coat from the ruſſetings.]

Extract of a letter from Mr Benj. Cook, *F.R.S. to Mr* Peter Collinſon, *F. R. S. concerning the effect which the* Farina *of the bloſſoms of different ſorts of* Apple-trees *had on the fruit of a neighbouring tree.* N°. 477. p. 525. Aug. &c. 1745. *The* Apples *ſhewn* Nov. 14. 1745.

Theophraſtus takes notice of this Παραλλαγη, as he calls it; and tells us the old Divines were wont to make a great pother about it, and foretel great events by it: *Pliny* informs us, there was one who wrote a whole book about ſuch changes. But the uſe I ſhould make of it, is chiefly this, that it may be of importance to the curious in fruits, to take care how their trees are ſorted, and what company they keep. For tho' this change be not ſo conſpicuous in apples which have a ſmooth green coat, as in the ruſſet-breed, yet one may ſuppoſe impreſſions of this ſort often made on them; and perhaps their juices altered for the better or worſe. *

2. I ſent

* Sir *Joſ. Ayloffe*, Bart. F. R. S. communicated, on *July* 1. 1731. from the Rev. Mr *Henchman*, Prebendary of *Saliſbury*, ſome obſervations of peaſe of different colours infecting one another in the ſame manner as the apples above-mentioned.

Mr *Henchman*, in the ſpring 1729. ſowed a piece of ground in his garden with white peaſe, and two double rows of blue peaſe, with an alley four feet wide between; in autumn,

——*by the ſame.* N°.490. p. 602. Dec. 1748. *Read* Dec. 22. 1748.

2. I ſent you laſt year a ſpecimen of the effect of the *farina* of a rough coat apple ſtriking on the flower of a ſmooth-coat; I have now ſent an example of the *farina* of the latter changing the former into it's own dreſs and likeneſs.

The ſituation of the ruſſeting was ſuch, that he was ſurrounded by winter pippins, pearmains, and ſuch-like; and we put the maſter-fruit together with ſeveral of the changelings, as they grew on the ſame branches mixed together.

This inſtance will ſhew what alterations may be expected in cognate ſpecies; and I ſhould have given an example of a kind of antipathy betwixt the pear and the apple in like circumſtances, but was diſappointed.

——*by the ſame.* N°. 493. p. 205. Oct. &c. 1749. *Read* Nov. 2. 1749.

3. When the farina of one apple impregnates another's bloſſom of differing ſpecies, we ſee the change in the fruit; but whether any laſting impreſſion is left on the bough which bore it (as ſeems to be in tulips and ſome other flowers), is not ſo eaſy to determine, experiments of this ſort being not to be made at all, but catched at diſtant opportunities; and till this point is ſettled, the diſtemper of my good friend's tree muſt reſt unexplained.

Artificial helps of ſight have added to former diſcoveries the explosive manner of the *farina*'s action; but what may be the effect of the inconceivably fine ſubtile matter emitted from it's globules, and continually wafted about in great plenty and variety in the ſummer air, not only on vegetable productions (where on different ſubjects it may not improbably have oppoſite effects) but other matters not yet ſuſpected to be ſo much under it's influence, remains a field of inquiry for future ages. However, to what Mr *Loggan* hath very juſtly obſerved on the manner of impregnation of the ſeeds in mayze, I can add this, that if the ſeed and whole ſpecies of mayze be planted about two yards diſtance from each other, there will be a mixture of red and white grains in the ears of each plant, and you may with pleaſure obſerve the filament in the white plant, which hath been ſtruck with the red farina, diſcovering it's alien commerce by a conſcious bluſh, and by counting the threads they ſtained, foretell how many correſponding ſeeds will appear red, at the opening of the ear, when ripe.

autumn, upon gathering ſome for ſeed, he opened one of the pods, and was ſurprized to ſee one blue pea at the end next the ſtalk, with ſix white peaſe: but after having examined ſeveral other ſhells very carefully, he found a great variety of intermixtures of the white and blue peaſe in the ſame ſhells; ſometimes one white (or blue) only at one end, ſometimes at each end; ſometimes two white (or blue) with one of the other colour interchangeably; and thus the whole parcel that was rubbed out for ſeed was intermixt white and blue. The next year, he ſays, not having plotts of white and blue peaſe ſtanding near one another, he did not find any ſuch mixture in the ſeveral parcels then ſaved for ſeed. But it is pity he did not pick out a ſufficient number of the blue peaſe from among the white, and ſow them by themſelves, in order to ſee what coloured peaſe this mixt breed would have produced. *C. M.*

XIII. 1. The

XIII. 1. The firſt experiment I made, was gathering the bud of a hollyhock ſo young, that the *petala* were not yet formed; and ſtripping off the *calix*, nothing appeared, but the *apices* cloſe to the *ſtylus* (for the *ſtamina* were not yet perceptible): theſe *apices* appeared to me to be a kind of bag; and I could plainly perceive a ſeam (if I may ſo call it) run down the middle of it. This occaſioned me to take a fine needle, and carefully open them; which I did, and found each full of *farina*, which ſeemed to lie very regular. This determined me to take notice of the courſe of the *farina* in each flower, and I obſerved the following particulars:

A letter from R. Badcock, *Eſq; to Mr* H. Baker, *F. R. S. containing ſome microſcopical obſervations on the* Farina Fœcundans *of the* Holyoak *and the* Paſſion-Flower. N°. 479. p. 150. Mar. &c. 1746. *dated* Kenſington Nov. 6, 1745. *Read* April 13. 1746.

Aug. 24. I took notice of a flower juſt going to blow, and the *petala* appeared; the *farina* was then juſt burſt from it's *apices*. The time of theſe burſting is as ſoon as ever the *petala* blow out enough to be affected by the ſun.

25. The flower opened more, and the *farina* appeared ſo thick on the outſide of the *apices*, that they ſeemed quite covered from ſight, without a very narrow inſpection.

26. The *farina* began to decreaſe viſibly, and continued to do till the

27. When I perceived ſome red curled *ſtamina*, without any *apices*, puſhing themſelves out at the top through the others. Theſe were, within their bend, thick ſet with a kind of hairs *, and in their paſſage took a good quantity of *farina* with them, which remained a day longer than that which was contained in the *apices*. I could not obſerve the *farina* to fall on any particular part of the flower, but ſeemed rather to be diſperſed. When theſe red *ſtamina* appear, the *farina* is going, and the *apices*, which contained it, dead.

The flower was kept till it withered, and the *ſtylus*, *&c.* cut off; but in neither experiment was there found any difference, after a month's keeping the *farina*, except in the colour, which was deeper.

Cutting off the *ſtylus*, *&c.* may have a conſiderable effect upon the ſeed, but ſeems to have but little on the flower: for, tho' it was cut off as ſoon as poſſible, yet the flower blew out the ſame as if nothing had happened, till about the time that the *farina* might be ſuppoſed to act; then the *petala* began to look black next the *ſtylus*, and dropped off a day ſooner than the regular blowing flower.

Not having an opportunity of purſuing this further, for want of flowers and warm weather, I applied myſelf to the experiments of Mr *Needham* †. I bruſhed off ſome dry *farina*, and, putting *Thames*-water to it, found it would not burſt, under the ſpace of 7 or 8 minutes, and not till they are ſoaked in the liquid: for, at the time of acting, they

* I cannot obſerve any thing in this flower, unleſs it be theſe, that deſerve the name of *Papillæ*: tho' the firſt magnifier could ſhew nothing ſatisfactory as to this point.

† New *Microſcopical* Diſcoveries, *&c. Lond.* 1745. 8vo. p. 74.

ſeldom or ever lie one upon another, but float off, till they are clear of all incumbrances. But I obſerved one particular, which ſeems entirely to have eſcaped Mr *Needham*, which was, That, on the application of water, they inſtantly emit a pellucid kind of matter (much thinner than that at burſting) thro' their capillary prickles, with which they are thick ſet. Upon the application of *Briſtol* water they are found to burſt much ſooner, and with leſs emiſſion. In vinegar they ſcarce ever burſt; at leaſt, if they attempt it, are inſtantly ſtopped by the ſharpneſs of the liquor. I dont find vinegar to have any other effect than this.

In making my experiments on a freſh-blown holly-hock, I obſerved a *luſus naturæ* of two globules quite ſmooth and ſhining (contrary to their nature, which is rough): one of theſe acted very ſoon, the other not at all. The whole *farina* ſeem to me to have a ſtrong ſuction; for I was obliged, in the ſpace of 10 minutes, to apply water 3 times, in order for them to have enough to act in; and I obſerve, that they burſt with a greater force, and throw out a much larger pulp, when thrown into a depth of water.

Tho' I have been often obliged to ſupply them with water, yet I find the greateſt number always act with the firſt water. I have often ſeen a globule, tho' it has been burſt on one ſide before, yet has burſt a ſecond time on the oppoſite. Which ſeems to me as if the firſt aperture was inſtantly cloſed, ſo as not to emit again: for I have made it an obſervation, that tho' the pulp is never thrown out at the ſame place a ſecond time, yet the globule, before it has done acting, ſhall have had ſo many burſts, as to look like a picture of a bomb-ſhell, with it's various diſcharges before the ſeparation of it's parts.

The paſſion flower I look upon to be the fitteſt flower for experiments on the *farina*, of any. Firſt, as it is large, and long in bloom: ſecondly, as the flower by it's nature preſerves itſelf and it's *farina* from injury: for, no ſooner is the ſun off of the flower, but it gradually cloſes up as the ſun declines, till the *petala* are ſo cloſe, as not to admit any but very violent ſhowers, This, with the diſpoſition of the *farina*, which is on the inſide of it's *apex*, when the flower is cloſed, likewiſe preſerves it from wind. Add to this the infinite quantity of *farina*, which may be taken off (from the largeneſs of it's *apex*) without any force, damage to the flower, or itſelf. To this likewiſe we may add, that, after a night's keeping gathered, the *farina* has the ſame effect and action in the morning, as it had when freſh-gathered: which quality no other *farina* has. *See* Needham, *page* 77.

The *farina* of the paſſion-flower appears (by Mr *Cuff*'s double reflecting microſcope) Magr. 6, 5, 4, to be a ſmooth round globule, of a pretty full yellow, like the appearance *Fig.* 113. which we ſuppoſe the *area* of the microſcope. Theſe globules, on being more magnified, are found to have ſome 3 circles (as *Fig.* 114.) others two, others none. Among theſe I have found a conſiderable number quite white, as attempted to be ſhewn in *Fig.* 113.; but I never obſerved theſe act. When the

Fig. 113.
Fig. 114.

globules,

bules, *Fig.* 114. come to be magnified with the firſt or ſecond magnifier, they appear indented, exactly like *Fig.* 115. All the coloured Fig 115.
ones, tho' differently marked, yet all act alike. I obſerve that theſe act in a much leſs ſpace of time than thoſe of the hollyhock, which are ten minutes, though freſh; whereas theſe act inſtantly, tho' kept for 24 hours *: neither have theſe any ſuction or convulſive motion; acting entirely ſtill, and in the firſt water. Attempting to apply them to the opake microſcope after their action, they ſtuck round the point like wet ſkins: but one thing I obſerve, that they burſt but once, throwing out all their pellucid matter, which is yellow, at the firſt diſcharge. They act no otherwiſe in oil, but by emitting a matter much thinner than that at burſting: but, having lain in oil for a minute, and put from thence into water, they act inſtantly, and with a ſeeming additional force. Being put into malt-ſpirits, they exhibit a very agreeable appeararance: all thoſe which emit, as in oil, lie dead and ſtill; but thoſe which neither burſt nor emit, are thrown into ſo violent an agitation, that they appear like animalcules; ſometimes joining ten or a dozen together; on a ſudden, an imperceptible force ſhall throw a globule, ſometimes (two or three) three parts over the *area* of the microſcope; often two globules ſhall be whirled round with incredible ſwiftneſs, for the ſpace of near a minute, then ſeparated by the ſame imperceptible ſwiftneſs, fly each a different way. They will act thus, till the liquor may be ſuppoſed to dry up, when ſupplying them with liquor, will regain their motion; and though you put liquor often to them, yet every time will give them that ſwiftneſs. Upon applying the magnifier, N°. 2. I find it is the white unacting globules that do thus, and imagine that they riſe with that ſpirit which evaporates; and their not being volatile occaſions them to ſtop at top, and continue this motion as long as the liquid has any evaporation; for I obſerve, after a certain time, they lie like the others which have acted. In this liquid they burſt, in ſuch a manner, as that the places from whence they burſt are perceptible *(See Fig.* 117.) and the pieces broke off very plain. The way I obtained a ſight of this, was to let the globules dry after their action, on the glaſs. Some burſt ſo fierce as to break off a piece, as *Fig.* 117.; others can ſcarce be ſeen to have any alteration: yet no Fig. 117.
magnifier will go ſo far as to ſhew the matter thrown from them, any otherwiſe than as a yellowiſh water.

Upon applying *aqua fortis* to this *farina*, the ſhape and marks are inſtantly changed to thoſe marked in *Fig.* 116.; whereas, on the holly- Fig. 116.
hock, it has no other effect than burning up their capillary prickles.

The *lilium flore reflexo* of Mr *Needham* I have never ſeen; thoſe kind of flowers having been a long time out of bloom; but, as to this of the paſſion-flower, what is placed on the top of the piſtil, the deepeſt mag-

* I have ſince had a flower lay in a window from *Friday* morning till *Monday*, and the *farina* has acted very briſkly.

nifier will not ſhew perfect: and though I have endeavoured all poſſible means, could never obtain any thing ſatisfactory; ſo that I muſt freely own either my misfortune or my ignorance. On the piſtil of the ſtock gilliflower there are very plain to be ſeen ſome kind of capillary tubes on it's top; but then they appear to ſtand thus ⓪, without any aperture, as deſcribed by Mr *Needham*. Whether theſe may have their pores, or not, I am not able to ſay. I ſhall continue making obſervations on this part of the flowers which may fall under my inſpection, and hope ſoon to be ſatisfied in this point: but, as to his opinion of the action of the *farina*, I cannot, at preſent, grant it any other active power than ſuction; for had there been any inward mechaniſm, the various experiments I have tried muſt have ſhewn it in ſome ſhape or other; whereas thoſe who have the greateſt motion before action, I can only obſerve to ſwell, and look larger, on the almoſt immediate application of water.

Further obſervations and experiments on the Paſſion-Flower, *and it's* Farina; *by the ſame.* Ibid. p. 166. *Read* April 24. 1746.

2. In my laſt account I concluded with obſerving, that what was in the top of the piſtil, was ſo far from making me believe it *papillæ*, or any other paſſage for the action of the *farina*, that I deſcribed it to ſtand thus ⓪; and queried if there might not be pores on it's top, as granting the action of the *farina* to have it's effect and conſequence, as deſcribed by Mr *Needham*, *p.* 80. as there was no poſſibility of it's ever being in the nature of the *lilium flore reflexo*, deſcribed by him, by it's *papillæ* being of uſe in the manner and figure deſcribed *Plate* 5. *Fig.* 2. of his book. I have ſince taken all poſſible methods to ſatisfy myſelf, and ſhall communicate the following experiments, being the moſt material, without any apology: as, upon a conviction of an overſight or miſtake, I am very ready to acknowledge my error.

After the *calix*, *petala*, &c. are ſtripped off, the firſt thing the flower preſents to view are a double row of purple threads: theſe threads appear thus; *Fig.* 118. on which we may plainly perceive a ſort of capillary tubes (or whatever you will call them) ſtanding as I before obſerved. Here we may be at a loſs for a paſſage for the acting matter of the *farina*; we muſt therefore look further. Upon cutting theſe threads longitudinally, they appear in many places as this before us, and are often pretty full. The occaſion of theſe appearances, (*Fig.* 119.) I own I am not Botaniſt enough to ſolve, nor will the firſt magnifier give me ſatisfaction. At the bottom of theſe, ſet round the ſtem, is a ſingle row of ſmall threads, not exceeding $\frac{1}{2}$ an inch: theſe appear to have much broader heads than the long purple threads around them; and being ſo well ſecured and fortified from injury, I imagine to be of great uſe and conſequence to the flower; yet they appear ſet in the ſame manner, though the tubes do not riſe ſo high. I am inclined to believe theſe may be deſigned in this flower as *papillæ*; 1ſt, As they are ſo well fortified from all injury from without; 2dly, As the *farina*, when the flower blows, and cloſes at night, is turned inwards; which time, I am likewiſe

Fig 118.

Fig. 119.

likewiſe inclined to believe, is the time of action *, at leaſt in this flower; for, after a hot day, why may not the evening dews penetrate, and the *farina*, having at that time a ſtrong ſuction, from the drought, occaſion it to act? But I have not made the obſervation at which time the *farina* acts moſt, morning or evening; which I fancy would ſatisfy. 3dly, From this being the only part of the flower which appears with a hollow or indenting on it's top; by which the action of the *farina* (thus lying inwards) may fall down, and ſettle in this hollow, as a drop of water would do: for I obſerve there are no obſtructions to ſuch a ſuppoſition, in the ſtructure of the flower.

We will go now to the top of the flower, where are 3 *ſtamina* placed on the *uterus*: theſe are ſet in a manner deſcribed before with tubes; but, on making a longitudinal ſection, I cannot find them carried on in any ſhape.

We come next to the *uterus*; here I cannot obſerve any tubes at all; nor is there any appearance to me remarkable, till we come to the bottom of the *ſtylus*; and then, by degrees, from a ſmaller to a greater it riſes, till the appearance becomes thus; (*See Fig.* 120.) Fig. 120.

On examination, I find the 5 appearances to anſwer the 5 *ſtamina* on which the *apices* are ſet; and from this appearance, growing nearer and nearer to each other by degrees, they join at laſt all in one in the ſtalk of the flower.

3. *March* 3. I obſerved a yew-tree in bloſſom, but having at that time only the bud (*Fig.* 121.) which, taken out of it's caſe, may be ſeen with it's *umbilicus a*, in *Fig.* 122. That every flower has it's *farina*, has been juſtly advanced; and as it was my deſign to obſerve that, I took a branch into the houſe; but, making but a ſlight obſervation at that time, I laid it on half a ſheet of brown paper in a warm drawer; and, to my great ſurprize, coming to examine it in a few hours after, I found the whole number of the buds blown out into full flower, and ſuch a quantity of *farina* on the paper, that it ſeemed more like a paper of brimſtone than any thing elſe. I then no longer neglected a thorough examination, which I began and completed in the following manner:

Concerning the Farina Fœcundans *of the* Yew-Tree; *by the ſame.* N°. 480. p. 189. *Read* May 15. 1746. Fig. 121. Fig. 122.

Fig. 123. is a ſeparate view of it's partitions, before it is near blowing. Every five of theſe go to a flower, and divide properly for blowing, ſome ſmall ſpace before they burſt: there are ſometimes ſix to a flower, but it is ſeldom: they open at the bottom *a a*, and immediately, letting drop the *farina*, turn themſelves up; ſo the top, which now appears the head, will, when blown, be the centre of the flower. (See an explanation of this in *Fig.* 124. where the whole diviſion makes the flower and the ſtrokes the diviſion of the *petala*.) *a* is the ſtem of the Fig. 123. Fig. 124.

* The two following experiments have given me grounds for this; 1ſt, That the *farina* I obſerve, is always damp in the morning; 2dly, On examining it after a froſty night, ſcarce one burſt. I at this time made an obſervation, which, I believe, has as yet eſcaped every one, That the intenſe cold has ſuch an effect on the globules as to throw many into the ſame ſhape as *aqua fortis* will.

flower:

flower: now the bottom opening diſcovers this ſtem; and the eaſy tranſition of turning very near inſide-out, makes a complete flower, Fig. 125. *Fig.* 125. the ſhape of which is ſeldom exact in any two; tho' there are Fig. 126. near 12 flowers formed by the bud. (See the back part of one, *Fig.* 126. in which thoſe ridges ſhew it's diviſion on the back.) We come now to Fig. 127. the *farina*, *Fig.* 127. which matches the reſt of the flower, as to irregularity; there ſcarce being two alike; and, when viewed opakely, has a great reſemblance to the ſmall pieces we obſerve in a paper of gum *arabick*; 'tis opakely of a clear white; but, when laid on paper in a quantity, appears like flower of brimſtone, only paler. It's action is as va- Fig. 128. rious as it's ſhape. (See ſeveral forms, *Fig.* 128.) It ſeems to be only fixed in one particular, which is, that tho' there are ever ſo many different ſhapes, when dry, water once put to them makes them all round, before any action begins: a proof to me, that there is a ſuction. In ſhort, the whole proceſs of this is ſo various and entertaining, that I never met with any thing hitherto to be compared to it.

My haſte had made me almoſt miſs the moſt material point of all; which is, that this flower has neither *apices*, *ſtamina*, nor *ſtylus*; which is the reaſon why ſo much *farina* is ſhed. I have not yet examined it as to impregnation, but ſhall go on that the firſt opportunity.

A letter from Mr John Hill, *Apothecary, to the* Preſ. *concerning the manner of the ſeeding of* Moſſes; *and in particular of the* Hypnum terreſtre, trichoides, luteo-virens, vulgare, majus, capitulis erectis. Raii Syn. *Ed.* 3. *p.* 84. N°. 478. p 60. Jan. & Feb. 1746. *Read* Feb. 13. 1745-6. *with alterations.*

XIV. The many late diſcoveries of the ſeeds of vegetables (formerly ſuppoſed to produce none) have opened a way to an extenſive ſcene of knowledge; and led to a ſeries of obſervations, in which there will be found an almoſt inexhauſtible ſtore of delight and admiration. The moſſes, in particular, one of the moſt beautiful parts of the vegetable creation, will afford the curious obſerver more matter of entertainment than perhaps any other claſs of it. A ſpecimen of which I do myſelf the honour of communicating to you in this letter.

The particular obſervations it contains, though new and ſurprizing, you will allow unqueſtionable in fact and certainty, when I add, that they are what I have not only often repeated myſelf, but have alſo ſhewn to Mr *Baker*, Dr *Parſons*, and Mr *Needham*, Gentlemen of too nice eyes, and too converſant in obſervations of this kind, not to have diſcovered miſtakes, if there had been any; and who all agree, that no diſcovery by the microſcope was ever clearer.

The manner of ſeeding of the moſſes in general is a thing perhaps as little underſtood, as any part of the vegetable ſyſtem: what I have to offer here is the explaining and deſcribing it clearly, in one ſpecies at leaſt, from a number of obſervations made on it in it's different ſtates, and at different ſeaſons of the year.

The thorough knowledge of the operations of nature in the ſeeding of one of theſe little plants, may, I hope, be a fair ſtep to the diſcovering it in them all. And the almoſt infinite variety of not only *ſpecies*, but *genus's*, in this claſs of vegetables, ſeems to promiſe the inquirer a vaſt variety of new diſcoveries.

I ſhall

I ſhall not trouble you with an account of the erroneous opinions of others on this ſubject; you will find, by this account, that thoſe who have been uſed to judge well in other parts of Botany, have been altogether miſtaken here: and even the accurate Mr *Hook*, who of all others has come neareſt a diſcovery of the truth, and who actually ſaw ſome part of the organization of theſe little bodies, was ſo far from making the leaſt gueſs at the nature and uſes of what he ſaw, that he even miſtook the ſtructure of it.

The particular ſpecies of moſs, whoſe head I here ſend you a deſcription of, is the *Hypnum terreſtre, trichoïdes, luteo-virens, vulgare, majus, capitulis erectis.* Raii Syn. *ed.* 3. *p.* 84. *Hypnum vulgare, ſericeum, recurvum, capſulis erectis cuſpidatis*, Dill. Hiſt. Muſc. 323.

The head of this moſs appears to the naked eye (as at *Fig.* 129.) of a pale-brown colour, and ſmooth ſurface, and is in part covered with a membranaceous *calyptra*, reſembling in ſhape an extinguiſher, or a funnel inverted. When this *calyptra* is taken off, and the head placed before the microſcope, the ſurface of it is ſeen to be ridged with longitudinal *ſtriæ*, the baſis of the head is of a dark orange-colour, and more opake than the reſt; and the top is bounded by an orange-coloured ring, ſwelling out ſomething beyond the ſurface of the contiguous parts of the head. A cloſe obſervation and good glaſſes have informed me, that in this little head there are not wanting the parts eſſential to the fructification of what are commonly called the more perfect plants. Fig. 129.

This ring is truly a monophyllous undulated *calyx*; and within it ariſe 16 pyramidal fimbriated *ſtamina:* Theſe are of a pale-greeniſh colour, and are loaded with a white oval *farina:* the *ſtamina* all bend toward each other from their baſes, and almoſt meet in a point at their tops. This is their appearance when the head is nearly ripe, and is what is expreſſed at *Fig.* 130. And immediately under the arch formed by theſe *ſtamina* is placed a ſlender, cylindric, hollow *piſtillum*, through which the *farina* makes it's way, and is diſperſed among the ſeeds in the head. The external membrane of the head is a continuation of the outer covering of the ſtalk, and is ſtrengthened at it's baſis by four or five ribs, which ſoon loſe themſelves in the *ſtriæ*. A longitudinal ſection of the head ſhews, that the membrane before-mentioned incloſes a ſeed-veſſel ſo large as to fill it every way: in moſt places they touch; but wherever they do not, a number of very ſlender, white, and tranſparent fibres ſhew themſelves, which join them together. This ſeed-veſſel is filled with perfect and very beautiful ſeeds; they are round, tranſparent while unripe, but afterwards opake, and of a very beautiful green; which colour they retain even when dried. Fig. 130.

The number of ſeeds in one of theſe heads is aſtoniſhingly great: I have many times attempted to count them, in ſuch as were full, and out of which few or none had been diſlodged by the cutting; and as the accounts, at different times, and in various heads, have not much differed, I ſhall venture to inſert a gueſs from them. It will eaſily be conceived,

conceived, that in ſeeds ſo minute as well as numerous, this muſt be a very difficult taſk; and indeed to count every ſeparate ſeed, I believe would be not much leſs than impoſſible: the method therefore by which I make the calculation is this. I count in $\frac{1}{8}$ part of the circumference of $\frac{1}{2}$ the head 9 ſeeds, $9 \times 8 = 72$; there are therefore 72 ſeeds in a line, which reaches round the circumference of $\frac{1}{2}$ of the head. I judge the length of this half to be to it's circumference as 3 to 2, or thereabout: therefore, in one longitudinal line in it, there muſt be 96 ſeeds; the whole quantity in $\frac{1}{2}$ the head therefore is $72 \times 96 = 6912$; and, doubling that for the equal number of ſeeds of the other half, there appear to be in one head $2 \times 6912 = 13824$ ſeeds.

Fig 131. *Fig.* 131. ſhews a longitudinal ſection of the head with the ſeeds, the *ſtamina*, and the joining of the capſule with the external membrane of the head.

The *ſtamina*, examined alone, afford a moſt pleaſing ſight; they are compoſed of a white tranſparent ſubſtance, of a pyramidal figure, everywhere covered with a pale-greeniſh cruſt; which is the receptacle of a vaſt quantity of an oval *farina*, ſo extremely minute, as to be viſible only with the moſt powerful magnifiers in the double microſcope.

The outer membrane of the head becomes ſeparable from the capſule when perfectly ripe and dry; and then, viewed in the double microſcope, ſhews a reticular texture, not viſible in it before.

When this head is firſt produced from the plant, the *ſtamina* are very ſlender, and ſtand erect; the head is ſcarce any thicker than the ſtalk, and the *calyptra* covers the whole, to ſhield the tender ſubſtance of the *farina* from external injuries. As the *farina* afterwards ſwells in the *ſtamina*, the ſeeds alſo in the head increaſe in bulk, and become viſible, and are then tranſparent; but when it is perfectly ripe, the *calyptra* falls off, and the wind diſlodging the *farina* at times, as it ripens ſome ſooner, ſome later, it makes it's way through the *piſtillum* into the head, and the ſeeds then become much larger and opake; to favour the falling of the *farina* into the *piſtillum*, the *ſtamina*, as they ripen, are, by the increaſe of thickneſs in the head, thrown farther and farther from each other at their baſes, but bend inward at the points, ſo as to form a kind of arch over the opening of it.

The annual product of theſe moſt minute ſeeds is aſtoniſhing: an ingenious gentleman has given an account of the wonderful increaſe of the mallow; one of which he found to yield in one year 200,000. But this is much inferior to thoſe of the little plant before us; for, allowing to a root of this 8 branches, and to each branch 6 heads (which any one, who will obſerve it in a thriving ſituation, will find a very moderate computation), the produce of this is $6 \times 13824 = 82944$; and $8 \times 82944 = 663552$ ſeeds, the annual produce of one ſeed; 13824 of which are contained in a head, whoſe length is but $\frac{1}{5}$ of an inch, and it's diameter but $\frac{1}{23}$ of an inch, and whoſe weight is but the thirteenth part of a grain.

Fig. 129.

Fig. 129. ſhews the head of this moſs in it's natural dimenſions, with and without the *calyptra*. *Explanation of the figures.*

Fig. 130. The ſame viewed through a powerful magnifier, without it's *calyptra*.

Fig. 131. A longitudinal ſection of the ſame.

Fig. 132. *Stamina* taken off from the head, and viewed by a more powerful magnifier.

Fig. 133. A piece of the outer membrane of the head, ſhewing it's reticular texture.

XV. About this time twelve-month, I found, accidentally, a paper of melon ſeeds that I had laid by, with the date of the year 1710 upon it. I ſowed ſome of them, not with any great hopes of their coming up; but, to my great ſurprize, I had a fine number of plants from them, which all proſpered very well, till they had put out 4 leaves, when they were all loſt by an accident. This I have mentioned to you, becauſe Mr *Triewald* has given an account of ſome old melon-ſeeds that produced fruit, though they exceeded mine 10 years in age: however mine may be a confirmation of their long retention of their vegetative quality; which I ſuppoſe may be aſcribed to the oilyneſs of the ſeed, and the hardneſs of it's outward coat.

Concerning the vegetation of Melon *Seeds 33 years old; by the late* Roger Gale, *Eſq; F. R. S. to Mr* P. Collinſon, *F. R. S.* N°. 475. p. 265. Jan. &c. 1745. *dated* Scruton *in* Yorkſhire, Jan. 14. 1743-4. *Read* Jan. 24. 1744-5.

XVI. I was poſſeſſed of ſeveral good microſcopes, both ſimple and compound. The compound microſcope was that portable reflecting one without a micrometer, of Mr *Benj. Martin*'s invention, which I brought with me from *England*. Firſt of all I expoſed to the luminous *focus* of this inſtrument a very ſmall, pure, thin bit of the bark of this root ſcraped off very carefully. It's external ſurface appeared almoſt opake, very uneven, of the colour of dry earth, as it appears to the naked eye; but the inner ſurface viewed in the ſame manner diſcovered a confuſed heap of very ſhort and thin little maſſes, every where interrupted, and moſtly acute, and angular, in the ſmalleſt viſible atoms, chiefly purple, almoſt tranſparent, like ſome reſin, but connected by interſperſed threads of a like figure, variegated, but chiefly whitiſh; but at the edge there appeared real prickles; and the whole contexture ſeemed like that which in the animal œconomy Anatomiſts deſcribe in the fat, when they ſay this oil is lodged in cells, which cohere with that lanuginous ſoft cellular ſubſtance. Thus alſo this purple ſubſtance in form of reſin ſeemed to be mixt with the whitiſh threads. And thus, in many repeated obſervations, both by ſimple and compound microſcopes, this bark always appeared to me, with this only difference, that the reflecting one repreſented every particular more diſtinctly than the reſt.

Microſcopical obſervations on the root of Ipecacuanha; *by* P. Fred. Gmelin, *Med. Lic.* Wurtenberg. N°. 476. p. 382. Apr. &c. 1745. *Preſented* May 9. 1745.

But I had not yet examined a nerve of this root, which was very brittle and deprived of it's bark, and made the greateſt bulk of the root, tho'

tho' it ſeemed to yield much to the bark in weight, as being very dry, by the compound microſcope: but to the naked eye it ſeemed to be ſprinkled with dark purple ſpots. I was deſirous to know what appearance theſe would have under the *focus* of a microſcope; and therefore viewed that little bit, which had the ſpots. The nerve itſelf appeared equally convex, and cylindrical, conſiſting of uninterrupted longitudinal fibres, cloſely applied to each other, very thin, and very white: but the ſpots, as they appeared to the naked eye, when ſubmitted to the microſcope, were found to be tranſparent dark-red little maſſes raiſed above the convexity of the cylinder: but I was in doubt whether I ſhould believe that theſe little maſſes did not riſe above the nerve, or determine that they inhered in the nerve itſelf, and belonged to it's ſubſtance. However on the application of ſome ſimple microſcopes, I was convinced that they were raiſed above the nerve, and belonged not ſo much to the nerve as to the bark.

From theſe ſeveral microſcopical obſervations I conclude *à priori*, that the bark muſt be more efficacious than the whole root, eſpecially if we call in that indubitable and ſo often confirmed practical experience, that the purging principle of vegetables reſides chiefly in their reſinous part. Beſides, from my obſervation, that the appearance of the whole bark by the microſcope, which ſhews it to be compoſed of many ſmall, ſharp, ſhort *ſpicula*, that *hypotheſis* will alſo be explained, which mechanically explains the force of purging and vomiting medicines, by ſuppoſing ſuch *ſpicula*, like ſo many little wedges (or perhaps prickles, ſuch as appear even to the naked eye in nettles, which ſenſibly irritate a living human body) which by the periſtaltic motion of the ſtomach, tho' mediately applied to it's muſcular coat, and being frequently toſſed about by this perpetual motion, ſtimulate the moving fibres, and excite them to a ſtronger contraction than ordinary: though at the ſame time I would not yet aſſert this *hypotheſis*, how probable ſoever it may appear from theſe microſcopical obſervations.

The Bark prevents catching cold; in a letter from the Rev. Dr Sam. Salter *to Mr* Arderon. N°. 478. p. 3. Jan. & Feb. 1746. *Read* Jan. 9. 1745-6.

XVII. Dr *Salter*, one of the prebends of *Norwich* cathedral, writes, in a letter to Mr *Arderon*, that he thought it might be of ſervice to take notice of the following effect of the *Jeſuit's Bark*: the doctor uſed to be ſubject eaſily to take cold, and, in conſequence thereof, to be ſubject to have a ſore throat to a very great degree; but the laſt time, above 15 years ago, after his recovery, he was adviſed, by Sir *Benj. Wrench*, to take ℥ii of the *Bark* (after due preparation by bleeding, or purging, or both, when he was altogether without complaint) every ſpring and fall. This, he ſaid, would more effectually guard him againſt taking cold; which he has found ſo far to anſwer, that he is now able to go 500 miles with leſs hazard of cold, than he could go 20 before; and he has never had what he can ſtrictly call a ſore throat ſince.

XVIII.

Pla. XXII. *Vol.* X. *Part* II. *Pag.* 762.

Fig. 113.

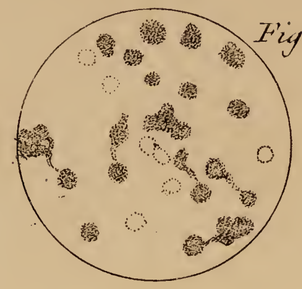


Fig. 114.

Fig. 115.

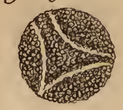


Fig. 116.

Fig. 117.

Fig. 118.

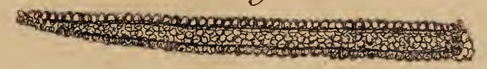


Fig. 123.

Fig. 122.

Fig. 121.

Fig. 119.

Fig. 120.

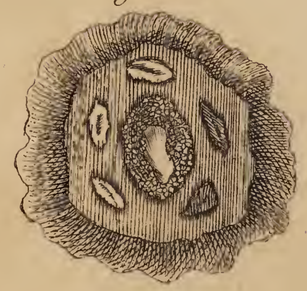


Fig. 124.

Fig. 130.

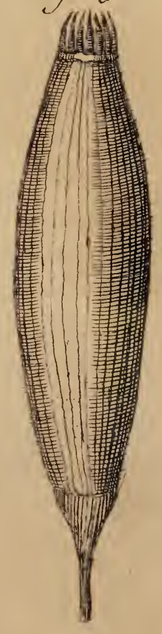


Fig. 131.

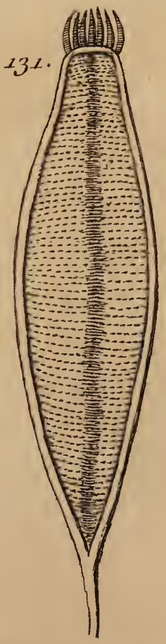


Fig. 125.

Fig. 127.

Fig. 126.

Fig. 128.

Fig. 129.

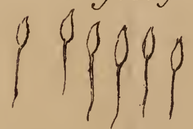
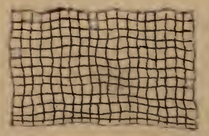


Fig. 132.

Fig. 133.



XVIII. Notwithſtanding the number of inſtances, which occur among writers, concerning the poiſonous quality of our common hemlock, or *cicuta major C. B*; ſuch as, that of *Cardanus* mentioning a man killed by a cake, wherein this plant was an ingredient; that of *Braſſavola*, who aſſures us, that it is mortal not to men only, but to geeſe and ſwine; as well as thoſe of *Matthiolus*, *Scaliger*, *Kircher*, *Boccone*, and others; yet the fatality of it's poiſon, when growing in this kingdom, has been doubted by many; inaſmuch as that faithful collector the late Mr *Ray* mentions, *Syn. Ed.* 2. *p.* 326. that not only his friend Mr *Petiver* eat half an ounce of the root of this plant, but that Mr *Henly*, a friend of Mr *Petiver*'s, in his preſence, eat, without any inconvenience, 3 or 4 ounces of the ſame root. From hence it has been thought, either that the root has effects different from the ſtalks and leaves, or, that difference of climate varies the degree of the violence of the poiſon.

Concerning ſome perſons being poiſoned *by eating boiled* Hemlock; *by Mr* William Watſon, *Apothecary, F. R. S.* N°. 473. p. 18. May &c. 1744. *Read* May 10. 1744.

An obſervation indeed of the ſame kind occurs in the *German Ephemerides*. *Linnæus*, *Hort. Cliff.* makes alſo ſome doubt concerning the malignity of this plant; and, in naming it, has kept to the old appellation of *Theophraſtus* and *Dioſcorides*, *Conium*; and has transferred that of *Cicuta*, to the *Cicuta aquatica* of *Geſner*, and of *Wepfer*. Beſides, many of the accidents, ſaid to have proceeded from *cicuta* or hemlock, have been occaſioned by different plants; ſome of the accidents, probably, from the common one, but many more from the *cicuta aquatica* before-mentioned, and from the *Oenanthe ſucco viroſo, cicutæ facie*, *Lob.* This confuſion appears manifeſtly in ſeveral authors, and ſome of them of the greateſt credit. Which of theſe plants, or whether any of them, was the *Athenian* poiſon, nobody has determined.

Although the eating of the roots, as abovementioned, was attended with no bad conſequences, a late melancholy accident has been ſufficiently convincing of the poiſonous quality of the leaves of the *cicuta major*.

On *Sunday*, *May* 6. 1744. two of the *Dutch* ſoldiers lately arrived, who were quartered at *Waltham-Abbey* in *Eſſex*, collected, in the fields adjoining, a quantity of herbs, ſufficient for themſelves and two others for dinner, when boiled with bacon. Theſe herbs were accordingly dreſſed, and the poor men firſt eat of the broth with bread, and afterwards eat the herbs with the bacon. In a ſhort time after, they were all ſeized with violent *vertigo's*; they ſoon after were *comatoſe*; and two of them grew convulſed, and died in about 3 hours.

The people of the town being exceedingly alarmed at this accident, Dr *Barrowby*, jun. being there, immediately went, and ordered the other two, at that time almoſt dead, large quantities of oil; by which means they threw up moſt of what they had eaten, and afterwards grew better. In all of them the effects were the ſame as thoſe from a large doſe of *opium*.

The next day, being at the place, I ſaw one of theſe men much recovered, and only complaining of a heavineſs in his head; but the other was ſo well, as to be gone to perform exerciſe with the other ſoldiers. There was a fifth ſoldier, whom I ſaw, who told me, he eat ſome of the bread out of the broth, but felt ſcarce any inconvenience therefrom. It ſo happened, that the two men, who gathered the herbs, were both killed.

As I went down to the place to ſatisfy myſelf in this matter, a *Dutch* officer went with me very courteouſly to an inn, where there were two other ſoldiers, who had ſeen and knew the herbs which had been eaten: he was ſo kind alſo as to attend me with theſe ſoldiers into the fields, to ſhew me the plants growing. They firſt gathered me the *cicutaria vulgaris J. B.* or cow-weed; then, the *myrrhis ſylveſtris ſeminibus aſperis C. B.* or ſmall hemlock-chervil. They then gave me ſome *cicuta major*, and, ſmelling it, immediately ſaid, That this was the herb that killed their comrades; which I then had no reaſon to doubt of; as of the two former plants, the firſt grows almoſt under every hedge, and is eaten by the cows, and the other is frequently given to tame rabbets for food; whereas cattle conſtantly refuſe to eat hemlock.

Before I was thus ſatisfied, I imagined this accident to have proceeded rather from *Lobel*'s *Oenanthe*; thinking, that as that plant grows near the ſides of rivers, theſe ſoldiers might have gathered it by the river *Lee*, which runs by the town, and eaten it for ſmallage, to which it has ſome reſemblance.

It is now known, that the *cicuta major*, the *cicuta aquatica*, and the *oenanthe* of *Lobel*, are certain poiſons; but there are two others of the ſame claſs, growing common in *England*, and not much unlike theſe in ſmell and other circumſtances, vehemently to be ſuſpected: the one is the *cicutaria tenuifolia* of Mr *Ray*, which grows frequently in waſte places, and in gardens among pot-herbs, of which *De la Champ* gives ſome account of it's malignancy; the other is the *cicutaria paluſtris* of *Lobel* and *Tabernæmontanus*, or *phellandrium* of *Dodonæus*, which grows in muddy ditches and ponds.

I don't remember any hiſtory of the pernicious effects of the *cicuta major* in this kingdom; but as the detecting poiſonous plants is of very great conſequence, I preſume to lay this paper before you.

Critical obſervations concerning the Oenanthe aquat. ſucco viroſo crocante Lob. *by the ſame; occaſioned by an extract of a letter from*

XIX. "Eleven *French* priſoners had the liberty of walking in and "about the town of *Pembroke*: 3 of them, being in the fields a little "before noon, found and dug up a large quantity of a plant with it's "roots (which they took to be wild *celeri*) to eat with their bread and "butter for dinner. After waſhing it, while yet in the fields, they all "3 eat, or rather taſted, of the roots.

"As they were entering the town, without any previous notice of "ſickneſs at the ſtomach or diſorder in the head, one of them was "ſeized with convulſions. The other two ran home, and ſent a ſur-

"geon

" geon to him. The ſurgeon endeavoured firſt to bleed, and then " vomit him; but thoſe endeavours were fruitleſs, and he died pre- " ſently.

" Ignorant yet of the cauſe of their comrade's death, and of their " own danger, they gave of theſe roots to the other eight priſoners, " who all eat ſome of them with their dinner. I cannot learn exactly " how much they eat, they being ſent away a little time before your " letter arrived.

" A few minutes after, the remaining two, who gathered the plants, " were ſeized in the ſame manner as the firſt; of which one died: the " other was bled, and a vomit with great difficulty forced down, on " account of his jaws being, as it were, locked together. This ope- " rated, and he recovered; but was ſome time much affected with a " dizzineſs in his head, though not ſick, or in the leaſt diſordered in " his ſtomach. The other 8, being bled and vomited immediately, " were ſoon well.

" There were in theſe men none of thoſe comatoſe ſymptoms you " mentioned * to have happened to the *Dutch* ſoldiers, who were poi- " ſoned by eating the *cicuta major*.

" As I was not preſent myſelf, I ſend you the beſt information I was " able to procure. After I had done examining, I ordered ſome of the " herb and root to be brought me. As you ſuggeſted in your letter, " I found it to be the *oenanthe aquatica cicutæ facie* of *Lobel*, which " grows in great plenty all over this country, is called by the inhabi- " tants *five-fingered root*, and is much uſed by them in cataplaſms for " the fellon, or worſt kind of whitflow. The *Frenchmen* eat only the " root, and none of the leaves or ſtalk. — I muſt beg your pardon for " ſending you this imperfect account: had this accident happened at " *Haverford*, you ſhould have had one more exact."

Mr George Howel, *ſurgeon, at* Haverford-weſt, *to the author, giving an account of the poiſonous effects of this plant to ſome* French Priſoners *at* Pembroke. N°. 480. p. 227. May & June 1746. *Read* June 12. 1746.

So far Mr *Howell*'s letter.

The poiſonous effects of this plant, in the inſtance beforementioned, exactly ſquare with thoſe mentioned of the ſame plant, in N°. 238. of the *Phil. Tranſ.* where 8 young lads, near *Clonmel* in *Ireland* (where this plant is called *tahow*) miſtook it's roots for thoſe of *ſium aquaticum*, or water-parſnep, and eat plentifully of them. About 4 or 5 hours after, going home, the eldeſt, almoſt of man's ſtature, without the leaſt previous diſorder or complaint, fell down backwards, and died convulſed. Four more died in the ſame manner before morning; not one of them having ſpoken a word from the moment the venomous particles had attacked the *genus nervoſum*. Of the other 3, one ran ſtark-mad, but came to himſelf next morning. The hair and nails of another fell off. One of them only eſcaped without any harm, who ran home above 2 miles, and drank warm milk, which cauſed a *diaphoreſis*. A *Dutchman* likewiſe was poiſoned with the leaves of this plant, boiled in his

* See the preceding article.

pottage;

pottage ; which he took for ſmallage, and to which it's leaves have great reſemblance.

Dr *Allen*, in his *Synopſis Medicinæ*, mentions an inſtance of 4 children, who eat of theſe roots. They indeed were in great agonies, before they fell into convulſions In their fits they vomited, which was encouraged by large draughts of oil and warm water ; and by other proper care they all did well. He takes notice likewiſe of a pig's dying in convulſions, from eating ſome of theſe roots, which it had grubbed up.

Stalpart van der Wiel, in his obſervations, takes notice of the deadly effects to two perſons, who had eaten theſe roots, miſtaking them for *Macedonian* parſley. Theſe men (like thoſe quoted from Dr *Allen)*, ſoon after eating theſe roots, were troubled with violent heats in the throat and ſtomach, attended with a *vertigo*, ſickneſs at the ſtomach, and purging. One of them bled at the noſe ; the other was violently convulſed. Both of them died ; one in 2 hours, the other in 3. This author has given us 3 figures of the *oenanthe :* the 2 tables of the roots and the leaves are tolerably well executed ; but that expreſſing the whole plant is very deficient. It were much to be wiſhed, that all botanical authors had induſtry and ingenuity enough to delineate their own tables, as *Columna* and *Dillenius* have done ; which will always heighten the value of their otherwiſe excellent works.

It is very remarkable, that neither the *French* priſoners, who were killed at *Pembroke*, nor thoſe before cited in the *Phil. Tranſ.* felt any heat or diſorder in their ſtomach, before the attack of the convulſive paroxyſms : whereas theſe mentioned by Dr *Allen*, and *Stalpart van der Wiel*, were in great agonies, from the violent heat in their ſtomach and throat, before they were attacked by convulſions.

The ſame variety of ſymptoms we meet with in *Wepfer*, with regard to thoſe people who were poiſoned by the *cicuta aquatica* ; where ſome of them, who had eaten the roots of this plant at the ſame time, ſtood and aſſiſted their friends, till they died of convulſions, without feeling themſelves any wiſe diſordered ; and afterwards, in their turns, died in the ſame manner. Others were violently affected by it, as ſoon almoſt as they had eaten it. Confer *Wepfer*'s Hiſtory with the * *German Ephemerides*. *Linnæus* mentions, in the † *Flora Lapponica*, the great ſlaughter, and miſerable manner, in which the horned cattle died, from eating this plant at *Tornæa*. This author alſo, in his *Flora Suecica*, acquaints us (notwithſtanding *Rivinus* and *Mappus* have aſſerted, that the horned cattle not only eat this plant without detriment, but are very fond of it) that three oxen were killed by eating the roots thereof. He was fully convinced that they were the roots of the *cicuta aquatica* ; becauſe, ſoon after this accident, the country people brought him ſome of them, deſiring to know to what plant they belonged. He thereupon

* *Ephemerid.* Natur. Curioſor. Dec. 2. Ann. 6. Obſ. 116.
† See *Flor. Lappon.* p. 72.

planted

planted them in the academical garden, and was fully ſatisfied what they were.

Wepfer has confounded his *cicuta aquatica*, in the Hiſtory thereof *, with the poiſonous *oenanthe* of *Lobel*; where he ſays, that *Lobel* has deſcribed the *cicuta aquatica* under the name of *Oenanthe cicutæ facie, ſucco viroſo crocante*; and mentions, that it is not very frequent, but in the northern parts of *England* by the ſides of rivers, and in watery places: he adds, that *Lobel* has not been exact in his deſcription. To which I anſwer, that *Lobel*'s deſcription of the *oenanthe* is very exact, for the time he lived; and it is very evident, that *Wepfer* never ſaw this *oenanthe*; which plant, I believe, is not found in *Germany*. *Wepfer* likewiſe, in the *Ephem. Nat. Curioſ.* † is under the ſame miſtake; and tells you, that *Stalpart van der Wiel* differs from him; and calls the plant, mentioned in his Obſervations, *Oenanthe*, as *Lobel* does: and though *Stalpart* has given figures of the plant accurate enough for a common obſerver to diſtinguiſh the plants by, and tho' nine years lapſed between the publication of his book de *Cicuta* and his Obſervations in the *Ephemerides*, he was ſtill in the ſame error; and believed the *oenanthe* of *Lobel*, and his *cicuta aquatica*, as well as that of *Geſner*, to be the ſame poiſonous plant. The accurate *Hoffman* ‖ alſo, when treating of vegetable poiſons, makes no mention of this difference.

Neither the roots of the *oenanthe* of *Lobel*, nor thoſe of the *cicuta* of *Wepfer*, have any flavour in them diſagreeable enough to deter thoſe, who taſte them, from eating. They both occaſion violent convulſions, and death, if not timely prevented. The intention of cure ſeems in both to be the ſame; *viz.* firſt, by emptying the ſtomach and inteſtines as ſoon as poſſible, and then by cauſing the patient to ſwallow large quantities of oleaginous fluids. But it is to be obſerved, that the cauſing the patient to ſwallow any quantity is attended with great difficulty, after he is attacked by the poiſon; becauſe of the jaws being, as it were, locked together by the violence of the ſpaſm. After the ſtomach is freed from this pernicious vegetable, the ſymptoms have generally diminiſhed by degrees, and the patient recovered.

* *Cicutæ aquat.* hiſtoria & noxæ, p. 15.

† Dec. II. Ann. VI. Obſ. 116.

‖ *Fred. Hoffman.* Medicin. Rational. Syſtematic. *Tom.* II. p. 174. *Edit. in 4to.* "*Ex* "*vegetabilium regno inter præſentiſſima venena referri debeat cicuta vera, napellus ſive* "*aconitum cæruleum, ſolanum furioſum, hyoſcyamus, ac datura.*" If here the epithet *vera* to *cicuta* is underſtood only to point out the poiſonous ſort of hemlock, there are no leſs than three ſpecies of this claſs, which, from their being known certainly to be poiſonous, may lay claim thereto; *viz. Cicuta major C. B. Cicuta aquatica Wepf.* and *Oenanthe cicutæ facie Lob.* But, it is very probable, the two laſt were unknown to the Ancients. The deſcription of *Dioſcorides*, lib. iv. cap. 79, which is the only one to be met with among the *Greek* writers, and that but obſcure, relates, in my opinion to the firſt of theſe. Κώνειον καυλὸν ἀνίσι γονατώδη ὡς μάραθρον μέγαν. Φύλλα ὃ νάρθηκι ἐμφερῆ, τενώτερα ὃ καὶ βαρύοσμα· ἐπ' ἄκρων δὲ ἀποφύσεις, καὶ σκιάδια, ἄνθος ὑπόλευκον· σπέρμα ἐμφερὲς ἀνίσῳ, λευκότερον. ῥίζα κοίλη καὶ ἐ βαθεῖα. *Pliny*'s Deſcription, lib. xxv. cap. *ult.* is taken from this of *Dioſcorides*.

Threlkeld,

Threlkeld, in his *Synopſis Plantarum*, mentions, that he has ſeen great plenty of this *oenanthe* in *Cumberland*, where the country-people call it *Dead Tongue*, and uſe it, when boiled like a pultice, to the galled backs of their horſes.

Neither the *German* Botaniſts *, nor *Haller* in his *Enumeratio Stirpium Helvetiæ*, mention this plant as growing amonſt them. I believe, therefore, it is ſeldom met with but in *Holland*, *England*, and in ſome parts of *France*; for *Moriſon* mentions it growing in *Bretagne* near the mouth of the river *Loire*. This plant was communicated to *Matthiolus* by a Profeſſor of Phyſic at *Padua*. (See *Matth. p.* 628.) *Linnæus*, in the *Flora Suecica*, ſays, that he received it from a correſpondent, who gathered it in *Scania*.

Lobel, and after him *John Bauhin* and others, take notice of this plant's growing in the northern parts of *England*. It grows alſo in the weſtern and ſouthern parts, by the ſides of rivers, large waters, and ſometimes by ponds. It grows near *Bath*. Dr *Allen* mentions it growing within 3 miles of *Bridgewater*. It's being produced in *Wales*, is the occaſion of this paper. I have ſeen it very frequently by the ſides of the river *Thames*, both above and below *London*. I have found it likewiſe by the ſide of a large pond near the road, in the town of *Dulwich*, not far north of the college; likewiſe by the ſides of a large water near the mills, half a mile S. E. of *Dartford* in *Kent*.

Lobel is the firſt, who has given a ſmall figure and a tolerable deſcription of this *oenanthe*, in his *Adverſaria Plantarum* †. He has likewiſe repreſented it in the 730th of his *Icones*. This ſeems likewiſe to be the plant deſcribed by *Valerius Cordus* ‖, under the denomination of *olſenichium*; and, by *Dodonæus*, under that of *apium ſylveſtre, ſive thyſſelium* §; where the deſcription, place of growth, and form of the roots, agree exactly with the plant under conſideration; tho' his figure is execrably bad. This bad figure is copied, and the deſcription tranſlated, by *Gerard* ** in his Herbal, without making any mention of *Dodonæus*. This figure is likewiſe copied in *Parkinſon*'s Theatre of Plants. *John Bauhin*, *Matthiolus*, *Gerard*, *Parkinſon*, and *Moriſon*, have given us figures of this *oenanthe*; but theſe repreſentations give us ſcarcely any other idea of the plant, than that it is an umbelliferous one, with roots divided like thoſe of *Aſphodel*. Of theſe, however, *Moriſon*'s †† is the beſt; and his deſcription, in his Book *de Umbelliferis*, is very exact and copious. Mr *Ray*'s deſcription is taken from *Lobel*. I have at the bottom of the ‖‖‖ page recited the various *ſynonyma*, under which this plant is mentioned amongſt authors.

As

* Unleſs the *olſenichium* of *Valerius Cordus*, and *thyſſelium* of *Dodonæus*, hereafter mentioned, are other names of the plant in queſtion.

† Adverſaria Plant. nov. 326. ‖ *Valer. Cord.* p. 149. § *Dodon.* Pempt. 687.

** *Gerard.* Emac. 1020. †† *Moriſon* Umbel.

‖‖‖ OENANTHES, *de qua hic agitur, ſynomyma.* Oenanthe tertia *Matthioli*, p. 629.

Oenanthe,

As it appears, from what I have laid down, that the *oenanthe* of *Lobel*, and *cicuta aquatica* of *Wepfer*, have not been ſufficiently diſtinguiſhed by medical writers hitherto, I hope I ſhall ſtand excuſed for making a few obſervations upon this laſt. This, though a plant frequently met with upon the Continent, and very well deſcribed by botanical writers, we ſeldom find near *London*; but it grows in many parts of *England* by the ſides of large ſtanding pools, and near the banks of fens. I am informed by *Robert More*, Eſq; an excellent Botaniſt, and a very worthy Member of this *Society*, that it grows plentifully in many parts of *Shropſhire*. I have lately received it from Dr *Wilmer*, who gathered it by the ſides of the river *Colne*, not far from *Uxbridge*. It is mentioned by Mr *Ray* to grow near *Brereton*-Mere in *Cheſhire*, and in ſeveral other places. You find it mentioned by *Geſner* *; and *Wepfer*, in his Hiſtory thereof, has given us four tables of different parts ſufficiently accurate. It is figured and deſcribed by *John Bauhin* †. *Lobel*'s *Icon.* 208. relates to this plant. *Dodonæus*'s figure, which is not a bad one for the time, is copied both by *Gerard* and *Parkinſon*. *Moriſon* has given us two figures thereof, one in his general Hiſtory, the other in his Book *de Umbelliferis*, though under different names. But the moſt elegant and deſcriptive figures are thoſe of the *Hortus Eyſtettenſis* and *Rivinus*. As the *ſynonyma* of this plant are very many, and very different, I have inſerted them at the bottom of the page ‖.

Though the medical writers have not ſufficiently diſtinguiſhed theſe plants, the Botaniſts have. Theſe indeed, in their turns, have been as

Oenanthe, ſucco viroſo, Cicutæ facie *Lobelii*. *J. B.* III. p. 193.
Oenanthe, Chærophylli foliis. *C. B. P.* 162.
Filipendula, Cicutæ facie. *Ger. Emac.* 1059.
Oenanthe, Cicutæ facie *Lobelii*. *Park.* 894.
Oenanthe maxima, ſucco viroſo, Cicutæ facie. *Moriſ.* Hiſt Sect. 9. Tab. 9.
Oenanthe, foliis omnibus multifidis obtuſis, fere æqualibus. Hort. *Cliff.* 99. *Royen.* 107.
Engliſh wild Parſley. *Ger.* 1020. and Hemlock Dropwort, p. 1059.

* *Geſner* Hort. 254. † *J. B.* III. 175.

‖ CICUTÆ aquaticæ *ſynonyma*.
Cicuta aquatica. *Geſn. Hort* 254. *Wepfer. Linnæi* Flor. Lap. 103.
Cicuta maxima quorundam. *Hort. Eyſtet.*
Cicuta. *Linn. Hort. Cliff.* 100.
Cicutaria. *Riv.* Tab. 76.
Sium alterum. *Dod. Pempt.* 579.
Sium alterum Oluſatri facie. *Lob. Ic.* 208. *Ger. Em.* 256. *Raii Hiſt.* 450.
Sium Erucæ folio. *C. B. P.* 154.
Sium majus anguſtifolium. *Park.* 1241.
Sium foliis rugoſis trifidis, ſeu multifidis dentatis. *Mor.* Umb. 63. Tab. 5.
Sium, pinnis laciniatis, pinnulis trifidis, nervo non folioſo. *Haller. Helv.* 436.

It is called in Engliſh *Long-leaved Water-Creſſes* by *Gerard*, p. 256. very injudiciouſly; and *Water Parſneppe* by *Parkinſon*, p. 1241. but much better named by Mr *Ray*, Long-leaved Water Hemlock.

negligent, when writing concerning their uſes (1). So that, notwithſtanding *Lobel* long ago (2) informed the world, that the *oenanthe cicutæ facie*, in it's effects, was very like hemlock; and that thoſe, who had eaten it in ſallads, were almoſt killed by it; this plant occaſioning *vertigo's*, and other violent ſymptoms; yet *Moriſon*, in his Treatiſe of umbelliferous plants, though very exact in the deſcription of the ſpecies of which we are now treating, recommends indiſcriminately the whole *genus*, as being temperately warm and dry; that they are uſeful in cleanſing the urinary paſſages, and in opening obſtructions: he quotes the authority of *Dioſcorides* for giving the powdered roots in wine to cure the dyſury, and to help aſthmatic complaints. What the *oenanthe* of *Dioſcorides* was (3), nobody has determined. He deſcribes it, as having leaves like parſneps, white flowers, a thick ſtalk about a ſpan high, ſeeds like thoſe of arrach, a large root divided into ſeveral round heads, and that it grows in rocky places. A ſhort account of the *oenanthe*, together with it's uſes in medicine, is taken from *Dioſcorides* by *Pliny* (4) the Naturaliſt. What the plant was that *Dioſcorides* here recommends, is uncertain: none of the ſpecies we are acquainted with come near this deſcription; all thoſe, that we know, much exceed his meaſure; none of them have leaves like parſneps, and all grow in watery places. *Ruellius* (5), *Fuchſius* (6), *Tragus* (7), *Dodonæus* (8), and *Matthiolus* (9), have given us the *filipendula* or *dropwort*, for the *oenanthe* of *Dioſcorides*: but this cannot be that plant, becauſe it's ſeeds are not like thoſe of *arrach*; neither has it a large root divided into many heads. *Parkinſon* (10), no great favourer of *Lobel*, ſays, that "*Lobel* only brandeth his *oenanthe cicutæ facie* to be virulent and ve- "nomous, from the relations of the north country people, where he "ſays it chiefly grows." Mr *Ray*, in his Hiſtory, though he has tranſcribed *Lobel*'s Deſcription, in which it's venomous qualities are taken notice of, leaves this matter to further examination, other Botaniſts being of a different opinion.

The inſtances mentioned in theſe papers are but too ſufficient teſtimonies of the malignant properties of this plant; but Mr *Miller*, a worthy Member of this *Society*, informed me further, that, not many years ſince, a whole family were poiſoned therewith at *Batterſea*. As this plant is frequent ſo near us, and as it's appearance and ſmell are ſo like *ſmallage* and *celeri*, we are greatly intereſted that the knowledge of it be extended as much as poſſible. As I find no good repreſentation thereof among authors, and as a good figure conveys a ſtronger idea to the generality of readers than the moſt accurate deſcription, I have procured

(1) *Matthiolus*, ſpeaking of the *oenanthe*, ſays, p. 628. *Putamus tamen a cæteris filipendulis non multum differre.* (2) *Lobel*'s *Adverſaria* were publiſhed in 1572.
(3) Οἰνάνθη τὰ μὲν φύλλα ἔχει ὥσπερ σαφυλῖνος, ἄνθη ὑ λευκά. Κάι καυλὸν πάχυν σπιθαμιαῖον· καρπὸν δε ὥσπερ ἀτραφάξιος· ῥίζαν μεγάλην κεφαλὰς ἔχουσαν πλείονας στρογγύλας· φύεται ἐν πέτραις· *Dioſcorid.* lib. iii. cap. 135. (4) *Plinii* Nat. Hiſt. lib. xxi. cap 24. (5) Pag. 265. (6) *Fuchſii* Hiſt. 563. (7) *Trag.* p. 883. (8) *Dodon.* Pempt. 56. (9) Pag. 627. (10) *Park.* Theat. 895.

that

that admirable artiſt Mr *Ehret* to draw not only this plant, but alſo the *cicuta aquatica* of *Wepfer*; that they may be the more eaſily known from all plants, and diſtinguiſhed from each other by their being both ſeen at one view. From theſe drawings the ingenious Mr *Mynde* has very accurately engraved the figures hereunto annexed.

P. S. I am informed by Mr *Ehret*, that, in drawing the *oenanthe*, which he has executed with his uſual elegance and accuracy, he was obliged to have a quantity of it placed before him upon a table; when, the room being ſmall, the *effluvia* thereof cauſed in him an univerſal uneaſineſs, with a *vertigo*; ſo that he was conſtrained to have it removed, and never after place before him but a ſmall piece at a time.

There is ſomething in the formation of the root of the *cicuta aquatica* before-mentioned, deſerving particular notice. This plant generally grows either near the ſides of large ſtagnant waters, or in ſhallow rivers, whoſe ſtreams are ſlow. Towards the end of autumn, or the beginning of winter, the root for the ſucceeding ſummer is formed out of the lower part of the ſtalk. Out of the crown of this root are then ſeen the rudiments of the leaves of next year (*ſee Fig.* 136. *a.*) Fig. 136. and from the ſides of this grow the crowns of ſeveral ſmaller roots. This root, in it's whole length, is divided tranſverſly into a number of large unequal cells (*ſee Fig.* 137.) correſponding with the par- Fig. 137. titions, which divide theſe cells, the ſurface of the root is marked circularly with little round depreſſions. So great a part of this root is occupied by the cells, that it becomes ſpecifically lighter than water; ſo that, in winter, upon the increaſe of water in the rivers and pools, this root, as well that part intended for the ſucceeding ſummer, as that which furniſhed the plant the preceding, is buoyed up. The old root then rots, and floats upon the ſurface of the water with the new one all the winter (*ſee Fig.* 136. *b.*); and in rivers theſe are frequently carried to very great diſtances from the places of their growth. In the ſpring the old root is waſhed away; and the new one, upon it's coming near the ſoil, ſends out from the circles before-mentioned, particularly from thoſe neareſt the bottom, a great number of long ſlender white fibres, by which this root becomes again fixed to the ſoil, propagates it's ſpecies, and remains thus, until, by the rotting of theſe fibres, it is again weighed up. The old root decaying, and being waſhed from the new, is the cauſe of that truncated appearance we obſerve in the root of the figures of *Dodonæus*, *Parkinſon*, and *Moriſon*, who have exhibited this plant in a flowering ſtate. This proviſion of cells in the root ſeems to be given to this plant by nature, that, as great part of it's root is apt to periſh in winter, vegetation might not be prevented, nor the root deſtroyed, unleſs the whole number of cells are ſpoiled, which very rarely happens.

Explanation of Fig. 134. *exhibiting the* Oenanthe Cicutæ facie.

a, It's tuberoſe roots ſurrounding the ſtalk. *b*, *b*, *b*, *b*, A leaf taken from near the bottom of the ſtalk. *c*, A branch with the umbels of flowers in different ſtates. *d*, An anterior view of the flower of it's natural ſize. *e*, A poſterior view of the ſame. *f*, The anterior appearance of the flower through a miſcroſcope. *g*, The poſterior view of the ſame. *h*, A view of the rudiments of the fruit after the decay of the flower. *i*, The ſame magnified.

Explanation of Fig. 135. *repreſenting the* Cicuta aquatica *of* Wepfer.

a, A branch of this plant with it's umbels of flowers in different ſtates. *b*, The appearance of the bottom of the ſtem, growing from the crown of the old root. *c*, An anterior view of the flower of it's natural ſize. *d*, An anterior view of the ſame magnified. *e*, A poſterior view of the flower magnified. *f*, The *Vaſculum ſeminale*, and ſeed. *g*, The ſame magnified.

Explanation of Fig. 136, and 137. *repreſenting the root of the* Cicuta aquatica *in Winter.*

Fig. 136. *a*, The rudiments of the leaves. *b*, The old rotten root not yet ſeparated from the new one of the preceding ſummer.

Fig. 137. A longitudinal ſection of the root exhibiting the cells.

An account of the poiſonous root *lately found mixed among the* Gentian; *by* Rich. Brockleſby, *M. D. F. R. S.* N°. 486. p. 240. Feb. & Mar 1748. *Read* March 17. 1747.

XX. The following account is the beſt I have received of the poiſonous effects of a noxious root, lately found in a parcel of gentian, and exhibited for uſe to ſeveral perſons inſtead of it. And as it is attended with ſuch dangerous conſequences, I thought even an imperfect relation of facts had better be given immediately, than to expect more circumſtances, and wait ſo long for them, till greater miſchiefs might happen, by the inattention of ſuch as are conſtantly adminiſtring medicines. The following account was ſent by a gentleman of *Hambleden* pariſh, *Buckinghamſhire*; and is found to agree in general with ſome other fatal inſtances that have happened ſince in *London*.

Mary Burgeſs, aged 60 years, about 5 in the morning, drank of an infuſion of only one pennyworth (without other ingredients) of ſuppoſed gentian-root, in $\frac{1}{2}$ a pint of white wine: it is uncertain what preciſe quantity ſhe took; but in 2 hours afterwards ſhe faltered in her ſpeech, had twitchings and convulſions of her hands ſo far, that the ignorant by-ſtanders alledged the poor woman was drunk; and ſo left her in bed till 12 to ſleep it out. On their return however ſhe appeared much worſe, was ſpeechleſs, and remained ſo 3 whole days, and did not know any body all that time. In her illneſs a purging came on, and at laſt carried her off.

Katharine Woodward, aged 44, took about a tea-ſpoonful of the ſame wine, and ſoon after fell down ſpeechleſs, and her limbs were paralytic near 36 hours: after that ſhe recovered her ſpeech, but continued ill above a fortnight, and part of that time her under jaw was convulſed, and ſhe bled both at mouth and noſe, in the beginning.

Mary Diggins, aged 33 years, taſted a much leſs quantity of the ſame wine than the former had done; and though terrified at her neighbour's bad ſymptoms, ſhe drank warm water with oil, in order to vomit; yet ſhe

Fig. 134.

***OENANTHE**, Cicutæ facie, succo viroso crocante.* Lob. adv. 326.

Fig. 135.

CICUTA Aquatica Wepf. *Sium alterum Olusatri facie* Loh. Icon 208.

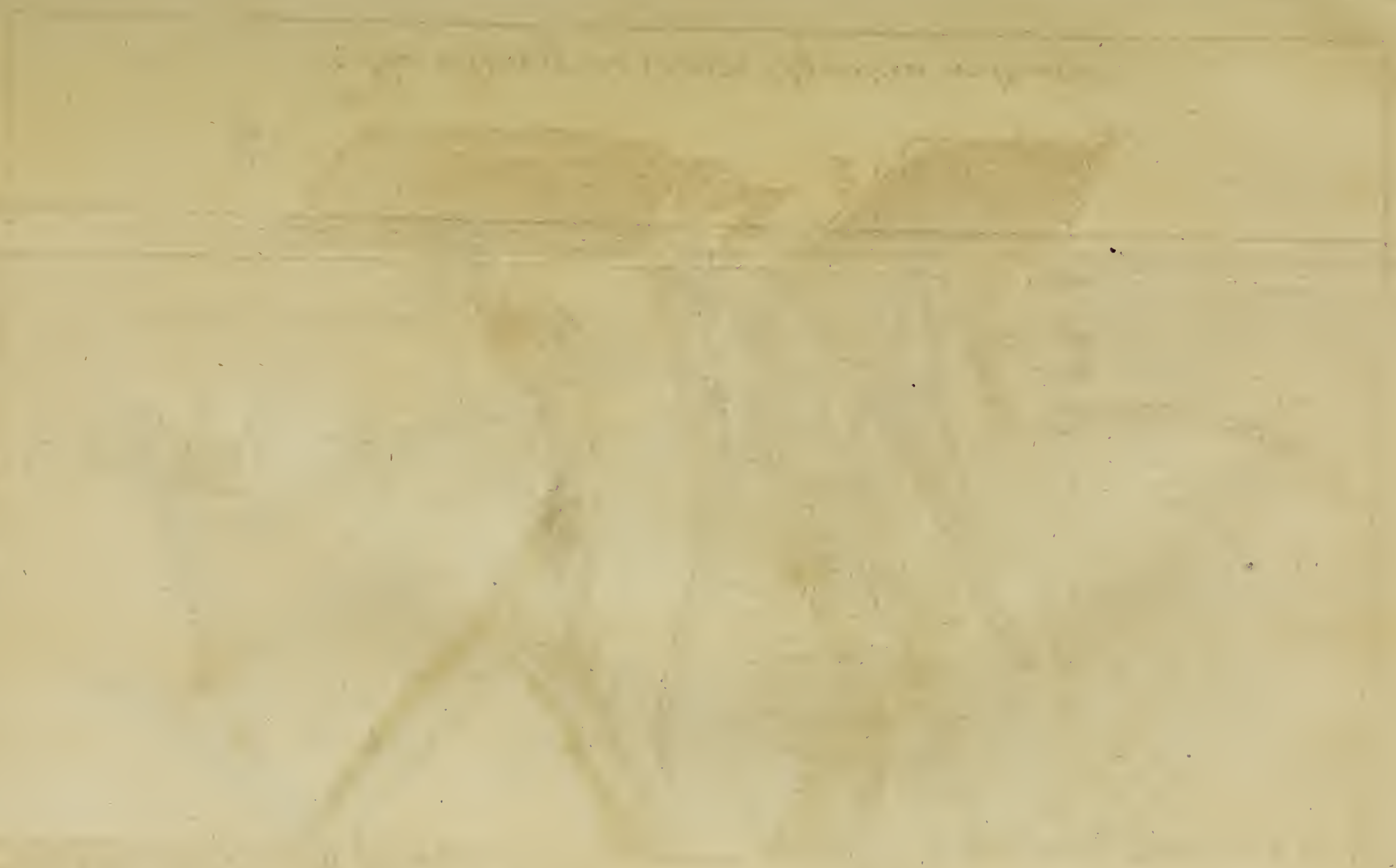
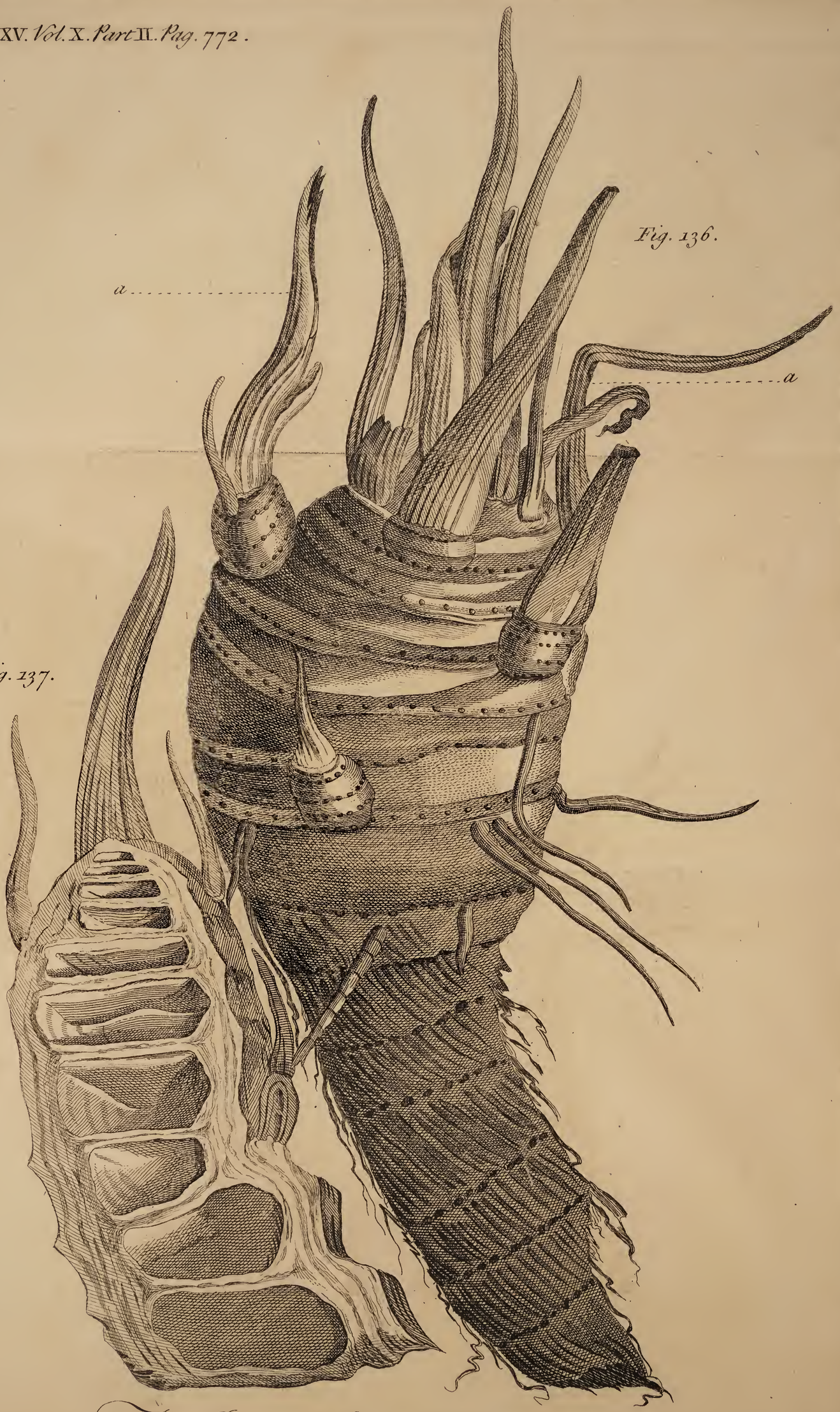


Fig. 136.

Fig. 137.



The Root of the Cicuta Aquatica in Winter.

ſhe ſoon ſtaggered, and grew delirious, could not ſwallow any ſolids, and loſt the perfect uſe of her eye-ſight a fortnight.

The vague reports of theſe, and Mr *Pots*'s caſes induced me to obtain the favour of 2 or 3 Druggiſts to look over ſome gentian-root, one parcel of which had no leſs than $\frac{1}{20}$ of a root, which at firſt ſight was diſcovered to be no gentian.

This root, for which we have yet no name, is of a greyiſh brown colour externally, but it is browner, and more reſinous internally: moſt of that which I have ſeen, is about the thickneſs of a finger; tho' ſome is much larger and whiter; which is a reaſon with ſeveral for thinking there are 2 ſorts of it; and indeed ſome pieces emit a ſtronger and more nauſeous ſmell: but this I apprehend may be occaſioned only by a larger quantity of reſin in them. All of them are of an acrid pungent taſte, and leave a dryneſs on the tongue.

I judged it therefore neceſſary to try what effects this root might have on dogs, that I might thereby the better conjecture concerning them on the human ſpecies. With this intention I decocted ℥ſs of this unknown root, powdered groſsly in ℥x of fair water, till ij were evaporated; then let the decoction ſtand 6 hours. After this I gave half of it, ſtirring up the powder, to a young dog. This made him inſtantly foam at the mouth; he grew ſick, and vomited part of the doſe; yet in leſs than $\frac{1}{2}$ an hour reeled like one drunk, had twitchings of his limbs, and after ſome time the motion of his heart was irregular, and intermittent, tho' ſtrong: he was ſleepy about an hour, but came gradually to himſelf in $\frac{1}{2}$ an hour more, and eat victuals, which before he refuſed.

Two days after, the ſame dog took ℥iv of decoction of gentian made as ſtrong as the former; but I diſcovered not any bad ſymptom from it. I uſed this quantity, as gentian-root is ſometimes given to that quantity in the practice of phyſic. It is above 10 days ſince he took the firſt decoction; and hitherto continues well.

Another dog took above ℥i of the unknown root, finely powdered, and mixed with butter: it inſtantly made him foam from the mouth, and cauſed ſudden vomiting, and, in $\frac{1}{2}$ an hour, weakneſs of his limbs, and ſtaggering, which laſted $\frac{1}{2}$ an hour, and then he recovered.

I tried to give a larger quantity to another dog; but it being too much like other irritating medicines, cauſed ſo great a vomiting, as deſtroyed the effects which a ſmaller quantity had before produced. One of the dogs had ſome looſe ſtools after taking it; another urined plentifully. Like experiments have been made by Mr *Pearce* at St *Thomas*'s hoſpital, which had nearly the ſame event.

Though none of the dogs were killed by this drug, but remain to appearance well, yet all Apothecaries have ſufficient reaſon to examine very ſtrictly their gentian, and to reject what they find not genuine, ſince one of the women before-mentioned, and a man that I have heard of, are both dead; and ſince gentian is of general uſe in medical compoſitions,

duce it, prejudice ſupport it, and engage men of parts and authority to recommend it to their inattentive ſucceſſors.

I ſhall finiſh this abſtract with remarking, that, were ſome of the leiſure moments of men of great abilities and experience devoted to inform the world of the inefficacy of ſuch methods and medicines as they have proved to be ſo, Phyſic would be reduced into narrower bounds; they would merit the thanks of every one in the profeſſion; and poſterity, at leaſt, would commend their endeavours.

Concerning the method of gathering Manna *near* Naples, *in a letter from* Robert More, *Eſq; to Mr* W. Watſon, *F. R. S.* N°. 495. p 470. May &c. 1750. *Read* June 21. 1750.

XXII. At *Arienzo*, a town between *Naples* and *Benevento*, I found an aſh-coppice, of 8 or 10 years growth, from which they collected *manna*. It ſeemed to have been tapped 2 years for that purpoſe; the branches had been barked each year about an inch broad, and 2 feet high; but they told me this was done by an inch at a time.

They place a cup at the bottom of the wound, which they empty every 5 days. This liquor becomes *manna*. They formerly let it dry upon the tree; but the preſent way keeps it cleaner. The manna begins to run (they ſay in the Scripture ſtyle to *rain*) the beginning of *Auguſt*; and if the ſeaſon proves dry, they gather it 5 or 6 weeks. The King of *Naples* has ſo large a revenue from it, that he is extremely jealous of it, during the ſeaſon guards the woods by *Sbirri*, who even fire upon people that come into them, and he makes the ſtealing of the liquor death. The ſeaſon in which I was at *Arienzo* prevented my ſeeing the ſpecies of aſh. I believe it to be what our Gardeners call the flowering aſh; the complexion of the bark and bud agrees with one of them I have in my garden at *Lindley*. The man who ſhewed me the wood, told me, it bore a pretty flower in the ſpring. At *Piſa* in the phyſic-garden they ſhewed me that tree in bloom as the manna-aſh. The tree is indeed common enough in that neighbourhood: I wonder Mr *Ray* does not mention it among the plants found there by him. The *Italians* call it *Orno*. A Botaniſt at *Rome* told me it was the *ornus officinarum*. A Phyſician at *Benevento* to the ſame purpoſe, that it was the *ornus* uſed in medicine. A perſon is gone from *Rome* to *Naples*, who has promiſed to be very particular in getting you information of their manner of curing it. He was bred a Chemiſt, and told me many ways of counterfeiting the ſeveral appearances of it. The moſt common is with *Glauber*'s ſalts and ſugar, with a ſmall mixture of manna. The price of manna at *Naples*, they told me, was 4 *carlins* (4 $\frac{1}{2}$ *d.* ſterling each) the *rotolo* (32 ounces).

An account of the preparation and uſes of the various kinds of Pot-aſh; *by* John Mit-

XXIII. Altho' pot-aſh is a thing daily uſed, and well known even to the vulgar; yet, as the making it is a mechanic art, practiſed only by the vulgar, and neglected and overlooked by the learned, ſo we have had no ſatisfactory account of it; and they, who underſtand it, generally keep it a ſecret, leſt others ſhould learn ſo beneficial an art. But as it is a commodity that no nation hardly can well be without, either for

for making ſoap, glaſs, dying, or bleaching, ſo the way of making it is generally underſtood in moſt countries, except our own. For in *France*, and other countries where they make wine, they make a kind of pot-aſh in an eaſy manner from the lees of their wine. In thoſe and other more ſouthern climates, they have many kinds of herbs hereafter mentioned, either ſpontaneous, or cultivated on purpoſe, which they as eaſily convert into pot-aſh. In *Germany*, and other more northern countries, they make great quantities of pot-aſh by extracting the ſalts of their wood-aſhes, in a manner that is well known. But it is only in *Ruſſia*, *Sweden*, and other northern nations, where the art of converting their wood-aſhes into pot-aſh, without the tedious proceſs of elixiviation, is either well known to the learned, or practiſed by the vulgar.

chell, *M. D. F. R. S.* N°. 489. p. 541. Oct. & Nov. 1748. *Read* Nov. 17 *and* 24. 1748.

By this means moſt nations are ſupplied with this neceſſary commodity of their own, except the *Engliſh*, who might be ſupplied with any quantities of it, from the great plenty of otherwiſe uſeleſs wood they have in their colonies, if not at home, if they knew how to make it. But it ſeems this art is ſo little underſtood among us, that many attempts I have known to make pot-aſh have all proved unſucceſsful merely upon that account, ſo as to be entirely laid aſide. This has put me for ſome time upon inquiring into the ways of making this commodity, of which ſeveral have been ſuggeſted to me, from the ſeveral trials and informations hereafter mentioned.

It is well known, that the aſhes of all kinds of vegetables whatever, afford pot-aſh in ſome meaſure or other; altho' ſome are much more fit for that purpoſe than others, which may be determined from the experiments of *Redi*, *Phil. Tranſ.* N°. 243, p. 281. *Boerhaave*, *Merret*, and others; ſo that we need not inſiſt upon them here.

As for the trees and herbs of our colonies in *N. America*, moſt of thoſe that are common in their woods are known to be fit for this purpoſe, as the aſhes of them all, burnt promiſcuouſly in their houſes, make a very ſtrong lye fit for ſoap. Of theſe, the fitteſt for that purpoſe is their *hiccory*, the moſt common tree in their woods, which makes the pureſt and whiteſt aſhes, of the ſharpeſt taſte, and ſtrongeſt lye, of any wood I have ſeen. Their *ſtickweed* is ſaid to do the ſame, which is as common a weed. For this reaſon the aſhes of both theſe plants were uſed by our *Indians* there, inſtead of ſalt, before they learnt the uſe of common ſalt from the *Europeans*. The aſhes of *tobacco* likewiſe, when damnified, or not fit for a market, or it's ſtalks, ſtems, and ſuckers, of which great quantities are thrown away, and rot and periſh, are very fit for pot-aſh, as they contain a great deal of ſalts, and are well known to make a ſtrong lye.

On the other hand, *pines*, *firs*, *ſaſſafras*, *liquid amber*, or *ſweet gum*, or all odoriferous woods, and thoſe that abound with a reſin or gum, are unfit for pot-aſh, as their aſhes are well known, even to our planters, to make a very weak lye, unfit for ſoap.

Beſides theſe that contain little or no ſalt, there are ſome other vegetables that afford a large quantity of it, but make a bad kind of pot-aſh, at leaſt for many purpoſes, on account of a neutral ſalt with which they abound. This ſeems to have been the caſe of the pot-aſh made in *Africa*, in a manufacture of that commodity ſet up there by the *African* Company, which Mr *Houſton* (who was chiefly concerned about it) tells us, in his *Travels*, proved ſo bad, on account of a neutral ſalt it contained, that the manufacture was left off on that account; or, perhaps, from their not knowing how to make it aright. What thoſe vegetables are, that afford this kind of aſh, is not well known, if it be not fern, and ſome ſea-plants.

Whatever vegetables we make our pot-aſh of ſhould be freſh or green, and no ways rotten, dried, or decayed. They ſhould likewiſe be burnt to aſhes by a ſlow fire, or in a cloſe place; otherwiſe, when they are burnt in the open air by a ſtrong fire, great quantity of the aſhes is conſumed in ſmoke, by the ſaline and terreſtrial parts being carried up in fumes, before they are ſeparated from theſe exhalable parts by the action of the fire. For the difference between burning wood in a cloſe place, or the open air, is ſo great, that the quantity of aſhes obtained from one is more than double the other. This we learn from the experiments of *Lundmarck* hereafter mentioned, who tells us, he burnt a quantity of birch in a cloſe ſtove, from which he obtained 5 pounds of aſhes; whereas the ſame quantity of the ſame wood burnt in the open air, yielded only 2 pounds.

It is for this reaſon, that moſt people who make pot-aſh, burn their wood in kilns, or pits dug in the ground; altho' the *Swedes* burn it in the open air, as the author above-mentioned informs us. This firſt ſtep, or the burning the wood to aſhes, ſeems to be taken by many for the whole proceſs of making pot-aſh; for they who pretend to have learned this art in *Ruſſia*, as well as *Lemery* and ſome other authors, hardly give us any other account of it.

But, in order to convert the aſhes, prepared in this or any other manner, to what is called pot-aſh, there are many different ways practiſed in different countries, which make as many different kinds of pot-aſh, that are all to be found in our markets, and have all their reſpective uſes.

1. The firſt of theſe is commonly called pearl-aſhes by our people, who import great quantities of it from *Germany*. This is no other than the lixivial ſalt of wood-aſhes, extracted by making a ſtrong lye of them, and by evaporating it to dryneſs, in a manner that is well known, and ſufficiently explained by *Kunkelius* in his art of making glaſs, *Boerhaave*, and many others; ſo that we need not inſiſt upon it here; we ſhall take a more fit opportunity to explain it, for the uſe of our people in *America*.

2. But the art of converting theſe wood-aſhes into pot-aſh, without this tedious proceſs of elixiviation, is only practiſed in *Ruſſia*, *Sweden*, and

and other northern countries, where it has been lately diſcloſed by one *Lundmarck*, who tells us he had often made it himſelf, in the manner he now deſcribes. This account is contained in an academical diſſertation upon this ſubject at *Aboe* in *Sweden*, and was communicated to me by Dr *Linneus*, Profeſſor of Botany at *Upſal*, as a genuine account of this art; which I think has hitherto been generally unknown.

This author tells us, " They have many large woods of beech in " *Smoland*, and other parts of *Sweden*, in want of which they take alder: " of theſe they are allowed to uſe only the old and decaying trees for " this purpoſe, which they cut to pieces, and pile in a heap, to burn " them to aſhes, upon the ground, by a ſlow fire. They carefully " ſeparate theſe aſhes from the dirt or coals in them, which they call " raking them; after which they collect them in baſkets of bark, to " carry them to a hut built in the woods for this purpoſe. This they " continue to do till they have a ſufficient quantity of theſe aſhes. Then " their whole art follows; for which they chooſe a convenient place, " and make a paſte of theſe aſhes with water, by a little at a time, in " the ſame manner, and with the ſame inſtruments, as morter is com- " monly made of clay or lime. When this is done, they lay a row of " green pine or fir-logs on the ground, which they plaſter over with " this paſte of aſhes: over this they lay another layer of the ſame ſtrait " logs of wood, tranſverſely or acroſs the others, which they plaſter " over with the aſhes in the ſame manner: thus they continue to erect " a pile of theſe logs of wood, by layer upon layer, and plaſtering " each with their paſte of aſhes, till they are all expended; when their " pile is often as high as a houſe. This pile they ſet on fire with dry " wood, and burn it as vehemently as they can; increaſing the fire from " time to time, till the aſhes begin to be red-hot, and run in the fire. " Then they overſet their pile with poles, as quickly as they can; and " while the aſhes are ſtill hot and melting, they beat and clap them, " with large round flexible ſticks made on purpoſe, ſo as to incruſt the " logs of wood with the aſhes; by which the aſhes concrete into a ſolid " maſs as hard as ſtone, providing the operation has been rightly per- " formed. This operation they call *Walla*, i. e. *Dreſſing*. At laſt they " ſcrape off the ſalt thus prepared, with iron inſtruments, and ſell it " for pot-aſh; which is of a bluiſh dark colour, not unlike the *ſcoriæ* " of iron, with a pure greeniſh white ſalt appearing here and there " in it."

All the pot-aſh we have from *Ruſſia*, *Sweden*, and *Dantzick*, is exactly like what our author here deſcribes, and ſeems to be made in this manner. It is, however, generally obſerved, that the *Ruſſian* is the beſt of theſe, on account of the greater quantity of ſalt in it. Now if, in the preceding proceſs, we make our paſte of the aſhes with lye, inſtead of water, it is plain the pot-aſh will be impregnated with more ſalt, and make all the difference there is between theſe ſorts of pot-aſh. This then is likely to be the practice in *Ruſſia*; where their wood may likewiſe

wiſe be better for this purpoſe, and afford more ſalt. This is well known to be the caſe of different kinds of wood: ſo our author above-mentioned tells us, he obtained $2\frac{27}{64}$ ℔ of ſalt out of eight cubic ells of poplar, which was very ſharp and cauſtic; but the ſame quantity of *birch* afforded only one pound of ſalt, and that not ſo ſtrong; and *fir* hardly yielded any at all.

The way of making pot-aſh above deſcribed may be the more eaſily underſtood by our people in *America*, for whom this is chiefly intended, as it is the ſame with their way of making lime of ſhells, the only lime they uſe in moſt places. Theſe ſhells they burn to lime between the layers of a pile of wood (inſtead of a kiln) till it is reduced to aſhes, in the ſame manner as is here directed to be done with aſhes, to make pot-aſh. The lime, thus made, is reckoned very good; but, as it is impregnated with the aſhes of the wood, and the marine ſalt that is often in the ſhells, it is apt to make the houſes that are built with it very damp in moiſt weather; ſo that the water often runs down their walls in ſtreams; which cannot but be very unwholſome in an air that is naturally cloſe and damp: the only way to prevent which would be, to waſh and dry their ſhells frequently, and burn them in dry pine, that afford little or no lixivial ſalt. But to return to our purpoſe:

3. There is another way of making pot-aſh, practiſed chiefly in *England*, where they make it in the following manner, as I am informed by ſeveral, who have ſeen it done:

With their aſhes of fern, or wood of any kind, they make a lye, which they reduce to what they call pot-aſh, by burning it with ſtraw. To do this, they place a tub full of this lye nigh a clean hearth of a chimney, in which they dip a handful of looſe ſtraw, ſo as to take up a quantity of lye with it. The ſtraw thus impregnated with lye they carry as quick as they can, to hold it over a blazing fire on their hearth, which conſumes their ſtraw to aſhes, and at the ſame time evaporates the water from the ſalts of the lye. Over the blaze of the firſt parcel of ſtraw they burn another dipt in lye in the ſame manner. This they continue to do till their lye is all expended. By this means the coals and aſhes of the ſtraw, and ſalts of the lye, are left on the hearth, and concrete together into a hard ſolid cake of a greyiſh black colour, which they ſcrape off, and ſell for pot-aſh.

This is an eaſy way of making pot-aſh, in want of proper veſſels to extract the ſalt of the lye by evaporation, or in want of wood to reduce the aſhes to pot-aſh in the way above mentioned, for which it ſeems to be contrived, and for which it is only to be commended. For the pot-aſh made in this manner is full of the coal of the ſtraw, and it's ſalt is not ſo ſtrong, as our workmen ſay, or ſo ſharp and corroſive as the ſalt of the foreign pot-aſh, that is calcined in an open fire; beſides other differences hereafter mentioned; which makes this pot-aſh unfit for ſome purpoſes, and not above half the value of the foreign.

4. They have a very different way in the north of *England* of reducing their *kelp* to pot-aſh, which they uſe for making alum. This is made of

of the different kinds of *fuci*, or ſea-weeds thrown upon the ſhore, or gathered on the rocks; which they dry a little in the ſun, and afterwards burn them in a kiln, built of the ſtones they find on the ſhore, in a cylindrical form, and about 2 foot or leſs in diameter. In this they firſt burn a ſmall parcel of the herb, and before it is reduced to aſhes they throw on more, till the kiln is full, or their materials are expended. This is ſaid to reduce the aſhes to a hard and ſolid cake, by the heat of the kiln, and quantity of ſalt in the herb, which makes what is commonly called *Kelp-Aſhes*.

There are ſome other ways of making pot-aſh, ſuggeſted by ſeveral, both authors and others, which appear to be more eaſy and ready than any of the above mentioned; for which reaſon they are apt to be tried by thoſe who make attempts of this kind. Theſe are deduced from what they reckon the nature and properties of this production: and there is no doubt, but if that was well underſtood, it might afford ſome inſight in the way of making it. For this reaſon we made the following experiments with the beſt *Ruſſia* pot-aſh, in order to diſcover it's nature and properties, and how they are moſt probably communicated to it; that we might ſee what we are to make; in order to imitate the beſt, or to make what is accounted good pot-aſh.

1. *Ruſſia* pot-aſh, as it is brought to us, is in large lumps, as hard as a ſtone, and black as a coal, incruſted over with a white ſalt, that appears in ſeparate ſpots here and there in it.

2. It has a ſtrong fetid ſulphureous ſmell and taſte, as well as a bitter and lixivial taſte, which is rather more pungent than other common lixivial ſalts.

3. A *lixivium* of it is of a dark-green colour, with a very fetid ſulphureous ſmell, and bitter ſulphureous taſte, ſomewhat like gun-powder, as well as ſharp and pungent like a ſimple *lixivium*.

4. Altho' it is as hard as a ſtone, when kept in a cloſe place, or in large quantities together in a hogſhead; yet, when laid in the open air, it turns ſoft, and ſome pieces of it run *per deliquium*; whilſt moſt other kinds of pot-aſh only turn friable, and crumble in the open air.

5. It readily diſſolves in warm water, but leaves a large ſediment of a blackiſh grey colour like aſhes, which is in a fine ſoft powder, without any dirt or coals in it, that are to be obſerved in moſt other kinds of pot-aſh.

6. As it is diſſolving in water, I have ſcummed off from ſome lumps of it a dark-purple bituminous ſubſtance, like *petroleum* or tar, which readily diſſolved in the *lixivium*.

7. This, or any other true pot-aſh, or a *lixivium* made of them, will preſently tinge ſilver of a dark purple colour, difficult to rub off; whilſt a mere lixivial ſalt has no ſuch effect.

8. Pieces of this pot-aſh boiling in water make a conſtant exploſion like gun-powder; which was ſo ſtrong as not only to throw the water to ſome height, but to lift up and almoſt overſet a ſtone cup in which

I boiled

I boiled them. Theſe exploſions were owing not ſo much to the included air, which ſome perhaps may imagine, as to the ſulphureous parts of the compoſition expanding and flying off: for this boiled *lixivium* had neither the green colour, nor fetid ſulphureous ſmell and taſte; at leaſt in any degree like what it has when made of the ſame pot-aſh by a ſimple infuſion in warm water.

9. I evaporated ſome of the green *lixivium*, made only by infuſion, and filtred thro' a double rag: as ſoon as it began to boil, a green powder, to which it's colour is owing, fell to the bottom, and the lye became pale. After it was evaporated to a pellicle, and ſet in a cool place, a ſalt ſeparated from it on the ſides of the cup, in angular cryſtals like tartar. Theſe cryſtals were ſoon formed, and in pretty large quantities, but were difficult to ſeparate from the alkaline lye and ſalt, in which and the open air they were apt to diſſolve: but from the pellicle I obtained ſome pieces of the ſame ſalt that would not diſſolve in the open air.

10. Oil of vitriol makes a ſtrong effervеſcence with this green precipitate, with a white fume, and a very ſtrong ſulphureous ſmell. It does the ſame with theſe white cryſtals, altho' the ſulphureous ſmell is not ſo ſtrong. But with the pure fixed alkali there was no ſuch ſulphureous ſmell to be diſcerned.

From theſe experiments we may determine ſomething about the nature and contents of pot aſh. This we are the better enabled to do, from the accurate experiments and reaſonings of the learned Mr *Geoffroy*, on a like ſubſtance made of charcoal and an alkali ſalt calcined together, in which he obſerved all the properties and contents of pot-aſh abovementioned, particularly related in the *Mem. R. Acad.* 1717. This was made of the ſame materials, and had all the properties above related of our pot-aſh; particularly a green *lixivium*, a ſtrong ſulphureous ſmell and taſte, a ſulphureous green precipitate, cryſtallized ſalts, and ſulphureous fumes with oil of vitriol. From hence this learned author concludes, that this ſubſtance contained the active ſulphureous parts of the wood, blended with more active igneous particles. Theſe, united with the alkaline ſalts, make a kind of ſoap, or ſulphureous ſaponaceous ſalt, reſembling ſoap of tartar, or *hepar ſulphuris*. The cryſtallized ſalts he attributes to the acid of the wood, mixing with the alkaline ſalts. All theſe parts of the wood then are contained in our pot-aſh; and he obſerved the ſame in the common *ſoda*, or *cineres clavellati*; altho' they are in a leſs degree in that than in the *Ruſſian* pot-aſh.

Beſides theſe, he ſhews that pot-aſh contains a metallic ſubſtance, which affords the *Pruſſian* blue. We may add further, that the combination of theſe principles makes many properties in pot-aſh, more than what reſult from them in a ſtate of ſeparation. The moſt remarkable of theſe ſeems to be it's exploſive quality; which we take to proceed from the cryſtallized ſalts approaching to the nature of nitre, and uniting with the ſulphur and charcoal; by which they form, from all

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theſe ingredients of gun-powder, a kind of that exploſive ſubſtance, whoſe parts are highly rarefied in an intenſe and confined heat, by which they readily explode in boiling lye.

By this we may perceive, that the difficulty in making pot-aſh aright, is, firſt, to reduce the materials to cinders and aſhes, and at the ſame time to preſerve their volatile, ſulphureous, and exhalable acid parts, that are totally deſtroyed in ſuch a degree of heat; and, ſecondly, to calcine theſe aſhes ſtill further, ſo as to flux their ſalts, and vitrify their terreſtrial parts, and at the ſame time to keep them ſeparate from each other, or prevent their running into an indiſſolvable glaſs. To give pot aſh ſome of theſe properties, ſeems plainly to require a degree of heat that will totally deprive it of others.

The moſt likely way by which it comes to receive all theſe properties, is from the way of making it in *Sweden* above deſcribed. In that proceſs, the green fir, in which the aſhes are burnt, impregnates them with the acid ſaline parts of the wood or tar, which is well known to be in pretty large quantities, and is abſorbed and fixed by the alkaline ſalts, and porous terreſtrial parts of the aſhes in this proceſs; ſo that, beſides the fixed alkaline ſalts of the aſhes, the pot-aſh, thus made, muſt likewiſe contain the more volatile ſalts of the pine, which are exhaled in ſmoke by burning the pine alone in the open air. Beſides theſe, it likewiſe contains the reſinous parts and ſulphureous fumes of the pine, that are hindered from exhaling by the heap of the maſs.

At the ſame time the alkaline ſalts are fluxed in the open fire, and in a manner vitrified with the terreſtrial Parts of the aſhes, which gives them their hard and ſolid conſiſtence; whilſt the ſulphureous and acid parts of the green wood hinder them from turning to a perfect glaſs, or inert *calx*. All theſe parts united together in the fire, make that ſaponaceous ſubſtance we find in the pot-aſh thus made, which further hinders the vitrification of the maſs, and endows it with many of it's moſt peculiar and active properties.

From hence we may ſee how difficult it is to make a ſubſtance endowed with all theſe properties in any other manner. This is the reaſon why we could never before make pot-aſh equal to that of *Ruſſia*, and the other northern countries, although we have much greater plenty of materials, and perhaps better: for this way of making it has never before been thought of by the Learned, or practiſed any where elſe, as far as I can learn.

Somewhat of the ſame qualities are communicated to the *Engliſh* pot-aſh, by the way of making it above deſcribed; but in a degree as much inferior, as dry ſtraw, uſed for that purpoſe, is to green wood: accordingly our workmen find that pot-aſh as much inferior to the foreign, for many purpoſes.

From this account of the contents and qualities of pot-aſh, and the way of making it, we may form ſome judgment of the other ways of making it, propoſed by authors, and ſuggeſted by many. Thus *Le-*

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mery and others tell us, pot-aſh is made in *Ruſſia*, and all the northern countries, only by calcining the aſhes in pits bricked within, and ſprinkling them well with lye, till they become hard and ſolid. But ſuch a calcination of aſhes with a lixivial ſalt, muſt render them whiter, inſtead of black, and muſt further deſtroy the active ſulphureous parts of the wood, which we find in pot-aſh rightly made. So that this only leaves the aſhes in the ſtate they were at firſt, or turns them into a kind of indiſſolvable glaſs, as we have found upon trial.

This, and the like miſtakes about the way of making pot-aſh, ſeem to proceed from a general error concerning the nature of it; for it is commonly ſuppoſed to be only a kind of inert *calx*, impregnated with nothing but a lixivial ſalt. Some ſuch miſtake ſeems to have fruſtrated all the attempts hitherto made of making pot-aſh in *America*; for, upon trial, what they have made there was found to be no better than common aſhes.

But the moſt general miſtake about the way of making pot-aſh, ſeems to proceed from the accounts we have of making it, from glaſſwort, and ſome marine plants, which are ſaid to be eaſily converted to this kind of ſubſtance, in the manner above-mentioned. But we apprehend, the way of making it from wood muſt be very different: for theſe herbs are eaſily reduced to aſhes by a ſmall fire that does not intirely conſume their ſulphureous parts, which wood is not. Theſe aſhes abound with a great quantity of alkaline and ſome neutral ſalts, that readily convert them to a hard and ſolid conſiſtence, which wood does not. They have likewiſe few or no terreſtrial parts, to run them into an indiſſolvable glaſs, when fluxed in the fire, as happens in wood-aſhes. Beſides, theſe herbs have few or no ſulphureous or acid parts, like moſt woods; and the pot-aſh made of them has few of theſe principles in it, like what is made of wood.

It is however generally ſaid, if we burn our wood in a cloſe place, as a kiln in which we burn lime, or make charcoal, or a pit dug in the ground, we may impregnate the aſhes with the ſulphureous fumes and acid parts of the wood, only by the cloſeneſs of the place, or by ſmothering the fire in it. If at the ſame time we impregnate them with a greater quantity of lixivial ſalt, it will flux the whole maſs, and make it run into a ſolid hard conſiſtence like pot-aſh. This is commonly directed to be done, by throwing freſh or green wood or herbs upon the others, as they are burning, before they are quite reduced to aſhes; or by ſmothering the fire, as in making charcoal; and at the ſame time to ſprinkle the aſhes, thus burnt, with a ſtrong lye from time to time, in the manner commonly practiſed with glaſſwort.

This would be a more ready way of making pot-aſh than any of the above-mentioned; but as thoſe who give their advice about it, have neither tried it, nor ſeen it done; and thoſe who have tried this or any other way, find more difficulty in it, than they at firſt imagined, we ſhall ſuſpend our judgment about it, till we ſee it fairly tried, leſt we

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ſhould deter ſome from making uſeful experiments of it, or lead others into fruitleſs and expenſive attempts.

By the various ways of making pot-aſh above-mentioned, and the different materials it is made of, there appear to be many different kinds of it, that have as different qualities. It would lead us too far beyond our preſent deſign, to give a particular account of each of theſe; but as they are uſed in many of our manufactures, it ſeems worthy inquiry, to know what ſorts are generally uſed, and what are the fitteſt to be uſed in them.

The workmen in *England* make two general kinds of it, which they diſtinguiſh by the names of pearl-aſh and pot-aſh. The firſt is a mere lixivial ſalt, which is ſuppoſed to be the only ingredient of any efficacy in pot-aſh; but, upon trial, there is found to be a great difference between them, eſpecially in making ſoap. The ſalt is ſo weak in the pearl-aſh, that it does not intirely diſſolve and unite with the fat. The reaſon ſeems to be, that theſe ſalts are diſſolved in water, in order to extract them, by which they loſe many of their cauſtic igneous parts; whereas in pot-aſh, the ſalts are calcined and fluxed in an open fire, with the ignited terreſtrial parts of the aſhes, which makes them more ſharp and corroſive: they are likewiſe incorporated with the coal, and fuliginous parts of the vegetables they are made of, or with the reſinous parts of fir, which gives them the ſulphureous quality above-mentioned, and makes a kind of *ſoap* of *tartar*, or *hepar ſulphuris*, in all pot-aſh; which makes theſe ſalts ſo ready to diſſolve, and incorporate with oil, or other pinguious ſubſtances.

This is perhaps the reaſon, why the *Cineres Ruſſici* are ordered for this purpoſe, inſtead of a mere lixivial ſalt, by the *College* of *Phyſicians* in their late Diſpenſatory. The ſoap made of them muſt be impregnated with their heating ſulphureous quality, which will make it more aperient and detergent, but not ſo mild and ſoft as ſome others; by which it may be more fit for obſtinate and indurated obſtructions, but will be more offenſive to the ſtomach; which is much complained of by ſome people, who take large quantities of the ſharper kinds of ſoap.

But, to conſider pot-aſh as a commodity in trade and manufactures, which is it's chief uſe; it appears, that the people in *England* not only have it at a dear rate, but the worſt ſorts of it, at leaſt for moſt purpoſes; which cannot but have a proportional influence on their manufactures: for it is generally of as great, and ſome ſorts of a greater value in their markets, than a pure lixivial ſalt; notwithſtanding the ſmall quantity of ſuch ſalt in aſhes, and the trouble and expence of extracting it; which ſeems to be occaſioned by their not knowing how to convert aſhes into this commodity; for in *Sweden*, where this art is known, *Lundmarck* tells us, pot-aſh is ſold for little more than a farthing a pound, which coſts our workmen nigh ſix-pence.

But this is not the only inconvenience we labour under for want of this commodity; the ſorts we are chiefly ſupplied with are perhaps the worſt of any, and unfit for many purpoſes for which pot-aſh is uſed. The only pot-aſh almoſt to be met with here, comes from *Ruſſia*, *Sweden*, and *Dantzick*, or is made in *England*. Theſe are all made either of wood or fern-aſhes, whoſe ſalts are never ſo pure and white at the beſt, as ſome others: but, by the way of making them, and the experiments on them above-mentioned, they appear to be impregnated with coal, ſmoak, and ſoot, which renders them ſtill more foul and impure, makes them of a black, brown, or green colour, and of a peculiar ſulphureous quality. On this account they are entirely unfit for making white glaſs: they make a very coarſe and ſtrong kind of ſoap; they are too foul, ſharp, and corroſive for bleaching, and are as unfit for dyeing, at leaſt many colours.

It is perhaps for this reaſon, that the workmen here, as they ſhewed me themſelves, make all their white glaſs with ſalt-petre; which muſt not only be more coſtly, but *Neri*, *Merrett*, and others, tell us it is not ſo good, at leaſt for the better ſorts of glaſs, as a ſharper lixivial ſalt. What they uſe for dyeing I am not ſo well appriſed of: it is ſaid, they uſe the volatile alkali of urine; but the *French* pot-aſh, made of the lees of wine, is generally allowed to be the beſt for that purpoſe. So likewiſe the *Alicant* pot-aſh is reckoned much the beſt for bleaching, and making of ſoap; as the *Syrian* and *Egyptian* is for making glaſs.

Theſe purer kinds of pot-aſh are all made of herbs, that grow only in the more ſouthern climates, whoſe ſalts are finer and whiter, and leſs acrid and corroſive than the ſalts of wood, or moſt other vegetables; and by the way of extracting them by calcination in a more open fire, they are more free of coal, ſmoak, and ſoot, or any other heterogeneous mixture. On this account they are much better for the purpoſes abovementioned, than the coarſe and foul kinds of pot-aſh that our people are ſupplied with.

All we have of theſe kinds of pot-aſh, it ſeems, comes only from *Spain*; for which reaſon our people were obliged to petition to allow the importation of pot-aſh from thence, during the late war; as appears by an order of the king and council of the 24th of *June* 1742. ſince they could not do without it in many manufactures: ſo that it may be worth our inquiry, to know what it is that produces ſo neceſſary a commodity.

This kind of pot-aſh is commonly called *Barrilha*, from an herb of the ſame name in *Spain* that produces it. The firſt account we have of this *Barrilha* is from *Amatus Luſitanus*, who leaves us much in the dark about it. It is generally ſaid in *England* to be a plant pretty well known to the Botaniſts by the name of *Ficoides Neapolitana, flore candido. Hort. Lugd. Bat.* but for what reaſon I cannot ſay. We have as little reaſon to believe with *John Bauhine* that it is what he calls *Kali vulgare*: For Mr *de Juſſieu* has ſhewn us, that the true *barrilha* is a different plant from

from any of theſe, from his own obſervations of it in *Spain*, where it was cultivated; of which he has given us a particular account, by the name of *Kali Hiſpanicum, ſupinum, annuum, ſedi foliis brevibus. Mem. Acad.* 1717. *p.* 93. or *Alicant glaſs-wort.*

The pot-aſh made of this plant, he tells us, makes the beſt ſoap, the fineſt glaſs, and is the beſt for bleaching of any other; for which reaſon it is much ſought after in all countries, where they value themſelves for theſe manufactures. But I queſtion very much, whether our workmen have it either pure and genuine, or in ſufficient quantities for theſe purpoſes. All the uſe I find made of it among them, is to make hard ſoap; although they ſay what they have of it ſpoils their ſoft ſoap, by making it curdle. This is well known to be the effects of ſea-ſalt; and Mr *de Juſſieu* and others tell us, that the true *barrilha* is often adulterated with ſea-weeds, which contain ſuch a marine ſalt; ſo that it is probably only this adulterated ſort that they have. Accordingly, all the *barrilha* I have found here, was of a dark brown colour, and very foul and ponderous; whereas the true ſort is ſaid, by all who know it, to be more porous, pure, and of a bluiſh colour. It is for this reaſon in all probability, that, notwithſtanding all the *barrilha* our workmen have at ſo dear a rate from *Spain*, yet they can never make ſo good ſoap, as what comes from thence, and ſome other places.

The only way then, by which we are likely to have this commodity either pure and genuine, or in ſufficient quantities at a reaſonable rate, is from the herb itſelf that produces it. Whether or not it would grow in *England* is not known, as I believe it has never been tried: but there is no doubt but it would grow very well in our colonies in *America*, as I am certainly informed it does in the *Spaniſh* colonies there, where they have great plenty of it; and a ſort that is indigenous, particularly in *Peru*, which might probably be found in our colonies, if ſought for by thoſe who knew it. But wherever it will grow in any of the *Engliſh* dominions, there is no doubt but it would be a conſiderable improvement, where pot-aſh of all kinds is ſo valuable a commodity, and ſo much wanted; for it grows on the ſame ground with corn of any kind, which it does no harm to, as it is a ſmall annual herb, that does not ſpread till the corn is ripe, or off the ground.

There are ſome other plants that are known to make a kind of pot-aſh, commonly called *rochetta*, which is ſaid to be even preferable to the *barrilha*, eſpecially for making glaſs. Theſe are the firſt and ſecond kinds of *kali*, deſcribed by *Proſper Alpinus*, in his account of the plants of *Egypt*. The firſt of which is the above-mentioned *ficoides* that grows in *Italy*, and all over the *Levant*, but the other is peculiar to *Egypt*. Theſe would be fit improvements for our colonies in *America*, where we ſeem to want nothing more than ſome proper production for the vaſt tracts of land we are poſſeſſed of there. But theſe plants alone afford a commodity, which *Pr. Alpinus* and *Rauwolfius* tell us they ſaw

many large ſhips yearly loaded with iu *Egypt*, and which gives the excellency to the glaſs and ſoap that are made at *Venice*.

It would be worth while then at leaſt to make a trial of a production, that is likely to improve both our trade abroad, and our manufactures at home. It was this that put me upon the preſent inquiry, as an improvement fit for our colonies, which if I find acceptable, I ſhall hereafter conſider ſome others.

Concerning the Propagation *and* Culture *of* Muſhrooms; *by the Rev. Mr* Roger Pickering, *V. D. M.* N°. 472. p. 96. Jan. &c. 1744. *Read* April 26. 1744.

XXIV. 1. The late rains having thrown up upon my muſhroom beds a great quantity of thoſe plants, I take the opportunity to ſend ſome additional obſervations to thoſe printed in theſe *Tranſactions*. *

After having repeated the experiments, then made, upon plants and ſeeds of this year, I find no reaſon to alter any thing there mentioned, either as to the *lamellæ* or chives on the concave ſide of the *umbella*, being the *ſiliquæ* or ſeed-veſſels; or the ſeeds falling from thence to a lodgement wiſely prepared for it on the middle of the *caulis*, and from thence eaſily ſliding to the earth contiguous to the mother-plant; or as to it's propagation by fibrous runners, or *ſtolones*, like potatoes; all which, I am perſuaded, theſe following new obſervations ſufficiently confirm.

1. Upon examination of ſeveral *lamellæ*, I not only diſtinctly obſerved ſeeds, of ſize and colour proportionable to the maturity of the plant, lodged therein, but alſo a ſiliquaceous aperture, with a row of ſeeds ready to fall through it; which is a very evident proof, that each diſtinct chive is a *ſiliqua* or ſeed-veſſel.

2. Upon obſervation of the filament ſituated on the middle of the *caulis*, upon which, as I before obſerved, I at firſt diſcovered the ſeed, I found both it's contexture and ſituation evidently demonſtrating the end for which the wiſe Creator placed it there; *viz.* to intercept the ſeeds in their fall to the ground; whereby the power which the wind would otherwiſe have upon ſuch minute bodies is leſſened, and the ſeed, with little or no diſſipation, ſecurely directed near the ſtem of it's mother-plant. For this filament is indented and pappous, to catch and lodge the ſeed as it falls from the *ſiliqua*; and is, at firſt, rigid, and ſtanding horizontal to the *umbella* or head, and at right angles with the *caulis*; whereby few or no ſeeds can fall without being intercepted: but, as the plant comes nearer to it's decay, this filament relents, falls down cloſe to the ſides of the *caulis*; and it's ſeveral indentures then making parallel lines with the fibres of the ſtalk, the ſeeds are, through them, conveyed, as through little ducts or channels, to the ground.

'Tis further to be obſerved, that this filament is not of ſo ſucculent a contexture as the *ſiliqua* or ſeed-veſſel; ſo that the ſeeds, which would otherwiſe rot in the *ſiliqua*, are here retained in full health, till the period of their falling to the ground. I have now by me the filament of a plant, laid by for obſervation ever ſince *October* the 28th laſt paſt,

* See Vol. VIII. Part ii. Chap. v. § xi. 1.

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which is near half a year ago; from which, two days ago, I took ſeeds fair and perfect.

3. Upon examination of the *caulis* in ſeveral ſections, I find the muſhroom a plant more perfect than has been thought. It has a perfect *radix*; a *caulis* conſiſting of fibres, the interſtices of which are filled up by a parenchymous ſubſtance, leading from the *radix* to the *umbella* or head: it has, as has been obſerved, it's *ſemen* and *ſiliquæ*, and more regular periods of vegetation than is ſuppoſed. The common opinion of a muſhroom's ſpringing up in a night, and periſhing in a day, has no foundation in fact. I have now by me ſome in all ſtates of maturity; ſome of which, to my knowledge, are near a fortnight old, and yet but juſt arrived to a fitneſs for the table.

4. Upon examination of ſeveral muſhrooms, expoſed to the open air, but kept from the injuries of the ſun and rain, I find no *animalcula* bred therein, nor, as yet, a tendency to putrefaction; though they have been expoſed thus for a week. On the other hand, upon examining a muſhroom, very far from being full grown, putrefied by the rain, and moiſture of the dung in the bed, I found *animalcula*, diſcoverable only by the third magnifier, floating in the liquor, ſqueezed out from it: from which I think it evident, that the dangerous conſequences which hiſtory has informed us to have attended the eating of muſhrooms, have not ariſen from any poiſonous quality eſſential to them, but from the accidental *ova* or *animalcula*, which the richneſs of their nutriment has allured to them, and which their contiguity to the ground, and the places they are produced in, render them obnoxious to. Theſe *animalcula* I have lately had an accurate view of; but as they demand a fuller account, than this paper, already too long, will permit, I ſhall reſerve the obſervations upon them for another opportunity of being honoured with the attention of the Society.

However, it may not be amiſs to ſubjoin a ſhort account of the culture in the kitchen-garden of a plant which contributes ſo much to the delicacy of polite tables, which may be depended upon, from perſonal trial and ſucceſs; as thoſe few writers upon the ſubject, not being acquainted with the true muſhrooms, are not entirely to be depended upon.

In the melonry, or place allotted in the garden for hot-beds, the muſhrooms muſt be thus ordered: having marked out a portion of ground one yard and a half broad, and of any length, as the ground will permit; faſten two ſticks at each end of the diametrical diſtance already marked out, which ſhall, by inclining to each other on the top, form an *Iſoſceles* triangle. To the breadth and height of theſe ſticks muſt the bed be made, of old, rich, dry dung, cloſely trod together: neither new nor moiſt dung is proper; for the muſhroom being naturally of a ſucculent and ſpongy contexture, too much heat, and too much moiſture, muſt neceſſarily injure it. Having raiſed your bed to the height and breadth propoſed, cover it with fine ſcreened mould, to

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the thickneſs of three inches, into which, at proper diſtances, put either that white fibrous ſubſtance, which may be collected from the place where muſhrooms have formerly grown; or elſe water it with water in which the chives and parings of muſhrooms have been ſteeped; or you may put in the chives in groſs. If you take the firſt ſtep, the muſhroom is propagated by tranſplantation; that white fibrous ſubſtance, already mentioned, being no other than the *ſtolones* of old muſhrooms, from which others are propagated, like potatoes: if you take the ſecond, that is, by watering; the ſeeds lodged in the parings being, by the water, ſeparated from the *ſiliquæ*, and with it poured upon the mould, are that which gives fertility to the beds thus managed. If you put the chives in groſs into the mould, it is no more than ſowing the ſeeds in the pods, as in other plants it is ſometimes neceſſary to do. Over the bed, thus prepared, muſt conſtantly be kept a covering of long new litter, to the thickneſs of one foot, to preſerve the plant from the froſt, the ſun, and the wind. During the middle of ſummer, and the extremity of winter, it is beſt to make theſe beds under ſhelter; but at other times they are beſt expoſed, the warm rains not a little contributing to their fertility; which, by the ſloping faſhion of the beds, are ſuffered to moiſten them no more than neceſſary.

I ſhall only add, that when I ſpeak of the muſhrooms, as I have all along done, I mean the *fungus* * *poroſus*, *craſſus*, *magnus*, called, by way of eminence, in *England*, the *Muſhroom*.

Remarks *on the preceding paper, with* Obſervations *upon the* poiſonous faculty *of ſome ſorts of* fungi; *by Mr* W. Watſon, *F. R. S.* N°. 473. p. 51. May &c. 1744. *Read* May 31. 1744.

2. I hope I ſhall have the Rev. Mr *Pickering*'s excuſe, if I lay before you a few further obſervations upon his papers concerning *muſhrooms*.

With regard to the ſeeds of *muſhrooms*, although they were never ſhewn to the R. S. before, the fact was known to many Members thereof: for the induſtrious *Micheli* did not only raiſe muſhrooms from their ſeeds, but has, in his tables, ſhewn the daily progreſs from their firſt point of vegetation, even to their perfect ſtate.

The *Fungus poroſus craſſus magnus* is not the muſhroom uſually raiſed in *England* for the table, as this gentleman did imagine; that name being given by *John Bauhin* †, to a ſpecies which is to be diſtinguiſhed from all other *fungus's*, by the inferior ſubſtance not being divided into *lamellæ*, or (what we call in *England*) gills; but has, in lieu thereof, a great many *papillæ*; and being of a greeniſh yellow colour. But what is raiſed in *England* (of which this learned gentleman brought ſeveral ſamples to the Society) is the *Fungus campeſtris albus ſuperne*, *infernè rubens*, of *John Bauhin*, which differs *toto cælo* from the former, and which Dr *Dillenius* enumerates among the ſpecies of *boletus*; whereas the latter is a ſpecies of *amanita*.

I muſt beg leave to differ from this gentleman likewiſe, in regard to the uſe of the ring, which ſurrounds the ſtalk of this *muſhroom*. He imagines it placed there, by the wiſe Author of Nature, to break the

* See the following article. † Hiſt. III. 833.

fall

fall of the ſeeds when ripe; whereby thoſe light bodies may be preſerved from the fury of the winds, in order to the abundant propagation of their ſpecies. I have reaſon to believe, that thoſe ſeeds, which fall upon this ring, fall there by accident; and adhere there only from the viſcoſity, whereby they are intangled. But, before I examine this matter, give me leave to make a few obſervations upon the oeconomy of this plant. The *fungi*, then, are of that claſs of vegetables, which are ranged, by that moſt ſkilful Botaniſt *Linnæus*, under the appellation of *Cryptogamia*, or thoſe which perform their fructification in ſecret. Under this head we find the fig-tree, all the ſpecies of fern, moſſes, muſhrooms, and a few others, whoſe flowering and ſeeding are obſerved with more difficulty, than in thoſe we uſually call the more perfect plants. In ſome of this claſs, the fructification, notwithſtanding the great aſſiſtance furniſhed to the modern Botaniſts by microſcopes, which the ancient were wholly deſtitute of, remains yet undiſcovered. This plant then being of this claſs, almoſt all thoſe whoſe ſtems are thick and fleſhy, as well as their umbels, have a ring upon their ſtem; from which, when the plant is young, and until it arrives at a flowering ſtate, there ariſes a membrane, which connects the rim of the umbel to the ſtem, and preſerves the under part of the plant in this ſtate: but, when this is over, the umbel, which before was almoſt of an hemiſpherical figure, growing larger, and the membrane not giving way, is looſened from the rim of the umbel, and adheres only to the ſtem. Soon after this ſtate, the ſeeds ripen, and the umbel, loſing it's former figure, commences almoſt a plane; and the plant in this ſtate is ſold in our markets, by the name of *flaps*. Now, when the umbel is of this figure, the ſeeds, being perfectly ripe, muſt fall naturally upon the whole ſpace the umbel covers (which *Micheli* obſerved, by placing leaves of trees under them); and, upon the ring, as well as any other part; though I have reaſon to believe not more. As for thoſe ſpecies of *fungi* whoſe ſtems are thin, and whoſe umbels are ſoft, and more ductile, they need not, nor have they, this ring or membrane; becauſe, in their tender ſtate, the rims of their umbels clap themſelves quite cloſe to the ſtalk, in the form of a contracted *umbrella*; and expand as the others do, when their ſeeds are ripe: nevertheleſs the ſpecies of this tribe are as numerous as the former.

I now come to conſider how far the poiſon of *muſhrooms* can poſſibly proceed from animalcules: but, firſt, give me leave to doubt, whether or no any perſon was ever injured from eating the common *muſhroom*, or *amanita*; unleſs ſuch accident may have proceeded either from eating too many at once, and thereby overloading the ſtomach; or from ſome particular diſlike in the conſtitution; as we ſometimes ſee, even with regard to honey, cheeſe, and ſome of the moſt innocent parts of our diet; but which, notwithſtanding this, are by no means to be ranked among poiſons. If there were many inſtances of their being pernicious, ſuch muſt frequently occur to the practitioners in Phyſic, on account

of

of the vaſt quantity annually conſumed in *London*; but I don't remember to have even heard of any ſuch accident; but many inſtances occur of the noxious quality of many of the other ſpecies of this tribe: nor is it at all wonderful, that the different ſubjects of this claſs of vegetables ſhould differ in their effects more than thoſe of the more perfect kind. The roots of carrot, parſnep, and many others of the umbelliferous claſs, are daily uſed as food; but the water-hemlock, and *Lobel*'s *oenanthe*, though of the ſame claſs, are moſt certain poiſons.

Here I muſt obſerve what pains have been taken by Naturaliſts, to diſtinguiſh the uſeful from the pernicious kinds. Among the *Romans*, the *boletus* mentioned by *Juvenal*, on account of the death of the Emperor *Claudius*, is ſufficiently deſcribed by *Pliny*; but, among the later writers, *Carolus Cluſius* was of the firſt of thoſe, who, about the middle of the ſixteenth century, being tired with the Critics and Commentators of the time he lived in, preſumed to believe, that the whole of knowledge was not confined to the writings of the *Greek*, *Roman*, and *Arabian* Phyſicians; becauſe, from the revival of letters in the weſtern world to his time, nothing was regarded, as of any importance, but what was dignified with the authority of antiquity: and hence it came to paſs, that when the clouds of ignorance began to diſperſe, the *epocha* of Commentators took place; but many of the deſcriptions of the plants of *Theophraſtus*, *Dioſcorides*, and *Pliny*, were ſo very deficient, that little light could be acquired therefrom; eſpecially from this laſt author, who is to be conſidered as the only *Roman* Naturaliſt that we have handed down to us; and it is no wonder, if, among the vaſt variety of ſubjects that this moſt admirable Hiſtorian treats of, he is, in many inſtances, rather to be conſidered as an Enumerator, than as a Deſcriber: I ſhall only mention the imperfect ſketches he has left us of *Silaus*, *Geum*, *Molon*, among the many others.

There aroſe, I ſay, ſuch heats and diſputations among the critics upon thoſe authors, very often about trifles, that they rather increaſed than diminiſhed the ignorance of thoſe times. This excellent *Cluſius*, finding that a thorough knowledge of nature was neceſſary, not only to underſtand rightly the Ancients, but to lay the foundation of future knowledge, was deſirous to join careful obſervations of his own to thoſe which were to be acquired from books. How much he travelled, and what progreſs he made in this undertaking, his many valuable works are the beſt teſtimony. Among them, his hiſtory of *fungus's* bears not the *leaſt* character; he therein enumerates a great variety, not only of the eſculent, but noxious kinds; but, as the different appellations of every ſpecies was not, at that time, much conſidered, he gives no other *ſynonyms* to either claſs, than that of, *viz. Eſculentorum primum genus*, *noxiorum decimum genus*, and ſuch-like. But this want of ſpecific names has been ſufficiently ſupplied by *John* and *Caſpar Bauhin*, *Ray*, *Moriſon*, *Tournefort*, *Vaillant*; but, above all, by *Dillenius*, in his *Catalogus Giſſenſis*, and by *Micheli*, in his *Nova Plantarum Genera*. In moſt of theſe

theſe authors we find inſtances of miſchievous effects from the pernicious kinds; which property ſome of them have equal to *opium*, aconite, or henbane; but how far this property proceeds from animalcules, the following inſtance will ſufficiently demonſtrate. We have a ſort growing in *England*, called, by *Caſpar Bauhin*, *Fungus albus acris*; which Monſieur *Tournefort* has rightly obſerved ſtimulates the tongue, and is almoſt as ſharp as though it were ſteeped in ſpirit of nitre; and, being rubbed upon paper dyed blue with turnſole, turns it as red as any violent acid ſpirit will. This cauſtic quality remains even after the *fungus* is dry. We need make no further inquiry for the cauſe of the poiſon in this plant; the above-mentioned is a ſufficient criterion. *John Bauhin* likewiſe tells you, that after having handled this *fungus*, he rubbed his eyes by accident, and brought on a violent irritation upon his eye-lids. *Caſpar Bauhin* mentions a ſort which kills the very flies. *Micheli* deſcribes a ſpecies, which, upon eating them, almoſt killed the Painter he uſually employed, and an old woman, the painter's mother. This man, being ſent by the author to delineate ſome of theſe *fungus's*, and being taken with their appearance, ordered ſome of them to be fried, and he and his mother eat thereof; but were, in about two hours, ſeized with violent pains in their bowels, from which they were with great difficulty relieved. I might produce many other inſtances of this ſort; but the above, I believe, are ſufficient.

XXV. By covering up my trees with ivy, in *February* I have vaſt quantities of apricots and peaches, while my neighbours have hardly any. *Of covering trees with ivy; by* R. Gale, *Eſq*;

F. R. S. N°. 475. p. 267. Jan. &c. 1745. *Read* Jan. 24. 1744-5.

XXVI. The deſign of communicating the following paper to the *R. S.* is, to invite Gentlemen, after the example of a practice that has long obtained in *Herefordſhire*, to attempt an improvement of their waſte lands, by planting ſuch kind of fruit-trees, as are mentioned, in hedges and barren places; which, for aught appears, would thrive as well in other counties, perhaps in ſome parts of moſt counties in *England*, as in that of *Hereford*. *Extract of a letter from the Rev.* H. Miles, *D. D. and F. R. S. to the* Preſ. *relating to ſome improvements which may be made in* Cyder *and* Perry. N°. 477. p. 516. Aug. &c. 1745. *The letter read* Nov. 1745.

Extract from a manuſcript, written Anno 1657-8, *by Mr afterwards, Dr* John Beale, *and F. R. S. in the way of an epiſtolary addreſs to* S. Hartlib, *Eſq*; *for his uſe, and that of Mr* Pell, *the then* Britiſh *Reſident at* Zurich; *and which appears to have been intended as a ſequel to that ſcarce and valuable piece intituled* Herefordſhire Orchards, *inſerted in the later editions of Mr* Bradley's *New Improvement of Planting*, &c.

The author undertakes to evince, "That crabs and wild pears, ſuch "as grow in the wildeſt and barren clifts, and on hills, do make the "richeſt, *Concerning an excellent liquor made of*

a mixture of rough Pears *and* Crabs.

" richeſt, ſtrongeſt, the moſt pleaſant and laſting wines that *England* " yet yields, or is ever like to yield. — I have ſo well proved it al- " ready (ſays he) by ſo many hundred experiments in *Herefordſhire*, " that wiſe men tell me, that theſe parts of *England* are ſome hundred " thouſand pounds ſterling the better for the knowledge of it."

He mentions, of theſe kinds of auſtere fruit, the *Bareland* pear and the *Bromſbury* crab, of which notice is taken page 4th of the Tract intituled *Herefordſhire Orchards*; and intimates, " That tho' the diſco- " very of them was but then lately made, yet they had gotten a great " reputation."—He adds, " The croft crab and white or red horſe-pear " do excel them, and all others, known or ſpoken of in other coun- " tries." Of the red horſe-pear of *Felton* or *Longland* he obſerves, " That it has a pleaſant maſculine vigour, eſpecially in dry grounds, " and hath a peculiar quality to overcome all blaſts."—Of the quality of the fruits he ſays, " That ſuch is the effect which the auſterity has " upon the mouth on taſting the liquor, that the ruſtics declare 'tis " *as if the roof were filed away*;" and that " neither man, nor beaſt, " care to touch one of theſe pears, tho' never ſo ripe." Of the pear called *imny-winter*, which grows about *Roſſe* (in that county) he obſerves, " That it is of no uſe but for cyder; that if a thief ſteal it, he " would incur a ſpeedy vengeance; it being a furious purger; but, " being joined with well choſen crabs, and reſerved to a due maturity, " becomes richer than a good *French* wine; but, if drank before the " time, it ſtupifies the roof of the mouth, aſſaults the brain, and pur- " geth more violently than a *Galeniſt*." This quality, he apprehends, will ſufficiently ſecure the fruit from being ſtolen, tho' the trees ſhould be planted in the moſt remote grounds.

Of the quality of the liquor he ſays, " That, according as it is mana- " ged, it proves ſtrong *Rheniſh*, *Backrac*, yea pleaſant *Canary*, ſugar- " ed of itſelf, or as rough as the fierceſt *Greek* wine, opening or bind- " ing, holding one, two, three, or more years—that no mortal can yet " ſay at what age it is paſt the beſt. This (adds he) we can ſay, that " we have kept it till it burn as quickly as ſack, draws the flame like " *Naphtha*, and fires the ſtomach like *aqua vitæ*." He ſaith, " That " he made trial at his own houſe with wine *d'Hay*, by a Merchant of " *Briſtol* highly extolled, which, compared with a liquor made of crabs " and wild pears, was ſo much inferior, in the judgment of all, that " the compariſon was ridiculous." And he further relates, " That a " Gentleman (Sir *H. Lingen*) a great Planter, and expert in many ex- " periments, had then by him many tuns of a liquor made with this " mixture of fruit, which he, by a deſigned equivocation, called pear- " maine cyder, that carried the applauſe from all palates—that all his " common hedges yielded him ſtore of the ſaid fruit."

To recommend this eaſieſt, cheapeſt, and moſt profitable kind of agriculture, (as he calls it) he ſays, " That the beſt of theſe pears grow " upon very bare and ſandy hills, or vales; crabs on any mound or

" bank

" bank that may be raiſed on an heath; that one pear-tree ordinarily " bears yearly 40, 50, 60, 70 gallons of ſtatute-meaſure, and ſome 5, " 6, or 7 times as much. Since I undertook this argument (adds he) " within 10 miles of this place we made in one year 50,000 hogſheads, " as I examined, not by fancy, but by rule and inquiry; and this ſhews " the hardineſs of the fruit. Let our noble patriots weigh, that this is " not a thing in the air, but a moſt certain and apparent truth, import- " ing no leſs than the art of raiſing ſtore of rich wines on our common " arable, on our hills, and waſte grounds; the charge a trifle, the pains " very ſmall, the profit incredible. Hence my deſign is to urge the " incredible benefit that would redound to theſe nations, if leading per- " ſons would make themſelves, their tenants and cottagers, all happy " by following our example. I leave the reader to caſt up how many " millions of hogſheads of wine, in a few years, would be raiſed in the " land. And truly I conceive it the chief cauſe, that, in all theſe times " of late wars, none of our pooreſt cottages did ſee want; in all houſes " they had the ſame number of meals, and the ſame conſtant fare: our " arable ſeems not a jot the leſs, nor our paſture the leſs; and for ſome " uſes the ſhadow of the orchard brings on the graſs a fortnight the " ſooner, as commonly for ewes and lambs."

The author concludes his tract with theſe words, " If this Diſcourſe " be duly valued, we need not raiſe wars to deſtroy one another, or eat " up one another, as we do; in a ſhort time we may be provided of " fruit enough for another world as big as this, and to make this a true " Paradiſe."

The ſubſtance of ſome experiments of planting ſeeds *in* moſs, *lately made by Mr* Charles Bonnet, *of* Geneva, *F. R. S.* N°. 486. p. 156. Feb. & Mar 1748. *Read* Feb, 18. 1747-8.

XXVII. Mr *Bonnet* was inclined to try whether plants were capable of vegetation, when they were only ſet in moſs, inſtead of being planted in the earth. With this deſign, he filled with moſs ſeveral garden pots, and he compreſſed the moſs more or leſs, as he judged, the ſeveral plants he intended to place in them, might reſpectively require a cloſer or a looſer ſoil.

He then ſowed in moſs, wheat, barley, oats, and peaſe. And he found, firſt, that all the grains ſowed in that manner came to maturity later than thoſe of the ſame ſorts which were ſowed at the ſame time in mould.

2dly. That the ſtems from the ſeveral grains ſowed in the moſs were generally taller than thoſe which ſprung from the ground.

3dly. There came from the grains ſowed in the moſs a greater number of blades than from the grains ſowed in the earth.

4thly. The grains ſowed in moſs produced more plentifully than the others.

5thly. Thoſe grains that were gathered, from the produce of thoſe which vegetated in the moſs, having been again ſowed ſome in moſs, and ſome in earth, ſucceeded well in both.

Mr *Bonnet* has alſo planted in moſs, pinks, gillyflowers, daiſies, tuberoſes, tulips, hyacinths, jonquils, and narciſſus's; and all theſe plants ſucceeded as well as others of the ſame ſorts, which he at the ſame time planted in mould.

He alſo placed in moſs cuttings and layers of vines, and theſe cuttings and layers became vines; and theſe vines in a ſhort time grew larger than others, that came from cuttings and layers planted at the ſame time in the ground.

A letter from the Rev Mr R. Pickering, *F R. S. to the* Preſ. *concerning the manuring of land with* foſſil ſhells. N°. 474. p. 191. June &c. 1744. *Read* Dec 6. 1744.

XXVIII. I take the liberty of offering. by your hands, to the *Society*, a ſpecimen of foſſil ſhells, lately ſent me, which are pretty perfect; and, on account of the place from whence they were taken, remarkable. At *Woodbridge* in *Suffolk*, in a Farmer's ground, there are ſome pits, in depth equal to the uſual height of houſes, conſiſting of ſeveral *ſtrata* of ſhells from the bottom to within about 9 feet of the ſurface, where the natural ſoil of gravel and ſand begins. The maſs of ſhells here collected is prodigious; the ſorts various; but that kind which I have taken the liberty to produce, and which, I apprehend, is the *buccinum vulgare*, or whilk, prevails the moſt. The ſhells before you were taken up from the bottom of the pit, where the depth to which theſe ſhells reach is not yet dug down to. *Woodbridge* is ſeated 7 miles N. E. from *Ipſwich*; and is about the ſame diſtance from *Orford* on the ſea-coaſt, which bears from it due E. How, therefore, ſuch a maſs of ſhells ſhould get there at ſuch a diſtance from the ſea, when hiſtory has informed us of no remarkable inundation in thoſe parts, or that ſuch a tract of land was ever recovered from the ſea, appears to me difficult to determine, by any other than the *Moſaic* hypotheſis of an univerſal Deluge. 'Tis true, indeed the river *Deben*, which riſes at *Debenham* ſome miles off, runs by *Woodbridge*, within $\frac{1}{2}$ a mile of theſe pits, in it's courſe to the *German* ocean, where it empties itſelf: but ſuch a collection of ſhells can hardly be ſuppoſed to have been thrown up by it, and a ſurface of earth, to the depth of 9 feet, ſettled over it, without allowing a ſpace of time for ſuch a circumſtance, almoſt equal to the interval between us and the Deluge. But, however theſe things be, the Farmer, in whoſe ground theſe ſhells are, has, as I am informed, laid the foundation of an ample fortune from them. The man contented himſelf in the old beaten track of the Farmers (a behaviour which does infinite prejudice to the improvement of Natural Knowledge in Agriculture), till an happy accident forced him upon a bold improvement. He uſed to mend his cartways, when broken up by harveſt-work, with theſe ſhells; in which buſineſs his cart one day broke down, and threw the ſhells out of the cart-track into the cultivated part of the field. This ſpot produced ſo remarkable a crop next year, that he put ſome loads upon a particular piece, kept the ſecret to himſelf, and waited for the event. This trial anſwering expectation, he directly took a leaſe of a large quantity of poor land, at about five ſhillings the acre; and having manured

manured it heartily with theſe ſhells, in about 3 years it turned to ſo good an account, that he had 15 ſhillings the acre profered to take the leaſe out of his hands. I know that manuring land with ſhells, thoſe of oyſters in particular, is no novelty: I mention this with regret, as an inſtance of what poor hands, both as to landlords as well as tenants, agriculture, an extenſive branch of Natural Knowledge, is generally thrown into; which both requires and deſerves the cloſe attention of a philoſophical mind.

A Paper omitted.

A ſummary of ſome late obſervations upon the *Generation*, *Compoſition*, and *Decompoſition* of *Animal* and *Vegetable Subſtances*; in a letter to *M. Folkes*, Eſq; P. R. S. by Mr *Turbeville Needham*, F. R. S. N°. 470. p. 615.

The End of the Second Part.

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